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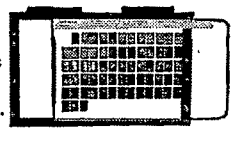
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SOCIAL DETERMINISM AND RATIONALITY AS BASES OF PARTY IDENTIFICATION*

ARTHUR S. GOLDBERG
University of Rochester

In *The Responsible Electorate,* V. O. Key urged upon us "the perverse and unorthodox argument . . . that voters are not fools."[1] He challenged the notion that the voting act is the deterministic resultant of psychological and sociological vectors. He believed that the evidence supported the view of the voter as a reasonably rational fellow. The present article offers a corollary to Key's "unorthodox argument." It suggests that certain sociological determinants, secifically group norms regarding party identification, may, upon examination, prove to be rational guides to action. For the voter who is a reasonably rational fellow, it will be argued, these group norms may seem rather sensible.

I. THE CONCEPT OF RATIONALITY

Before proceeding to the analysis of data, some discussion of the notion of rationality seems in order. The usage subscribed to in the present analysis derives from contemporary game theory.[2] Put most simply, being rational in a decision situation consists in examining the alternatives with which one is confronted, estimating and evaluating the likely consequences of each, and selecting that alternative which yields the most attractive set of expectations. Formally, this process entails making calculations of the following type as a basis for the decision:

$$E(Va_1) = P(o_1|a_1)V(o_1) + P(o_2|a_1)V(o_2) + P(o_3|a_1)V(o_3)$$
$$E(Va_2) = P(o_1|a_2)V(o_1) + P(o_2|a_2)V(o_2) + P(o_3|a_2)V(o_3)$$

where:

$E(Va_i)$ = expected value of alternative i.
$P(o_j|a_i)$ = probability of outcome j given that alternative i is chosen.
$V(o_j)$ = value of outcome j to the decision maker.

Having made such calculations, one then chooses the alternative with the greatest expected value. All individuals are assumed to act so as to *try* to maximize expected value. In this sense, all are equally rational. However, there is room for error in these calculations. Individuals may, therefore, vary in their effective rationality.

There are several sources of error. The relevant probabilities are not necessarily known, and may have to be estimated; the quality and cost of information thus affect the accuracy of the calculations. The calculations themselves are not cost free, being far more costly for some than for others. However, one component of these calculations is not admissible within the theory as a source of error, i.e., the values assigned to outcomes. The placing by an actor of a very high value for him upon having a Catholic for President, or upon obtaining an outcome which will aggravate his brother-in-law, or upon thwarting some target group cannot be adjudged within the theory. Therefore, in the analysis

* Much of the development and analysis in this article is a byproduct of efforts to develop a dynamic model of voting behavior. These efforts were funded by the National Science Foundation during the summers of 1966 and 1967. The body of data used in the present analysis was provided by the Survey Research Center at the University of Michigan. It is comprised of the 1956 election study (#417) plus occasional items from the 1958 and 1960 waves of the same panel.

[1] V. O. Key, Jr. *The Responsible Electorate* (Cambridge, Massachusetts: The Belknap Press of Harvard University Press, 1966), p. 7.

[2] See R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley and Sons, Inc., 1957), Chapter II.

which follows, terms such as "more rational" and "less rational" refer in all cases to effective rationality, i.e., to the accuracy of the expected value calculations. No set of values is being judged against some exogenous criterion of "rational correctness."

However, the analysis is heavily dependent upon an assumption about effective rationality. Specifically, the assumption is made that, *ceteris paribus*, effective rationality, i.e., the accuracy of one's expected value calculations, increases as a function of education. Education is assumed to lower information costs, e.g., by improving one's ability to read and comprehend such information sources as *The New York Times*, and thus to improve the accuracy of one's estimates of relevant probabilities. Education is also assumed to act upon innate intelligence so as to develop it toward its full potential. Thus, errors in the performance of the calculations ought to be reduced. In brief, education is assumed to facilitate accurate perceptions of means-ends relationships and to facilitate appreciation of the import of such relationships for one's own goals.[3]

Of late, there has been emerging from the voting literature a model of the voter as rational maximizer of expected utility.[4] The analysis of empirical evidence in the context of this model is still quite rare, but by no means non-existent. Current intellectual investments in the model, and the very nature of its logical qualities suggest that it will increasingly become a central concept in the discipline.[5] In its application, the political scientist is, of course, primarily interested in the substance of the expected value calculations. One is particularly concerned with ascertaining which factors are heavily weighted in the calculations. Are they policy outputs and promises as in the Downs model,[6] non-policy based anxieties as in some psychological findings,[7] desires for symbolic gratifications,[8] or what? What is the distribution of dominant factors across the electorate? Key was persuaded that policy counted heavily. (In fact, he seemed willing to regard behavior as rational only where this was the case—a usage different from that in the present analysis.) The present article, while not directly addressed to this question, is substantially related to it. The question at issue here is whether group political norms are seen as effective means to their desired ends by those best equipped to make the judgment.

II. A CONFLICT OF MODELS?

One of the most powerful determinants of voting behavior isolated to date appears to be the self-identification of the voter with a particular party. Empirical findings to this effect are legion.[9] Moreover, there is a substantial empirically-based literature, which indicates that an individual develops a party identification quite early in life, during childhood, in fact. This identification is well-established before the individual has any knowledge of either the policy history of the party with which he identifies, or of the implications of that history for his own

[3] One may regard education, from this point of view, as a capital investment oriented toward long-run reduction of information and calculation costs. For these notions on rationality and education, I am indebted to Peter C. Ordeshook, from whose work on a general rationality model of electoral behavior they are derived.

[4] The seminal work here has been Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957). For critical appraisals see: Donald E. Stokes, "Spatial Models of Party Competition," this *Review*, 57 (1963), 368–377; and Philip E. Converse, "The Problem of Party-Distances in Models of Voting Change," in M. Kent Jennings and L. Harmon Zeigler (eds.), *The Electoral Process* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1966), Chapter IX.

[5] See, for example: James M. Buchanan and Gordon M. Tullock, *The Calculus of Consent* (Ann Arbor: The University of Michigan Press, 1962); Otto A. Davis and Melvin Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," *Mathematical Applications in Political Science, II.* Arnold Foundation Monographs XVI. (Dallas, Texas: Arnold Foundation, Southern Methodist University, 1966), 175–208; and William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," this *Review*, 62 (March, 1968), 25–42.

[6] Downs, *op. cit.*

[7] For example, in Eugene Burdick and Arthur J. Brodbeck (eds.), *American Voting Behavior* (Glencoe, Illinois: The Free Press, 1959), see Arthur J. Brodbeck, "The Problem of Irrationality and Neuroticism Underlying Political Choice," Franz Alexander, "Emotional Factors in Voting Behavior," and Richard E. Renneker, "Some Psychodynamic Aspects of Voting Behavior."

[8] See Murray Edelman, *The Symbolic Uses of Politics* (Urbana, Illinois: University of Illinois Press, 1964).

[9] See, for example: Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (The University of Chicago Press, 1954), Chapters IX–X; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, "Party Loyalty and the Likelihood of Deviating Elections," *Journal of Politics*, 24 (1962), 689–702.

interests. Essentially, the acquisition process appears to consist in imitative and affective conditioning to the parental partisan preference.[10] Once acquired the identification proves to be remarkably stable, with some 70–80% of those voters who know their paternal party identifications reporting the same identifications for themselves.[11]

How well does the notion of the rational voter comport with the evidence cited which suggests that a party identification, established in childhood and seemingly quite resistant to change, is a major determinant of voting behavior? Having no wish to build straw men, let me immediately point out that there is not necessarily an incompatibility here. If one admits the possibility that the imitatively and affectively conditioned party identification learned in childhood might prove a rationally sound guide to action when examined in adulthood, a possible basis for reconciliation is available. This is the possibility which will be explored in the present analysis.

While the evidence to be examined sheds light on the question at hand, some of that evidence was initially analyzed for a different purpose. An appreciation of the meaning of the evidence requires that a somewhat circuitous route be taken in examining it. Some forebearance is therefore sought from the reader in the initial stages of the analysis. In return, assurance is given that the initial data presented, while somewhat removed from the main question, are themselves of substantive import, and lead to a sharp focus upon the main question, namely, rationality and social determinism as bases of party identification.

III. BACKGROUND OF THE INVESTIGATION

The original topic of inquiry was defection from parental party identification. In seeking to account for this relatively infrequent phenomenon, some unanticipated findings were encountered. After a bit of retroductively oriented contemplation, it became possible to "make sense" out of these findings. Quite simply, they made sense if the norms proffered by the parent were substantively rational for the self-interest of the offspring, i.e., increased his effective rationality. Since an explanation can be found for almost any set of findings, the discovery of an explanation was not, in itself, taken as proof of the validity of the explanation. Instead, several deductions were made, based upon the foundations of this explanation. Upon investigation, these deductions were found to be supported by the data, and, as a result, the reader is burdened with the present article. At the outset one must dwell a bit upon the original inquiry in order to appreciate the nature of the unanticipated findings.

Since the object of that inquiry—defection from parental party preference—occurs rather infrequently, some care was used in conceptualizing the independent variables in order that they might more readily be operationalized in productive ways. One of the major variables initially under consideration was social mobility. The usual reasoning applied: it was assumed that such mobility put the individual into a new social environment, the norms of which might be different from those of his parents. The resultant pressures of his adult environment might produce a shift on his part toward the norms of the new environment, and thus an abandonment of the parental party preference. A bit of additional thought on this matter, however, revealed that the logically implicit variable was not necessarily social mobility, but a strain between the parental partisan norm and the norm of the adult social environs. Moreover, there was an additional path to such strain, beyond that provided by social mobility. Specifically, it is logically possible for such strain to develop when the parental norms are atypical in their social environs and the individual remains in those environs in adulthood. Thus, the individual whose father is an Irish, blue-collar worker, and a Republican in an urban Democratic neighborhood and who is himself a blue-collar worker in that neighborhood, may be under as much strain as he would have been had his father been a Democrat while he himself became a physician and moved to the suburbs. It was not too difficult to devise a technique for measuring this hypothetical strain, however caused.

The technique used was to develop an "expected value" of the respondent's being a Republican, based upon his sociological characteristics.[12] In order to do this the respondent's party

[10] See: Herbert H. Hyman, *Political Socialization* (Glencoe, Illinois: The Free Press, 1959), Chapters II–III; David Easton and Robert D. Hess, "The Child's Political World," *Midwest Journal of Politics,* 6 (1962), 229–246; and Fred I. Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965), Chapter IV. An overview of the socialization processes involved is provided in Robert E. Lane and David O. Sears, *Public Opinion* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1964).

[11] Cf. Campbell, *et. al., op. cit.* p. 147; see also Philip E. Converse and Georges Dupeux "Politicization of the Electorate in France and the United States," *Public Opinion Quarterly,* 26 (1962), p. 14.

[12] The reader should note, at this point, that in

identification (Coded Republican = 1, Democrat = 0) was regressed upon five of his sociological characteristics in a multiple regression analysis.[13] The expected value thus generated is effectively an estimate of the probability that an individual with a given set of sociological characteristics is a Republican. One can compare this value with the father's party identification (Coded Republican = 1, Democrat = 0) and see how close the respondent's probability of being a Republican is to his father's actual state of being one. The absolute difference is a measure of strain between partisan norms of the respondent's adult environment, and the partisan preference of his father.[14] This concept will be referred to as intergenerational strain. Recapitulating: intergenerational strain is operationalized as the absolute difference between the father's party identification (Coded Republican = 1, Democrat = 0) and the respondent's regression-generated probability of being a Republican, given his sociological characteristics.

Given this perhaps elaborate operational procedure, one may well ask, does intergenerational strain account for much defection from the parental norm? The data are presented in Table I and again in Figure 1. That the hypothesized relationship is borne out by the data is fairly obvious. Since the frequencies associated with the scores 0.0 and 0.1, as well as with the scores 0.8–1.0, are quite small, interest centers on the scores between and inclusive of 0.2 and 0.7. Here the hypothesized relationship does hold, although the relationship is "S" shaped rather than linear. Whether much of the defection is thereby explained is another question. As against the standard of 100% of the variance, clearly not much is accounted for. Even using an asymmetric measure of association, Goodman and Kruskal's tau, designed to take advantage of the expectation that there is more information in the independent variable about the dependent variable than vice versa, errors in assignment on the dependent variable are deduced only by about 5.5%.[15] On the other hand, as against other single variable accomplishments with regard to this dependent variable, 5.5% is not trivially small.[16] This finding was, in itself, neither dramatic nor surprising. However, it was in the process of exploring the origins of intergenerational strain that a rather surprising finding did emerge, which in turn proved to be an opening wedge into the inquiry with which the present article is concerned.

In the interest of neatness, if nothing else, it

---

the operation to be described, statistical averages of a national sample were used as representations of felt environmental norms. This dubious procedure will be discussed at a later point in the analysis (see pp. 10, below). Particular sociological characteristics were chosen with a view toward reducing the discrepancies between such averages and felt norms and with a concern for minimizing multicollinearities. The specific characteristics utilized were: religion, self-rating on class, size of community, region (South, non-South), and race (white, non-white).

[13] In all cases, nominal data were converted to dummy variables and treated as prescribed in Daniel B. Suits, "The Use of Dummy Variables in Regression Equations," *Journal of the American Statistical Association*, 52 (1957), 548–551.

[14] The reader's attention is called to the fact that paternal partisan preference is ascertained on the basis of respondent's recall. Work by M. Kent Jennings and Richard G. Niemi suggests something of the magnitude of the error thus induced. From the point of view of correlation, Niemi finds a Kendall's tau of 0.60 between party identification claimed by the respondent's father and that ascribed to the father by the respondent. See Richard G. Niemi, *A Methodological Study of Political Socialization in the Family* (unpublished doctoral dissertation, University of Michigan, 1967), pp. 113–115, and 128. A further analysis of this data by this writer with the cooperation of Professor Niemi indicates that 78.4% of the students correctly identify their fathers in the broad categories of Democrat, Independent, Republican. The 21.6% of incorrect identifications are about evenly split between incorrect assignment with regard to the category of Independent (10.6%) and assignment to incorrect partisanship (11.0%).

Insofar as there is any systematic bias in the errors, it seems to operate in a manner not seriously debilitating to the present analysis. Respondents appear to be biased toward perceiving their parents as having a partisanship which is "normal" for people of like social characteristics. Thus where parents are identified as having a sociologically deviant identification, the ascribed identification is apt to be correct. This is rather important as such identification proves of central concern in the present analysis. See Niemi, *op. cit.*, pp. 126–128.

[15] See Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill Book Company, Inc., 1960), pp. 232–234.

[16] About the best that has been done along these lines with a single variable is the 7.29% of the variance in level of voter stability explained by family reinforcement, in Herbert McClosky and Harold E. Dahlgren, "Primary Group Influence on Party Loyalty," this REVIEW, 53 (1959), 774.

TABLE I. DEFECTION AS A FUNCTION OF INTERGENERATIONAL STRAIN[a]

	Score on Strain											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	4	1	22	51	67	46	51	17	2	—	1	262
Loyalty	7	11	109	234	135	73	55	25	2	—	2	653
Total	11	12	131	285	202	119	106	42	4	0	3	915
% Defecting $\tau_b = 5.5\%$	36.4	8.3	16.8	17.9	33.2	38.7	48.0	40.5	50.0	—	33.3	28.6

[a] $p < .05$ in this and other Tables, unless otherwise indicated.

seemed desirable to see how much of strain derived from social mobility on the part of the respondent, and how much of it derived from parents who had party identifications which were sociologically atypical. It was not anticipated that these deviant parental identifications would account for much strain, but some method had to be devised for measuring this "deviance" in order to put the question to rest.

Fortunately a measure of such deviance was readily at hand. The residual in a regression analysis is theoretically a measure of the extent to which an element differs in its dependent variable value from the average of other elements having the identical values on the independent variable(s). Thus, a multiple regression analysis in which the respondent's father's sociological characteristics were the independent variables and in which the father's party identification was the dependent variable would yield

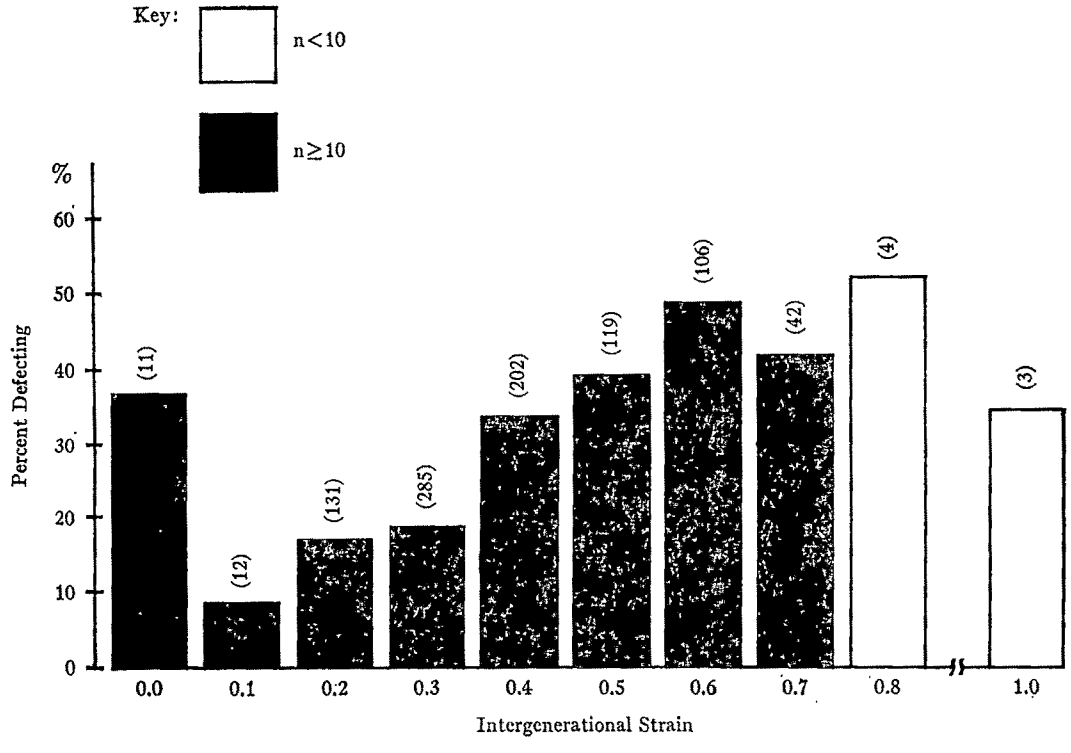


FIG. 1. Defection as a Function of Intergenerational Strain.

residuals well suited to the present task. Indeed, just such a regression was run, and the absolute values of the residuals thus generated were used to measure the extent of sociological deviance in paternal party identification.[17] It may be of some comfort to note that those parents who had the highest residuals, 0.847 and 0.718, were intuitively quite deviant, being on the one hand white, working class, Catholic, Northern, urban, Republicans, and on the other white, middle-class, Protestant, Northern, rural, Democrats. Similarly, those with the smallest residuals, 0.000 and 0.282, were what one would tend to regard as 'pure' types, being white, working class, Catholic, Southern, urban Democrats, and white, middle class, Protestant, Northern, rural Republicans. Withal, there are some caveats, beyond those concerned with statistical propriety, which should be observed. Statistical deviance from a sample norm and social deviance from a felt group norm are not identical, and yet the present study treats them as though they were. The most statistically deviant case in the national sample—the white, working-class, Catholic, Northern, urban Republican—can be found feeling not the least bit deviant and under no social pressures, in, for example, the city of New Haven, Connecticut.[18] Yet such errors of measurement, as they might affect the present study, would of necessity do so in a conservative way, biasing the outcomes *against* the findings which proved to be of interest. Thus, if such findings emerged, they did so in spite of and not because of the weakness in the operational measure.

TABLE II. INTERGENERATIONAL STRAIN AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION

	Score on Deviance[a]			
	High	Low	Total	% High Deviance
High Strain[a]	221	53	274	80.6
Low Strain	49	592	641	7.6
Total	270	645	915	29.6
% High Strain	81.9	8.2	29.9	
$\tau_b = 53.9\%$				

[a] High implies >0.500
Low implies <0.500
No respondent scored 0.500

How much, then, of intergenerational strain results from sociologically deviant paternal party identification? The r^2 between the two variables is 0.58. That is to say, 58% of the variance in one is explainable in terms of the other. Thus, there is a substantial covariation between the two. One can say more than this, however, after examining the pattern of the relationship. This pattern is revealed in Table II, wherein the variables are each dichotomized, with the attendant loss of information, and yet still yield a tau of 53.9%. Most particularly, one should note in this Table that 80.6% of high intergenerational strain seems to result from highly deviant paternal party identification.

Given so strong a relationship between intergenerational strain and sociological deviance in paternal party identification, it certainly seemed advisable to inquire into the effect of such deviance on the relationship between intergenerational strain and defection. This effect is set forth in Table III and is quite noteworthy. In the universe comprised solely of those whose paternal party identification is sociologically normal, i.e., where strain results only from social mobility, there is no significant relationship between differences in strain and rate of defection. In the universe comprised solely of those whose paternal party identification was sociologically deviant, there is a significant relationship between strain and defection. In the light of Table II one might be inclined to impute all of the high strain in this group to the deviant nature of the paternal party identification. However, this assumption is not altogether warranted. Therefore, some direct evidence was sought in the data regarding social mobility. Fortunately this was available in the form of information about intergenerational occupational status mobility.

[17] Clearly some qualms arise in using the residuals of a linear regression analysis in which the dependent variable was dichotomous. One worries about saturation effects, etc. [See Arthur S. Goldberg, *Econometric Theory* (New York: John Wiley and Sons, Inc., 1964), pp. 248–251.] In the actual event, visible saturation effects were infrequent (less than 1% of the cases had predicted values less than zero or greater than one). Moreover, the dichotomous nature of the dependent variable offered certain advantages. First, there is only one possible way to obtain each non-zero value of the residual. Second, there are only two points at which the residuals can be zero, namely, where the predicted and actual values are zero, and where the predicted and actual values are one. These polar points become, in effect, the "normal" points for their respective segments of the distribution (this division occurring at that value of X which yields a predicted value of y of 0.5). Thus, by comparing any two cases on the basis of the absolute values of their residuals, one can say which is closer to his respective "normal" point. It is on this basis that sociological deviance in paternal party identification was measured.

[18] See Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), Chapter V, especially Table 5.4, p. 60.

TABLE III. DEFECTION AS A FUNCTION OF INTERGENERATIONAL STRAIN CONTROLLING FOR DEVIATION OF PATERNAL PARTY IDENTIFICATION

(a) Given normal paternal party identification:

	Score on Strain High	Low	
Defection	14	130	144
Loyalty	39	462	501
	53	592	645
% Defecting	26.4	22.0	22.4

$p > 0.05$
$\tau_b = 0.2\%$

(b) Given deviant paternal party identification:

	Score on Strain High	Low	
Defection	103	15	118
Loyalty	118	34	152
	221	49	270
% Defecting	46.6	30.6	43.7

$p < 0.05$
$\tau_b = 1.7\%$

Therefore, the "high strain" category of Table III (b) was examined in terms of the relationship between occupational status mobility and defection. As can be seen in Table IV, there does not appear to be any significant relationship. Thus, on the bases of Tables III and IV, social mobility does not appear to be a significant factor in explaining defection from paternal party identification.

TABLE IV. DEFECTION AS A FUNCTION OF INTERGENERATIONAL OCCUPATIONAL STATUS MOBILITY GIVEN DEVIANT PATERNAL PARTY IDENTIFICATION AND HIGH INTERGENERATIONAL STRAIN[a]

	Intergenerational Occupational Status Mobility		
	Upward	None	Downward
% Defecting	48.0	43.4	49.4
(N)	(77)	(67)	(75)

[a] Two of the two hundred and twenty-one respondents in this category provided no information on occupational status mobility. The total N here is therefore 219 as against the 221 in the relevant column of Table III (b).

This finding concurs with that of Campbell, *et al.*, in their slightly different analysis of the same body of data.[19] However, it does seem to fly in the face of the current lore in the discipline. For example, Henry W. Riecken, in a survey of the literature dealing with primary groups and party affiliation, argues that:

> Upward mobility . . . signals not only alienation from parental values, but the likelihood that political preference will shift from Democratic to Republican since the upward mobile person will be moving into predominantly Republican affiliations in primary groups.[20]

He goes on to cite the works of Berelson, Lazarsfeld, and McPhee, of Maccoby, *et al.*, and of McClosky and Dahlgren in evidence.[21]

Surely such apparently conflicting conclusions about the role of social mobility in accounting for intergenerational changes in party identification deserve serious treatment. However, since the material to this point is very much by way of preface, such as extended digression here seems ill advised. Therefore a consideration of the works cited, as they bear upon this problem, is provided as an appendix to the present article. At this point, let it suffice to note that the assertion made in the present study, i.e., that social mobility is not a significant factor in explaining defection, is compatible with the assertion that social mobility, when it occurs, tends to produce defection. All that is required for this compatibility is that either there be relatively little social mobility or that the tendency toward defection induced by social mobility be rather weak. In the actual event, it appears that both conditions exist. For a fuller discussion, the reader is referred to the Appendix.

Presented with the substantial relationship between intergenerational strain and deviance in

[19] Campbell, *et. al., op. cit.*, pp. 458–459.

[20] Henry W. Riecken, "Primary Groups and Political Party Choice," in Burdick and Brodbeck, *op. cit.*, p. 167.

[21] *Ibid.*, pp. 167–168. The relevant works are: Berelson, Lazarsfeld, and McPhee, *op. cit.*; Eleanor E. Maccoby, Richard E. Matthews, and Anton S. Morton, "Youth and Political Change," *Public Opinion Quarterly*, 18 (195?), 23–29; McClosky and Dahlgren, *op. cit.* An intimation of the findings of the present study may be seen in Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in David E. Apter (ed.), *Ideology and Discontent* (London: Collier-Macmillan Limited, The Free Press of Glencoe, 1964), pp. 231–233.

TABLE V. DEFECTION AS A FUNCTION OF DEVIANCE OF PATERNAL PARTY IDENTIFICATION

	Score on Deviance											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	6	17	48	48	25	36	40	35	5	1	1	262
Loyalty	3	67	198	158	75	39	58	40	14	1	0	653
Total	9	84	246	206	100	75	98	75	19	2	1	915
% Defection $\tau_b = 6.4\%$	66.7	20.2	19.5	23.3	25.0	48.0	40.6	46.6	26.2	50.0	100.0	28.6

paternal party identification, as well as with the vitiating effect of such deviance when used as a control on the original relationship between strain and defection, one was naturally curious as to the direct relationship between sociological deviance in paternal party identification and defection from paternal party identification on the part of the offspring. The data are presented in Table V and in Fig. 2. The relationship is somewhat stronger than the original one between intergenerational strain and defection ($\tau_b = 6.4\%$ in the present case as against 5.5% in the earlier case). However, interest in the relationship derives not from its magnitude which

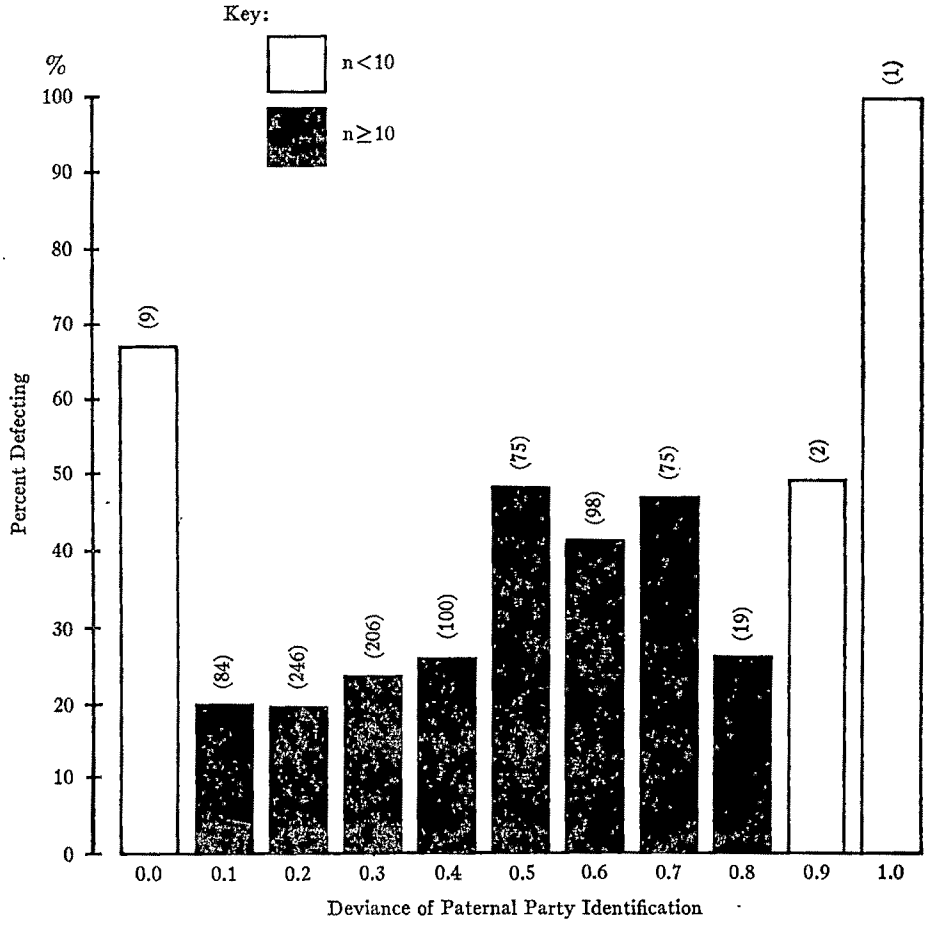


FIG. 2. Defection as a Function of Deviance of Paternal Party Identification.

is, after all, still quite modest, but from the fact that this phenomenon of sociologically deviant paternal party identification appears to be a more important source of intergenerational defection than does social mobility on the part of the offspring. Sociological deviance in paternal party identification thus assumes a heretofore unanticipated importance, and is perhaps deserving of serious recognition in future analyses.[22]

[22] At this point, it may be worth noting that this concept (sociological deviance) helps to clarify at least one peculiar finding in the existing literature. In *The American Voter,* Campbell, *et al.,* after noting that the direction of intergenerational social mobility, as measured by occupational status differences, did not seem to be associated with shifts toward the Republican or Democratic identification go on to note, on pp. 458–459, n. 15, that:

> Upward mobile people are slightly more likely to have shifted from Democrat to Republican identification, than those people whose status has moved downward, *but both types of changers* are more likely to have moved toward the Republican Party than away from it. [My italics.]

However, this appears to be a consequence of the operation of sociological deviance in paternal party identification, because when analysis is confined to those respondents whose fathers were normally identified, the findings are more in line with what one might traditionally have expected. Upward mobile offspring of Democrats are more apt to become Republicans than are downward mobile offspring of Democrats. Conversely, downward mobile offspring of Republicans are more apt to become Democrats than are upward mobile offspring of Republicans. Moreover, contrary to the SRC suggestion, the downward mobile progency of normal Republicans are more likely to become Democrats than are such progency of normal Democrats to become Republicans. Finally, where occupational status has not changed between generations, the Republicans have lost more than the Democrats—a finding compatible with the history of the Republican party over the past thirty years.

DEFECTION AS A FUNCTION OF OCCUPATIONAL STATUS MOBILITY: GIVEN NORMAL PATERNAL PARTY IDENTIFICATION

	Upward	None	Downward
Father Democrat			
% Defecting	26.4	12.3	18.0
N	(167)	(114)	(122)
Father Republican			
% Defecting	24.5	22.2	29.0
N	(94)	(72)	(76)

In the present analysis, this new variable, however intrinsically interesting, does not of itself shed light on the central problem of concern. This, the reader may recall, is the potential conflict between the rational voter model and the evidence for social determinism in the establishment of party identification. That sociological deviance in paternal party identification would produce defection among offspring who remained in the same social environs was anticipated under the general social determinism model: only the relatively high incidence of such cases was unanticipated. However, when a relatively routine control was applied to the relationship between deviant paternal party identification and defection, several findings emerged which appeared quite germane to the central concern of this inquiry.

IV. THE PRECIPITANT OF THE PRESENT INQUIRY

Pursuant to relatively standard operating procedure, respondent's education was applied as a control to the relationship between deviance in paternal party identification and defection from that identification on the part of the respondent. There was, at that point in the analysis, no genuine theoretical expectation as to the effect of this control. It did, however, produce a rather unexpected finding. The control revealed that the better educated respondents were more likely to defect from deviant paternal party identification than were the less well educated, and, conversely, the better educated respondents were less likely than the poorly educated to defect from normal paternal party identification.

As indicated earlier in Table V, errors in assignment on the dependent variable (defection from paternal party identification) are reduced by 6.4% by a knowledge of the extent to which paternal party identification was sociologically deviant.[23] When one controls for education (dichotomizing the sample at the median), the τ_b is increased to 11.3% in the universe of the better educated, whereas, in the universe of the poorly educated it is reduced to 3.4%. (These results may be seen in the data presented in Table VI, and in Fig. 3.) Education appears to sensitize one to the fact of deviance in paternal party identification where such deviance is or has been present. Moreover, where the father has been normally identified, education of the offspring enhances the likelihood of filial loyalty. Perhaps this may

[23] Strictly speaking, the reduction is calculated against a null model which is based upon random assignment in proportions prescribed by the distribution on the dependent variable. For clarification, the reader is again referred to Blalock, *loc. cit.*

TABLE VI. DEFECTION AS A FUNCTION OF DEVIANCE OF PATERNAL PARTY IDENTIFICATION, CONTROLLING FOR EDUCATION[a]

(a) Given high education level on the part of the respondent:

	Score on Deviance											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	3	12	27	22	13	19	24	20	1	—	1	142
Loyalty	1	42	125	100	45	16	28	16	5	—	0	378
Total	4	54	152	122	58	35	52	36	6	0	1	520
% Defecting $\tau_b = 11.3\%$	75.0	20.2	17.7	18.0	22.4	54.3	46.1	55.5	16.7	—	100.0	27.3

(b) Given low education level on the part of the respondent:

	Score on Deviance											
	0.0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	Total
Defection	3	5	21	26	12	17	16	15	4	1	—	120
Loyalty	2	25	73	58	30	23	30	24	9	1	—	275
Total	5	30	94	84	42	40	46	39	13	2	0	395
% Defecting $\tau_b = 3.4\%$	60.0	16.7	22.4	31.0	28.6	42.5	34.8	38.5	30.8	50.0	—	30.4

[a] Dichotomization of education level:
low = none through some high school; high = more than some high school

be more clearly seen in the percentages in Table VII, despite the loss of information entailed in dichotomization of the independent variable. Most likely to defect are the well educated offspring of fathers with sociologically deviant party identifications; the defection rate here being 50.0%. Least likely to defect (19.7%) are the well educated offspring of normally identified fathers. Among poorly educated respondents, those whose paternal party identifications were deviant are more likely to defect than are those whose paternal party identifications were normal (37.9% as against 26.2%). However, poorly educated respondents whose paternal party identifications were deviant, are less likely to defect than are well educated respondents with similar paternal antecedents (37.9% as against 50.0%). Finally, the poorly educated offspring of normally identified fathers are more likely to defect than the well educated offspring of normally identified fathers (26.2% as against 19.7%).

None of these differences in percent defecting are very likely to be due to chance. Of the four comparisons made, and on the basis of two-tailed tests, only one has a probability of

TABLE VII. DEFECTION AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION CONTROLLING FOR RESPONDENT'S LEVEL OF EDUCATION

Percent Defecting
($N = 915$)

		Education	
		High	Low
Paternal Party Identification[a]	Deviant	50.0 (130)	37.9 (140)
	Normal	19.7 (390)	26.2 (255)

[a] Deviant: abs value of residual > 0.5
(see above p. 9)
Normal: abs value of residual < 0.5

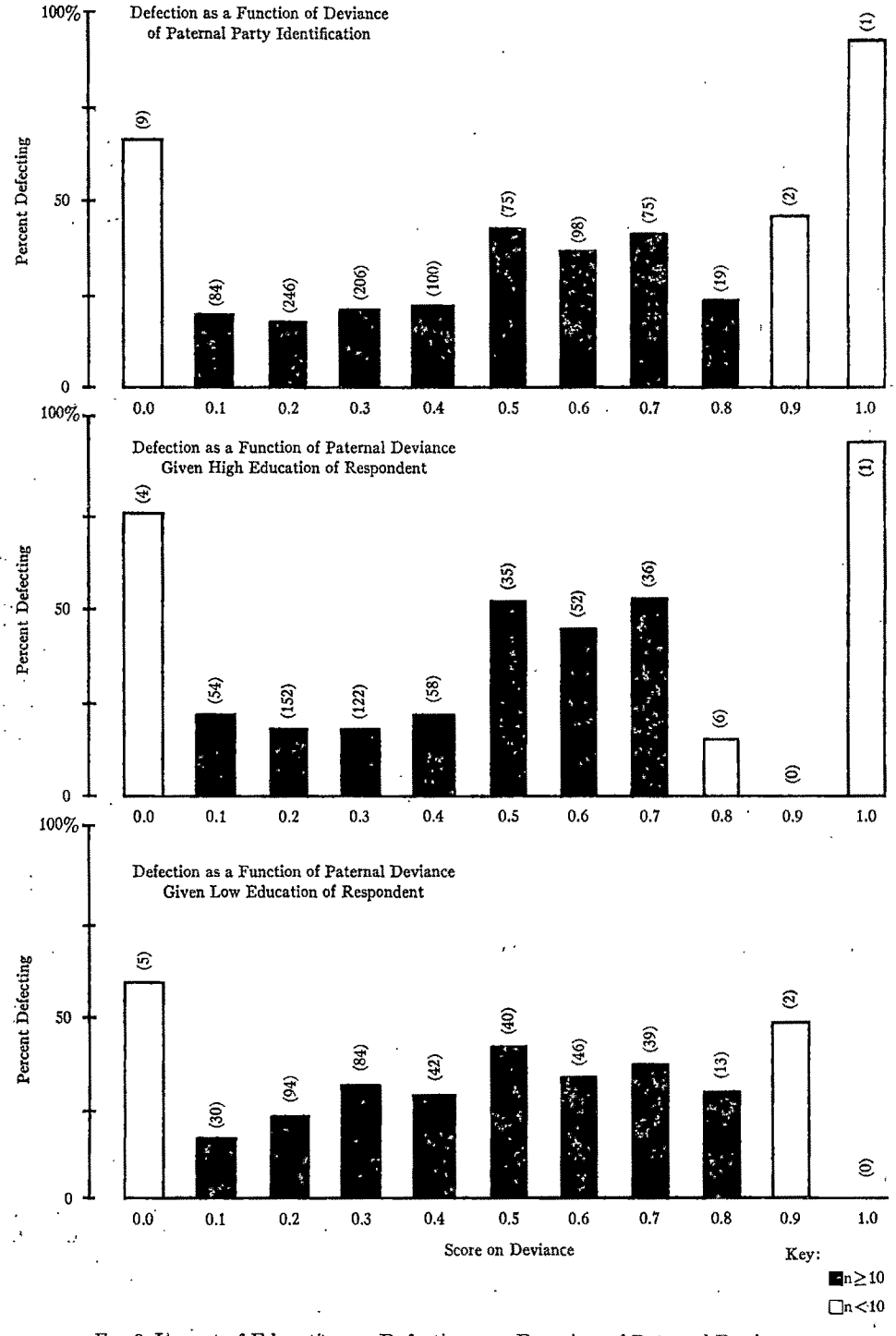


FIG. 3. Impact of Education on Defection as a Function of Paternal Deviance.

occurrence by chance greater than 0.05. The difference in question is that between the defection rates of poorly educated as against well educated respondents with normally identified fathers (26.2% as against 19.7%). Even here the probability of occurrence by chance is only 0.0525. These findings, then, seem to warrant serious attention.[24] Assuming for the moment that they are not a function of measurement error—a problem which will be discussed later—under what theoretical scheme do they make sense? Or put somewhat differently, under what set of assumptions would one expect these results?

V. EXPLANATIONS

There are at least two sets of assumptions under which one would expect these results. One of these, the "brainwashing" model, entails only one assumption, namely, that the educational system of the country "brainwashes" people into believing in their group norms. This model will not be treated seriously in the present study. It is mentioned because it is a logical alternative to the model which will be offered, and because none of the tests performed on the latter in this study are capable of discriminating between the two models. Thus, should any reader wish to dwell upon the matter to the extent of conceiving of a discriminating test, a service would be rendered.

[24] Lest the reader be apprehensive that the findings in Table VI reflect *only* the impact of education on offspring of sociologically deviant Democratic identifiers, through the ensuring that such offspring will be in a Republican environment, a further breakdown is presented below.

DEFECTION AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION, CONTROLLING FOR RESPONDENT'S LEVEL OF EDUCATION: BY PARTISANSHIP OF FATHER

	Percent Defecting			
	Father Democrat		Father Republican	
	High Edu.	Low Edu.	High Edu.	Low Edu.
Paternal Party Identification Deviant	56.8 (74)	38.7 (62)	41.1 (56)	37.2 (78)
Paternal Party Identification Normal	18.7 (235)	23.5 (170)	21.2 (155)	31.8 (85)

Two points are particularly worth noting in this breakdown. First, the pattern found in Table VII holds, regardless of paternal party preference. Second, normally identified Democratic parents also help to insure the political loyality of their children by furthering their educations.

The model which will be treated seriously, the "rationality" model, consists of the general utility maximization model plus three assumptions. These are: 1) that group-based norms regarding party identification produce effective rationality with regard to the goals of the group; 2) that the individual in the group shares the goals of the group; 3) that education increases rationality in the special senses of lowering information costs and developing innate intelligence toward its full potential.[25] Under this set of assumptions, one would expect the well educated respondents to be better able than the poorly educated respondents to accurately judge the effectiveness of the parental party identification as a guide for the implementation of their own goals. One would also expect, from this set of assumptions, that the sociologically normal identification would be more efficacious than deviant identifications for the accomplishment of the respondents' goals. The well educated being more readily able to see this than the poorly educated, one would expect defection rates in the following rank order, from highest to lowest: paternal party identification sociologically deviant and respondent well educated; paternal party identification deviant and respondent poorly educated; paternal party identification normal and respondent poorly educated; paternal party identification normal and respondent well educated. Thus, these assumptions lead to a set of expectations congruent with the empirical findings; i.e., they provide an explanation for the findings. Of course, an explanation's discovery is not to be taken as proof of its validity. Therefore, attention is shifted to testing, by way of empirical evidence, deductions derived from these explanatory assumptions.

VI. EDUCATION, DEFECTION AND RATIONALITY

Before proceeding with a description of these deductions, their testing, and the results thereof, a few words are very much in order regarding the direct impact of education in intergenerational defection. Insofar as there was, prior to the explanatory assumptions suggested above, any expectation with regard to this impact, it was that probability of defection would increase with respondent's level of education. This expectation was derived from the work of Maccoby, Mathews, and Morton who found that in Cambridge, Massachusetts, education was positively correlated with defection.[26] Upon application of Goodman and Kruskal's tau to their findings, one finds that a knowledge of ed-

[25] See n. 3, above.

[26] Maccoby, Matthews, and Morton, *op. cit.*

ucation yields a reduction in assignment errors on defection of 4.5%. Thus a similar relationship was expected in the present body of data. However, this did not prove to be the case; a very weak relationship was found, which did not prove significant at the .05 level. The data are presented in Table VIII.

The discrepancy between the findings of the Maccoby *et al.* study and the present one may reflect differences in sample bases or in operational procedures. Direct comparisons between the two studies are not quite legitimate, since the operationalization of the dependent variable, defection, was accomplished in substantially different ways. This operation in the present study entails only the establishment of whether or not the respondent continues to identify himself with the party with which he believes his father to have associated himself. Maccoby *et al.*, on the other hand, have built an index which takes into account the respondent's party identification and current vote preference, and the relationship of these preferences to those of both of his parents.[27]

More important for present purposes than their empirical finding, is the rationale which Maccoby *et al.*, offer for it.

> Presumably, in the course of obtaining an education, the young person is exposed to a wider variety of points of view and more information, so that he is able to make political choices on the basis of "rational" considerations rather than simply on the basis of family tradition (or revolt against family tradition).[28]

Although there is some ambiguity in their explanation—exposure to more points of view and more information could lead to increased defection, even without any increase in the rationality of political choice—the idea that education might produce more rational choice is certainly intriguing, as well as compatible with the assumptions offered in the present study. However, to argue that an increase in rationality—however one wishes to define it—must produce an increase in defection from the traditional primary group party identification is to argue that the latter is irrational. Doubtless this is not the authors' intent. A more tenable line of argument would be that an increase in rationality pro-

[27] The problem raised by such an index is that it is difficult, probably impossible, to ascertain which permutation of the components is reflected in a particular instance of a given value of the index, yet it is by no means clear that a given value of the index has the same theoretical relationship to the independent variables, regardless of the permutation of components upon which it is based.

[28] Maccoby, Matthews, and Morton, *op. cit.*, p. 38.

TABLE VIII. DEFECTION AS A FUNCTION OF EDUCATIONAL LEVEL[a]

	0	1	2	3	4	5	6	7	8	—	Total
Defection	0	29	42	49	5	58	31	28	19	1	262
Loyalty	2	74	90	109	14	146	63	86	66	3	653
Total	2	103	132	158	19	204	94	114	85	4	915
% Defecting	0.0	28.2	31.8	31.0	26.3	28.4	33.0	24.6	22.4	25.0	28.6

$p > .05$
$\tau_b = 1.0\%$

[a] The code for educational level is as follows:
0 = none
1 = some grade school
2 = completed grade school
3 = some high school
4 = some high school & other non-college
5 = completed high school
6 = completed high school & other non-college
7 = some college
8 = completed college
– = not ascertained

Note: Those for whom educational level was not ascertained constitute a violation of the rule that only those about whom full information was available—albeit an inadvertent violation. Where these four enter into calculations, and that is only when education is dichotomized, they are treated as though well educated.

duces an increase in defection from traditional identifications in some direct proportion to the extent to which those identifications are irrational. Such a line of argument is readily supported by the present empirical findings relating defection to sociological deviance in party identification with respondent's education as a catalyst in the relationship.

VII. TESTING THE EXPLANATION

It now remains to inquire into the tests of the "rationality" model. The reader may recall that in addition to the general utility maximization model, this approach assumes that group-based norms regarding party identification are rational and that education facilitates utility maximization by reducing information and calculation costs. If education has this catalytic effect, sensitizing the respondent to the irrationality in paternal deviance from group norms of party identification, it ought to be the case that the proportion of defection which occurs during the high school and college years is greater among respondents whose fathers have sociologically deviant party identifications than where those identifications are normal. If sociological norms are rational, and if education sensitizes one to this rationality, then it ought to be possible to predict party identification from sociological characteristics with greater accuracy for well educated than for poorly educated people. Finally, if the better educated, to a greater extent than the poorly educated, do use sociological referents in maintenance of their party identifications, this should be reflected in differences between these two groups in the strength of association between sociological characteristics and partisan attitudes. It was against these deductions that the "rationality" model was tested by way of their goodness of fit with available evidence.

In order to test the first deduction, i.e., that the high school and college years account for a greater proportion of the defection which occurs where paternal party identification was deviant than of the defection which occurs when such identification was normal, it was necessary to establish the respondent's age at the time of defection. This had to be done in an indirect way and a certain problem of inference developed. The body of data being analyzed permitted one to establish the respondent's recollection of his father's party affiliation, the respondent's own present party identification, whether or not the respondent had ever identified himself with a party other than the one with which he was currently identified, if so, when he had assumed his current identification, and his age. From these data one could establish whether the respondent had a party identification different from that of his father. If so, the respondent was classified as a defector and became of interest for present purposes. His age at defection was established on the bases of the year in which he had assumed his current identification and of his age in 1956. However, more than half of those who did not identify with the same party as that with which they identified their fathers, claimed *never* to have identified themselves as supporters of a party other than their current one.

The work of Greenstein and others suggests a substantial recall error here, perhaps because of respondents' confining themselves to their adult years in tracing their histories of party identification.[29] The data are presented in Table IX. The grouping of the data in the Table into five-year blocs was done for convenience, but the reader should bear in mind that those classified as defecting between birth and the end of the fourth year are actually those who claim never to have identified with the father's party.

An examination of the table reveals that the high school and college years (ages 15–24) account for 15/118ths or 12.7% of the defection which occurs where paternal party identification was sociologically deviant, and 16/144ths or 11.1% of the defection where such identification was normal. Although the difference in proportions is in the expected direction, the difference is so small as to have a very high probability of occurrence by chance. At this point, those respondents who claim *never* to have identified with the paternal party become a source of concern. If one assumes (and it is a rather arbi-

[29] See n. 10, above.

TABLE IX. AGE AT TIME OF DEFECTION AS A FUNCTION OF DEVIANCE IN PATERNAL PARTY IDENTIFICATION

		Frequency Distributions														
		Age at Defection														
		0–4	5–9	10–14	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65+	Total
Paternal Party Identification	Deviant	72	0	8	8	7	8	3	4	0	6	0	2	0	0	118
	Normal	68	2	13	9	8	18	4	11	6	1	3	0	0	1	144

trary assumption) that these respondents mean *never during their adult years*, then one may possibly infer that their defections occurred between the ages of 15 and 24. If these people are included in the previous analysis, then the high school and college years account for 87/118ths or 73.7% of the defection which occurs where paternal party identification was sociologically deviant as against 85/144ths or 59.0% of the defection where such identification was normal. This difference in proportions is not only significant at the .05 level, but has a probability of occurrence by chance of only 0.0132. However, despite this, the rather arbitrary assumption made with regard to those responding "never," requires one to conclude that at best the deduction being tested was not refuted by the data. To say that it was supported by the data would be unwarranted.

The next deduction to be tested was somewhat more straightforward, and the results were also rather more straightforward. If the sociological norms of party identification are rational, and if education enables one more readily to perceive this rationality, then one ought to be able to predict party identification from sociological characteristics with greater accuracy for the better educated half of the sample than for the less well educated half. With the sample thus divided at the median level of education into two groups, a linear multiple regression was run in each group in which respondent's party identification was regressed upon his religion, self-identification as working or middle class, community size (farm, town, metropolis), region (South, non-South), and race. In the better educated half of the sample, these characteristics accounted for 26% of the variance in party identification, whereas among the less well educated half of the sample only 7% of the variance was accounted for by these characteristics. Thus one can safely say that the data here support the deduction, in that sociological characteristics account for very nearly four times more of the variance in party identification among the well educated than in the case among the poorly educated. This was the strongest single finding supportive of the explanatory model, but the third deducation remained to be tested in order to shed light upon the cognitive processes involved. While sociological characteristics appeared to be rational guides to action, was this reflected in the attitudes of the respondents? Inquiry was made into this via the relationship between partisan attitudes and sociological characteristics.

Ascertaining the extent to which partisan attitudes reflect sociological characteristics was something of a problem. Had each of these been a single variable, the problem would have been relatively simple, lending itself to analysis by way of correlations. In the actual event, partisan attitudes consisted of four variables, and sociological characteristics were five separate variables.[30] The approach taken was to ask to what extent predictions of party identification based upon sociological characteristics lead to the same set of expectations as those generated by basing such predictions upon partisan attitudes. This approach was implemented by using the two sets of predicted values generated by regressions of party identification upon sociological characteristics and partisan attitudes respectively. Each of these sets of predicted values was then dichotomized at 0.500. Within each set, those with predicted values greater than 0.500 were classified as "expected Republicans", and those with predicted values less than 0.500 were classified as "expected Democrats." These two sets of expectations regarding party identification—one set based upon sociological characteristics and the other set based upon partisan attitudes—were then cross-tabulated and the Goodman and Kruskal tau measure of association used to establish how much information expectations based upon sociological characteristics contained about expectations based upon partisan attitudes.[31] This entire procedure was implemented separately in each of the two educational groups. The results are presented in Table X.

As can be seen in Table X, there is very nearly five times as much information about the dependent variable in the independent variable among the well-educated as among the poorly-educated. (Goodman and Kruskal's tau is 11.0% in the former instance and no more than 2.4% in the latter instance.) Thus, the findings support the idea that sociological considerations are a more salient referent for the formation of partisan attitudes among the well-educated than among the poorly-educated, and hence, they support the explanatory model being offered. However, given that even among the well-educated, the tau is only 11%, it seems clear that sociological considerations, as herein measured, are only a part of the total set of rational considerations.

VIII. SUMMARY, CAVEATS, AND CONCLUSIONS

The inquiry began with the discovery of two rather unanticipated relationships in the data.

[30] For operational procedures see: Arthur S. Goldberg, "Discerning a Causal Pattern Among Data on Voting Behavior," this REVIEW, 60 (December, 1966), 922.

[31] Cf. Blalock, *loc. cit.*

TABLE X. PREDICTION OF PARTY IDENTIFICATION FROM SOCIOLOGICAL CHARACTERISTICS AND FROM PARTISAN ATTITUDES

Given: High Education

		Prediction Based Upon Sociological Characteristics		
		Republican	Democratic	Total
Prediction Based Upon Partisan Attitudes	Rep.	122	63	185
	Dem.	63	143	206
	Total	185	206	391

$\tau_b = 11.0\%$

Given: Low Education

		Prediction Based Upon Sociological Characteristics		
		Republican	Democratic	Total
Prediction Based Upon Partisan Attitudes	Rep.	53	48	101
	Dem.	60	119	179
	Total	113	167	280

$\tau_b = 2.4\%$

First, the evidence suggested that sociological deviance in party identification on the part of the respondent's father was an important source of intergenerational defection in party identification. Moreover, and of central importance here, it was discovered that this relationship was stronger among the better educated than among the less well-educated respondents. An explanation was generated for the second finding. This explanation consisted in the utility maximization model plus three assumptions: 1) that group-based norms regarding party identification produce effective rationality with regard to the goals of the group; 2) that the individual in the group shares the goals of the group; 3) that education increases rationality in the special sense of reducing the costs of information and of calculation. This approach explained the findings in that, under the assumption of the approach, one would expect the well educated to perceive more accurately than the poorly educated the expected utility associated with the paternal party identification. One would expect, under these assumptions, that sociologically deviant party identification would have a lower expected utility than sociologically normal party identification. The findings, then, become anticipated results under the assumptions of the explanatory model.

The explanatory model was then tested by testing deductions made from it against available data. Three deductions were made and tested: first, that the proportion of defection occurring during the high school and college years would be greater where paternal party identification had been sociologically deviant than where it had been sociologically normal; second, that one would be able to predict party identification on the basis of sociological characteristics with greater accuracy for the better educated than for the less well educated halves of the population; finally, that attitudes toward each of the two parties would reflect sociological characteristics to a greater extent among the well educated than among the poorly educated halves of the population. As tested, all of these deductions were sustained by the data. However, the strengths of association and differences in proportions involved in the testing, while always in the predicted direction, and while in all cases, save one, statistically significant at the 0.05 level, are nonetheless small enough to preclude seriously considering the inquiry closed.

As the matter now stands, the present analysis seems to suggest that there is a rational component to sociological norms of party identification, but that there are most probably other rational considerations as well. In the absence of such additional considerations, the validity of the rationality model can only be defended by attributing *all* of the unexplained variance in each of the tests to measure error. Both an extension of the model to include other rational considerations, and a reduction in measurement error seem apt prescriptions for further research in this area.

At least three modifications should be made in the model. First, and perhaps most obviously, the model should take account of the fact that there are many routes—other than education—to the reduction of information costs. Occupational roles, leisure time activity, location in formal and informal communication networks, all produce an effect upon information costs beyond that accomplished by education. Second, the model should be made dynamic. That is, it should be able to take into account the fact that policy outputs through time entail shifts in the relative advantages or deprivations to various components of the electorate, and that rational group norms would change accordingly. Finally, as a model of human dynamics, provision ought to be made for the time lag involved in learning that an old norm is no longer functional. One might go further and recognize that there are very likely two time lags, during the first of which opinion leaders learn that the old norm is no longer desirable and during the second of which the new prescription offered by

these leaders are being considered and tested by their followers.[32] Indeed, one might even wish to distinguish between opinion leaders and followers by the factors which receive heavy utility weightings in their respective calculations. For opinion leaders, policy considerations might be taken to outweigh considerations of interpersonal affect. For followers, on the other hand, affective consideration might receive priority. Certainly, other modification may prove warranted, but these ought to provide at least a beginning.

Serious efforts are also required in the matter of reducing measurement error. In the earlier discussion of measuring "deviance" in paternal party identification it was noted that the present study treats a statistical norm as a felt norm and that these two are not necessarily equivalent. There are at least two solutions to the reduction of this aspect of measurement error. For purposes of testing the model, recourse ought to be had once again to community, or perhaps even subcommunity studies. Here one could not only more accurately measure norms, but could trace information flows much more readily than with national samples. On the other hand, for inferring national parameters, the simulation approach of Pool, Abelson, and Popkin seems more appropriate.[33] However, the problems of the simulation approach are formidable, most particularly if one recognizes the dynamics operative in the system being approximated by the simulation. It may be recalled that the authors of the Simulmatics Project used "common sense, social science theory, and similar guides" and thus expressed their "best judgment on how different types of voters would respond to a campaign focused on the religious issue."[34] These judgments were assessed aginst the extent to which they led to correct results when applied in the equations of the model.[35] However, as the authors themselves point out, the inference of national parameters in a complex model entails many difficulties, most especially if the dependent variable is dichotomous. No single trial can genuinely test the estimated parameters, even if the model is assumed correct, in as much as many permutations of the parameters would produce the same results.[36] Many deductions, entailing other than dichotomous outcomes, must be tested in many trials, before one can reasonably begin to believe the estimates of the parameters. However, if these parameters themselves are in flux, the problem becomes substantially more complicated, to say the least.

An even more formidable source of measurement error inheres in the identification of salient group norms. As social scientists, investigators are almost certain to be middle class people. As political scientists, they are apt to be atypically preoccupied with policy outputs. How likely, then, are such investigators to design a questionnaire which captures the salient events used by a slum dweller in his rational calculus. Even with regard to middle class norms, there are substantial areas of ignorance. What statistically based inference can one make from the 1956 election data about middle class values which will explain the Eisenhower victory and the Republican Congressional defeat in that election?[37] The problem of mapping and weighting preferences in the electorate is still rather far from solved. Thus, one can expect tests of the model to be contaminated by measurement error for some time to come.

Despite all these qualms, the present study strongly suggests that there is a rational component to party identifications rooted in group norms. At least in retrospect, the notion is an obvious one. If one considers the origins of group norms, one expects that at their inception they were rational means to group ends. It may not then be surprising to find that they continue to be rational guides to action, even when learned on primarily affective bases. It might not be untoward, then, for research to be addressed to a world of rational voters seeking to reduce information costs by following group norms, which have in the past seemed effective, with due allowance made in the analytical model for the learning and adaptative processes incumbent upon the voter in attempting to cope with a changing political environment, as well as with his sundry immediate and perhaps more pressing responsibilities.

[32] Cf. William N. McPhee, *Formal Theories of Mass Behavior* (London: Collier-Macmillan Ltd., The Free Press of Glencoe, 1963), Chapter II. See also William N. McPhee and William A. Glaser, *Public Opinion and Congressional Elections* (New York: The Free Press of Glencoe, 1962), esp. pp. 139–151.

[33] Ithiel de Sola Pool, Robert P. Abelson, and Samuel L. Popkin, *Candidates, Issues, and Strategies* (Cambridge, Massachusetts: The M.I.T. Press, 1964).

[34] *Ibid.*, pp. 54–56.

[35] *Ibid.*, pp. 56–57.

[36] *Ibid.*, pp. 102–106.

[37] Despite Key's laudable efforts in this direction in *The Responsible Electorate* (see for example, pp. 79–89 of that work), I suggest that the question remains open.

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APPENDIX:

An Evaluation of Some of the Literature on Social Mobility and Intergenerational Defection

Let us examine the literature which Riecken cites regarding the impact of social mobility on partisanship. The first piece of evidence cited in this connection is *Voting*, which presents a chart entitled "The Child's Correspondence with the Father's Vote is Affected by His Relative Status in the World."[38] Table A1, below, is abstracted from that chart (See Fig. A1) and presents information on the impact of upward mobility on the offspring of Democratic fathers. The tau produced by this array is 0.5% and $p > .05$. Thus, as presented in *Voting*, the impact of social mobility on voting behavior is at best very small, as illustrated in Table A1.

Maccoby, Matthew, and Morton in their article "Youth and Political Change" are also interested in the relationship between social mobility and partisan preference.[39] However, their central concern is with the phenomenon of adaptation to group norms. Their data do seem to support the contention that upward mobile persons tend to manifest the norms of the social strata into which they are moving to a greater extent than downward mobile persons.[40] Unfortunately, their data are presented in such a way that it is impossible to know the relevant rates of intergenerational defection, or, for that matter, the extent to which the percentage differentials represent any kind of defection at all. The relevant table is reproduced as Table A2, below.

The most directly relevant and, in many ways, the most substantial set of findings supporting social mobility as a major factor in intergenerational defection is that produced by McClosky and Dahlgren.[41] The relevant table from their article is presented as Table A3, below. Among those whose family preference is Republican, the relationship between respondent's social mobility and his party preference yields a tau of 13.5% ($p < .01$); among those whose family preference is Democratic, the same relationship yields a tau of 8.6% ($p < .05$). In comparison with the strengths of association developed in the present study, these are sufficiently large to warrant a reconsideration of the assertion that social mobility is not a significant factor in accounting for intergenerational defection. Yet, a careful examination of Table A3 reveals that McClosky and Dahlgren are utilizing the term social mobility in a rather unusual way. They do not use the trichotomy "upward-none-downward," but rather a trichotomy "favorable-neutral-antagonistic" to describe their categories of social mobility. An examination of the marginals strongly suggests that these are *not* equivalents. For example, if where the parental preference was Democratic, "antagonistic" is taken to mean upward mobility, then only seventeen percent of the offspring of Democratic parents have been upwardly mobile. Moreover, if "antagonistic" is taken to mean upward mobility in this case, then "favorable" ought to mean downward mobility; on this basis 45% of the offspring of Democratic parents have been downwardly mobile. Since recent studies suggest that upward mobility in the United States runs at about 40% and downward mobility at about 25%,[42] and since in the present study, about 40% of the offspring of Democratic fathers are upwardly mobile, as compared with about 30% of such offspring who are downwardly mobile, it seems most unlikely that "favorable-antagonistic" is in any sense a direct analogue to "upward-downward" or "downward-upward." Rather, it would seem that McClosky and Dahlgren have included in the "favorable" and/or "neutral" categories a

[38] Berelson, Lazarsfeld, and McPhee, *op. cit.*, p. 91.

[39] Maccoby, Matthews, and Morton, *op. cit.*

[40] *Ibid.*, pp. 33–36.

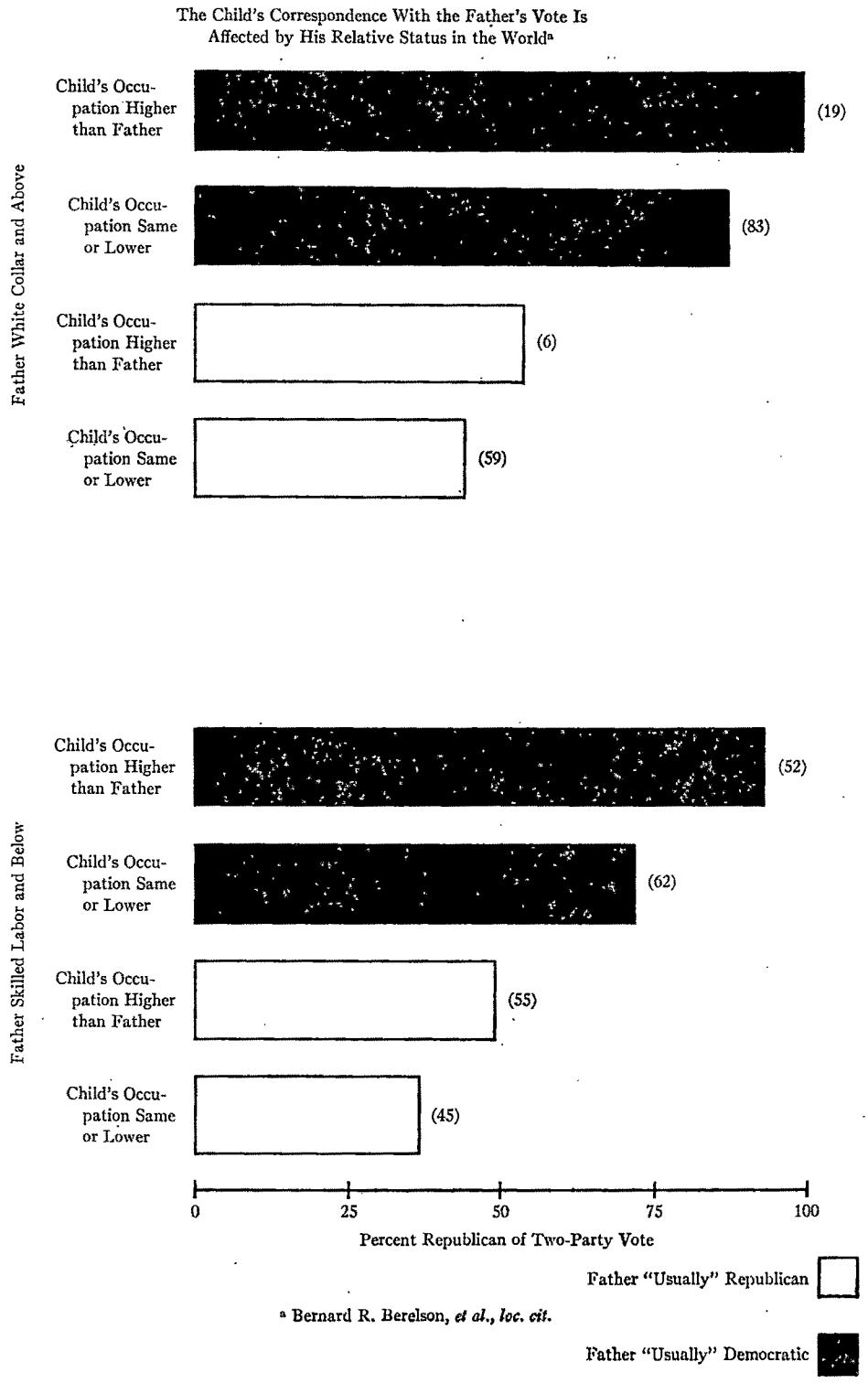
TABLE A1. INTERGENERATIONAL DEFECTION FROM DEMOCRATIC PATERNAL VOTING PREFERENCE AS A FUNCTION OF OCCUPATIONAL MOBILITY[a]

	Child's Occupation		
	Higher than Father	Same as or Lower than Father	Total
Defection	28	41	69
Loyalty	33	63	96
Total	61	104	165
% Defecting $p > .05$ $\tau_b = 0.5\%$	45.9%	39.4%	41.8%

[a] Abstracted from Chart xxxix in Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), p. 91.

[41] McClosky and Dahlgren, *op. cit.*

[42] Elton F. Jackson and Harry J. Crockett, Jr., "Occupational Mobility in the United States: A Point Estimate and Trend Comparison," *American Sociological Review*, 29 (1964), 5–15.



[a] Bernard R. Berelson, *et al.*, *loc. cit.*

FIG. A1.

TABLE A2. SOCIAL MOBILITY RELATED TO CHOICE OF PARTY AND CANDIDATE*[a]

Party preference	Upward Mobile	Non-mobile, in Class Where Upward Mobiles Originated**	Non-mobile, in Class to Which Upward Mobiles Moved**
Republican definitely	18%	8%	21%
Republican leanings	5	—	—
Independent	9	18	28
Democratic leanings	12	1	8
Democratic definitely	56	73	43
	100%	100%	100%
Number of cases	82	73	73
Candidate choice			
Eisenhower	48%	28%	37%
Stevenson	52	72	63
	100%	100%	100%
Number of cases	82	72	72

TABLE A2 (continued)

Party preference	Downward Mobile	Non-Mobile in Class Where Downward Mobiles Originated**	Non-Mobile, in Class to Which Downward Mobiles Moved**
Republican definitely	28%	21%	11%
Republican leanings	3	—	—
Independent	8	28	17
Democratic leanings	4	8	3
Democratic definitely	57	43	69
	100%	100%	100%
Number of cases	75	73	73
Candidate choice			
Eisenhower	46%	38%	27%
Stevenson	54	62	73
	100%	100%	100%
Number of cases	74	72	72

* Students are excluded from this table, since their mobility is still undetermined.

** For purposes of comparison, the non-mobile cases have been weighted to cancel out differences between them and the mobiles in socio-economic status. That is, for the first comparison, the non-mobiles have been weighted so as to have the same SES distribution as the mobile people at their point of origin. For the second comparison, the non-mobiles have been weighted so as to have the same distribution as the mobiles have in their destination class.

[a] Eleanor E. Maccoby, Richard E. Mathews, and Anton A. Morton, "Youth and Political Change," *Public Opinion Quarterly*, 18 (1954–55), 35.

TABLE A3. RELATION BETWEEN VOTER STABILITY, FAMILY PREFERENCES, AND SOCIAL DISTANCE[a]

Family Preference is Republican				Family Preference is Democratic			
	Respondent's Social Mobility is:				Respondent's Social Mobility is:		
Respondent's Preference is	Favorable (Rep)	Neutral	Antagonistic (Dem)	Respondent's Preference is	Favorable (Dem)	Neutral	Antagonistic (Rep)
	(%'s Down)				(%'s Down)		
Republican	84.6	60.8	42.1	Democratic	84.3	62.9	50.0
Democratic	15.4	39.1	57.9	Republican	15.5	37.0	50.0
Sample Size	39	23	19	Sample size	32	27	12
Mean Stability Score*	4.90	3.91	3.32		5.25	4.04	3.50

* The Mean Stability score is computed on a 6 point scale in which 6.0 represents highest party stability in the same direction as the family preference, and 1.0 represents highest stability in the direction of the party opposed by the family.

[a] Herbert McClosky and Harold E. Dahlgren, "Primary Group Influence on Party Loyalty," this REVIEW, 53 (1959), 769.

good deal of upward mobility for offspring of Democrats, and have done the same regarding downward mobility for offspring of Republicans. Thus, for them the upward mobility from unskilled to semi-skilled labor would not be regarded as unfavorable to the persistance of Democratic party preference. This is not necessarily ill advised, in fact it seems quite sound for their purposes—the measurement of support provided by current milieu for parental party preference. However, in such an approach mobility itself is not the explanatory factor. It is neither the fact of mobility itself nor the size of the increment which is important, but rather the thresholds which are crossed. Thus, a high rate of upward social mobility would not necessarily produce a steady flow to the Republican party. Such a flow could be expected only if the mobility occurred across certain critical points, e.g., skilled labor to administrative occupational categories. The McClosky and Dahlgren findings are sound, but their invocation in support of the contentention that "upward mobility signals. . . the likelihood that political preference will shift from Democratic to Republican" is a bit misleading, for it is true only with regard to such mobility across certain thresholds. Thus, the literature which Riecken cites does not preclude the acceptance of the findings on social mobility and defection presented in the present study.

CONCEPT FORMATION IN NORMATIVE AND EMPIRICAL STUDIES: TOWARD RECONCILIATION IN POLITICAL THEORY

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Despite the fact that the study of politics has become increasingly empirical, quantitative and "behavioral" in recent years, and despite the apparently increasing tendency to feel that whatever meaningful debate ever existed between the behavioralists and the anti-behavioralists has ended, should end, or at least has become irrelevant since a more sophisticated and empirically productive behavioralism now predominates in virtually all fields of the discipline,[1] the methodological debate continues, diminished perhaps in quantity but not in intensity.[2]

This essay is based on the assumption that the antagonists concerned with the methodological issues raised by the "new science of politics" have but rarely focused precisely on the arguments raised by their opponents. A second motivating assumption is that nothing constructive, conciliatory or conducive to the integration of the discipline can be done "until the issues have been squarely confronted on the basic and general plane of philosophy. . . ."[3] A thorough analysis of all of the meaningful issues involved can only be a task of long-range proportions. But in the hope of bringing about some degree of communication, if not reconciliation, it is my intention in this essay to bring one of these issues into sharper focus, to show that almost despite themselves, some of the critics and proponents of the "new science of politics" have addressed themselves to the problem of concept formation, and that despite their proclaimed differences are talking at cross-purposes about a similar problem. Indeed, it will be seen that the conflict between the "traditionalists" and the "behavioralists" is utterly dependent—in the area of concept formation—upon an outmoded positivistic interpretation of behavioral science and a misguided reaction on the part of some political theorists to that obsolete conception.

Concept formation as a general problem in philosophy, however, is extremely complex inasmuch as it covers questions of definition, classification, comparison, measurement, and empirical interpretation. To achieve our immediate and ironic purpose, however, it is desirable to restrict the analysis to the questions of definition and especially of the criteria of classification to be used in developing the basic concepts of political science, concepts of the attributes of political phenomena. Not only is it necessary to resolve this question prior to engaging in the questions of comparison and measurement, but it is primarily on this most basic level that it is possible to see most clearly the nature of the misunderstandings held by the antagonists on both sides of the controversy about a behavioral science of politics. Once we understand how we develop criteria of identity and classification, problems such as comparison become relatively simple on the conceptual level and permit us to concentrate our energies upon the application of our concepts and techniques to the data we wish to compare or measure.

I. THE "NEW SCIENCE OF POLITICS": OVERT BEHAVIOR AS A PRINCIPLE OF CONCEPT FORMATION

There are two extreme positions that have been taken regarding the nature of concept formation during the recent history of the social sciences. One is the view characteristic of the earlier forms of positivist and empiricist philosophy according to which "any scientific statement, however abstract, could be transformed,

[1] Robert A. Dahl, "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest," this REVIEW, 55 (December, 1961), 763–772.

[2] See Herbert J. Storing (ed.), *Essays on the Scientific Study of Politics* (New York: Holt, Rinehart and Winston, 1962); John H. Schaar and Sheldon S. Wolin, "Essays on the Scientific Study of Politics: A Critique," this REVIEW, 57 (March, 1963), 125–150; Herbert J. Storing, Leo Strauss, *et al*, "Replies to Schaar and Wolin," pp. 151–160; James C. Charlesworth (ed.), *The Limits of Behavioralism in Political Science* (Philadelphia: American Academy of Political and Social Science, 1962); W. G. Runciman, *Social Science and Political Theory* (Cambridge: University Press, 1963), especially pp. 156–175; James C. Charlesworth (ed.), *A Design for Political Science: Scope, Objectives, and Methods* (Philadelphia: American Academy of Political and Social Science, 1966); Eugene J. Meehan, *Contemporary Political Thought: A Critical Study* (Homewood, Ill.: Dorsey Press, 1967), especially pp. 1–110.

[3] Schaar and Wolin, *op. cit.*, p. 125.

by virtue of the definitions of its constituent terms, into an equivalent statement couched exclusively in observation terms: Science would really deal solely with observables."[4] As John Stuart Mill suggested, "the backward state of the moral sciences can only be remedied by applying to them the methods of physical science, duly extended and generalized."[5] For the purpose of scientific inquiry, at least, the behavioral political scientist in the eyes of his critics treats as non-existent all psychic phenomena, especially the valuing activities of human beings, and his concepts must be technical concepts, as remote from everyday, common sense constructs as possible.[6]

Arthur Bentley, of course, is the most emphatic exponent in the recent history of political thought of the idea that the study of politics should confine its inquiry to "observables" or, as Herbert Hochberg puts it, to "overt behavioral patterns."[7] "My epistemological point of view," Bentley claimed, "is admittedly naive, as naive, I hope as the point of view of the physical sciences; I nowhere lay any stress on the difference between the conscious and the unconscious. . . ."[8] This statement, it should be observed, does not refer to the *techniques* of the physical sciences, or to the *logic* of scientific procedure in general. Bentley is stressing a particular way of looking at phenomena, a way of constructing concepts of the objects of inquiry in the science of politics. In a very general sense, Bentley's claim of epistomological identity with the physical sciences is quite justified. The "viewpoint" of the physical scientist is indeed not that of introspection, and color and sound have no meaning in his explanations except in terms of wave lengths, which are theoretical constructs interpreted in terms of the observed behavior of material entities.

Theoretical constructs interpreted in terms of the externally observed behavior of humans is the similar goal of Bentley's political science. For Bentley, ideas and feelings, like color and sound, may be immediately experienced by subjects but cannot be the "raw material" of a science of politics. What, he asks, are "the practical realities" for which these feeling and idea factors stand?

> . . . we readily see that they stand for certain regularities . . . in activity stated as individual conduct. For instance, if a child is kind to its cat it is apt to be kind to its dog. We indicate the tendency by calling the child kind-hearted.
>
> However, while these feelings and ideas put themselves forth to be definite dependable things, experience proves that they only conform roughly to the actual activity that can be observed . . . from the standpoint of the feelings we can observe nothing more than unreliable, poorly defined tendencies of activity to correspond to them. Kindness to cat or dog is not accompanied by kindness to snake or mosquito.
>
> When we get to the application of these feeling and idea elements to social interpretation, our difficulties become greatly increased . . . The words commonly used for feelings and ideas break down, and for the social researcher ideas and feelings must be made so definite in meaning . . . . that they fit the facts of the case like shadows. . . .[9]

The concepts used by the social scientist must correspond not to the meanings imputed to

[4] C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952), p. 24. The original statement of this is to be found in J. B. Watson, *Behaviorism,* Revised Edition (Chicago: University of Chicago Press, 1930). Other contemporary philosophers of science describe early, or radical, behaviorism in this manner also. See Felix Kaufmann, *Methodology of the Social Sciences* (London: Oxford University Press, 1944), pp. 148, 151; Ernest Nagel, *The Structure of Science* (London: Routledge and Kegan Paul, 1961), pp. 476–477.

[5] Ernest Nagel, *John Stuart Mill's Philosophy of Scientific Method* (New York: Hafner Publishing Co., 1950), p. 307.

[6] This view of what is implied by the behavioral position is appropriate in regard to J. B. Watson's work, but not with respect to the work of contemporary behavioral scientists. And even exponents of radical behaviorism, while rejecting introspection as a research method, did not deny the existence of conscious mental states. See Ernest Nagel, *The Structure of Science, op. cit.,* pp. 477, 480.

This obsolete and incorrect view of what is implied by the behavioral position is dominant among critics of the "new science of politics." See Leo Strauss, "An Epilogue," in Herbert J. Storing, *op. cit.,* pp. 310–320; Leo Weinstein on Bentley, pp. 154, 174, 188, 189, 220; Herbert J. Storing on Simon, pp. 112, 123–132. See also Henry S. Kariel, "Political Science in the United States: Reflections on One of Its Trends," *Political Studies,* 4 (June, 1956), 114–115.

[7] Herbert Hochberg, "Physicalism, Behaviorism, and Phenomena," *Philosophy of Science,* 26 (April, 1959), 93.

[8] Arthur F. Bentley, *The Process of Government* (Evanston: Principia Press, 1935), p. 177. n. 1.

[9] *Ibid,* pp. 167–168.

their own conduct by the subjects themselves but rather must be constructs referring and corresponding to the external activity of the subjects as observed by the social scientist. "If we can get our social life stated in terms of activity, and of nothing else . . . we have at least reached a foundation upon which a coherent system of measurements can be built up."[10] This is the position also of Charles E. Merriam, G. E. G. Catlin and other early proponents of behavioral political studies.[11] Essentially the same position has been expressed in more recent polemic. David Apter, for example, argues for the development of "research theory" that is based upon concepts not derived from common sense. "Much of what seems to be a maze of jargon in social science," he points out, "is a direct result of the effort to cut through so-called common sense terms which, upon intensive probing, prove themselves to be of limited usefulness."[12] Apter feels that the epistomology that is implicit in most political research has been that of "historicism," or the attempt to discover the ideas of an historical period and the "structures" in which they are expressed.[13] The historical approach, unfortunately, has placed undue emphasis on the uniqueness of an age and therefore also on the idea that the age can best be understood through participation in "the social processes on one's environment." Contrary to this approach, Apter contends that "the only way in which questions can be phrased in terms of their maximum significance is to articulate the body of research theory to which the problem is relevant."[14] This, in turn, can best be done by developing abstract models of interaction.[15] The variables in these models, of course, will be only those which can be scientifically studied and which are significant in terms of the scientific problems which interest the scholar or the profession, rather than in terms of the political problems important in political life.

[10] *Ibid,* pp. 169, 184–185.

[11] Charles E. Merriam, *New Aspects of Politics* (Chicago: University of Chicago Press, 1925), p. 86; "Progress in Political Research," this REVIEW, 20 (Feb., 1926), p. 7. As G. E. G. Catlin put it, "Politics must view social phenomena externally, considering without moral *parti pris* the appraisals of value and conventional estimates which it observes that men, in given places or times, in fact entertain. . . . The appreciation of the meaning of the phenomena it must leave to art and religion, which alone are competent to give us 'empathically' a sympathetic understanding of the personal experience": *The Science and Method of Politics* (New York: Alfred A. Knopf, 1927), p. 106. For others see the discussion in Bernard Crick, *The American Science of Politics* (London: Routledge and Kegan, Paul, 1959), p. 129.

[12] David Apter, "Theory and the Study of Politics," this REVIEW, 51 (Sept., 1957), p. 755.

[13] *Ibid,* p. 755.

[14] *Ibid,* p. 755.

II. THE "OLD POLITICAL SCIENCE": VERSTEHEN AS AN INSTRUMENT OF EXPLANATION

At the opposite extreme from the radical behaviorists, who maintain the thesis that concepts in the science of politics are to be developed by the observer on the basis of his perception of the overt actions of his subjects, are those who have argued in favor of what is usually called the "*verstehen* operation."[16] Until recently there was little question but that the term *verstehen* referred to an explanatory technique, the "method" of empathy, or to the intuitive imputation of motives to human agents other than oneself on the basis of one's understanding of one's own motivation. It is the attempt to "understand" social behavior by imputing "springs of action" to the actors involved in it by means of an imaginative identification of the social scientist with the participants in the social situation under analysis. The "external" observation of social behavior, of "patterns of action," from this point of view is at best extremely limited in applicability, and at worst utterly fruitless. The political and social sciences, it is argued, should not, or cannot, be molded along the methodological lines of the physical sciences.

As Theodore Abel describes the "*verstehen* operation"[17] it involves the assumption that the social scientist has within the universe of his experience feelings or emotions which he has subjectively correlated with certain behavior he himself manifested in the past. The social observer also associates, often but not always quite logically, certain groups of value concepts with others—for example, a value concept asserting the value of life might be associated with other attitudes such as, but not necessarily, pacifism,

[15] *Ibid,* p. 757.

[16] The most succinct analysis of this is given by T. Abel, "The Operation Called *Verstehen*," *American Journal of Sociology,* 54 (Nov. 1948), 211–218. See also Maurice Natanson, "A Study in Philosophy and the Social Sciences," *Social Research,* 25 (Summer, 1958), 158–172. In political science the best-known proponent of *verstehen* as a necessary instrument of explanation in social science is Leo Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1953), pp. 50–59.

[17] Abel, *op. cit.,* pp. 211–218.

toleration, and opportunities for individual fulfillment. Then these feelings which are meaningfully associated with each other in the individual are used by him as an investigator to "explain" the behavior of others.

In political science, Leo Strauss, expanding in large part upon the methodological position of Max Weber, agrees with him that human conduct cannot be understood ". . . without being aware of the standard of judgment that is inherent in the situation and accepted as a matter of course by the actors themselves. . . ."[18] He draws a strong distinction between what he calls "the new science of politics" and the old, or Aristotelian, political science on the basis of several criteria, including the notion that ". . . Aristotelian political science views political things in the perspective of the citizen [prior to judging or evaluating those various perspectives on the basis of ". . . a more comprehensive and a clearer grasp of man's natural ends and their natural order. . . ."] whereas the new political science ". . . looks at political things from without, in the perspective of the neutral observer . . . ."[19] Weber himself, of course, believed that *verstehende* was a method that enabled the social scientist to avoid injecting value judgments into his studies by accepting the self-interpretation of his subjects in the construction of ideal types to be utilized in explanation. For Strauss, however, this aspect of Weber's approach creates a corrupt methodology for the social scientist, for he then falls "victim to every deception and every self-deception of the people one is studying. . . ."[20] This is also the position taken by Eric Voegelin, David G. Smith, Charner Perry, Harry V. Jaffa, Henry S. Kariel, to a lesser extent Bernard Crick and, with the emphasis upon methodological as opposed to historical relativity, by Robert M. Barry.[21] From this viewpoint, then, objectivity is impossible, and Weber's own work is an illustration of the fact that it cannot be achieved. In trying to avoid value judgments, Weber merely developed his analyses on the basis of the conceptual framework of his own society, his own historical milieu. Such historical relativity vitiated Weber's attempt to achieve a value-free social science. *Verstehen,* coupled with the desire to avoid the corruption of historical relativity, forces the social scientist, *qua* scientist, to make value judgments of a moral kind.

This position is also part of the broader methodological stance taken by Robert M. MacIver, who has argued that the verification of hypotheses in the scientific study of politics takes place on two levels. First of all, statistical analyses and related techniques are to be employed to determine whether and to what extent the phenomena under investigation are associated with other phenomena. All hypotheses must pass those tests before they can be considered for further study. Those hypotheses which meet this requirement must then be analyzed on the second level, where the political scientist tries to discover whether the association or correlation is *meaningful.* At this level the evidence is

> . . . the depositions, avowals, confessions, justifications, and other testimonies offered by agents, participants, or witnesses, professedly or ostensibly giving their own answers . . . to the question of causation . . . We must here essay the task of projecting ourselves by sympathetic reconstruction into the situation as it is assessed by others. . . .[22]

An even more radical criticism of the idea of a strictly behavioral science of politics that is based on the notion that common-sense criteria determine the concepts of the study of politics has been put forward by Charner Perry. It is more radical because it assumes that the application of the scientific method to a given subject matter

[18] Leo Strauss, *op. cit.,* p. 54.

[19] Herbert J. Storing, *op. cit.,* p. 310. See also Leo Strauss, "The Social Science of Max Weber," *Measure,* 2 (1951), 211–214; *The Political Philosophy of Hobbes* (Oxford: The Clarendon Press, 1936), pp. 129–170.

[20] Leo Strauss, *Natural Right and History,* p. 55.

[21] Eric Voegelin, *The New Science of Politics* (Chicago: University of Chicago Press, 1952), pp. 20-27; David G. Smith, "Political Science and Political Theory," this REVIEW, 51 (Sept., 1957), p. 739; Charner Perry, "The Semantics of Political Science," this REVIEW, 44 (June, 1950), p. 398; Harry V. Jaffa, "The Case Against Political Theory," unpublished paper presented to the annual meeting of the American Political Science Association, September 12, 1959, p. 2; Bernard Crick, *op. cit.,* pp. 120–122, 126, 128–129; Robert M. Barry, "Review of Alfred Schutz, *Collected Papers I: The Problem of Social Reality,*" in *International Philosophical Quarterly,* 3 (September, 1963), p. 649.

[22] R. M. MacIver, *Social Causation* (Boston: Ginn and Co., 1942) pp. 390–391. The dependence of the political theorist on "self-awareness and introspection" is asserted also by Norman Jacobson, "The Unity of Political Theory: Science, Morals, and Politics," in Roland Young (ed.), *Approaches to the Study of Politics* (Evanston: Northwestern University Press, 1958), p. 120, and David G. Smith, *op. cit.,* p. 742.

. . . involves a transformation of the subject matter, a substitution of one point of view and set of terms for another . . . briefly, for the common sense universe of discourse . . . there must be substituted a system of terms, concepts or categories which abstract from human purposes, needs and ideals, in Veblen's terms, concepts which disclose a 'colorless impersonal sequence of cause and effect.'[23]

Because in social behavior language is not merely an instrument, external to the behavior, "but is rather the essential stuff or structure of the behavior, . . ."[24] common sense is teleological and anthropomorphic whereas science, though it originates in common sense, ". . . in its explicit theory . . . uses terms and distinctions which are detached from reference to human desires, needs, or activities."[25] This difference, according to Perry, makes it impossible to apprehend social behavior in the categories necessary in science. While there have been many attempts to describe social action in terms appropriate to natural science, especially in recent years by the behavioralists, Perry considers them all ". . . abortive because the distinctions and preconceptions of common sense have quickly reimposed themselves even when mutilated by a strained terminology."[26] Taking the position that he does, it is necessary to stress the fact that Perry is not in his own eyes arguing for the subjective character of political inquiry. He often speaks of the "knowledge" and "facts" we have of political behavior, and toward the end of his analysis he specifically raises the question, How do the terms of social science have objective reference? He answers, however, in terms similar to those of the other critics of "the new science of politics," for he believes that to describe behavior adequately one must do so in terms of norms, in terms of the purposive activity involved, in terms of the common-sense perspective of the agent.[27]

III. VERSTEHEN AS A PRINCIPLE OF CONCEPT FORMATION

Among philosophers the meaning of the term *verstehen* is still at issue.[28] That interpretation which treats it as referring to *how* one knows or explains the motives of others, that is, to intuition as an instrument of scientific explanation, has been increasingly called in question. *Verstehen,* to philosophers who hold this view, such as Nagel and Hempel, is considered to be a *subjective process* because they hold that understanding the motives of another man's action by means of this procedure depends upon the private, uncontrollable, and unverifiable intuition of the observer. Recent criticism of this view emphasizes an aspect of the concept *verstehen* that is most significant for the problem of concept formation. It also brings the concept closer to its usage as originally established by Max Weber.

Contrary to Nagel and Hempel, one critic asserts that "social scientists, such as Max Weber . . . call *Verstehen* subjective because its goal is to find out what the actor 'means' in his action, *in contrast to the meaning which this action has for the actor's partner or a neutral observer.*"[29] *Verstehen* literally means simply

[23] Charner Perry, "The Semantics of Political Science," *op. cit.,* p. 398.

[24] *Ibid,* p. 395.

[25] *Ibid,* p. 397.

[26] *Ibid,* p. 399.

[27] *Ibid,* p. 406.

[28] See, for example, Thelma Lavine, "Note to Naturalists on the Human Spirit," *Journal of Philosophy,* 50 (February, 1953), 145–154; Ernest Nagel's criticism of Lavine's position in his article, "On the Method of Verstehen as the Sole Method of Philosophy," *Journal of Philosophy,* 50 (1953), 154–157; and Lavine's reply, "What is the Method of Naturalism?" *Journal of Philosophy,* 50 (1953), 157–161. A year later Carl Cohen joined the debate with an article on "Naturalism and the Method of *Verstehen,*" *Journal of Philosophy,* 51 (April 1, 1954), 220–225.

[29] Alfred Schutz, "Concept and Theory Formation in the Social Sciences," *Journal of Philosophy,* 51 (April, 29, 1954), p. 265. Underlining added. Schutz further observes that the controversy about *verstehen* ". . . suffers from the failure to distinguish clearly between *Verstehen* (1) as the experiential form of common sense knowledge of human affairs, (2) as an epistemological problem, and (3) as a method peculiar to the social sciences." Type (2) represents the way the critics of *verstehen* deal with its meaning, that is, as a problem in *how we know, in general,* viz., by intuition or by observation. Type (3), on the other hand, assumes that the logic implicit in the physical and the social sciences is the same, and treats the term as referring to a problem in method peculiar to the social sciences. That is to say, granted that the logic of all the sciences is the same, nevertheless, social reality, the "object of inquiry" in the social sciences (interaction, intersubjectivity, language) requires a *method of observation* (and therefore a type of concept formation) suitable for those objects, a method that *observes actions as they are meant by the actors* and not that observes actions with meanings imposed by the observer.

See Max Weber, *The Theory of Social and Economic Action* (Glencoe: Free Press, 1957), pp. 9, 18, 22, 90, but esp. p. 88; Talcott Parsons, *The*

to understand or to comprehend, and thus both usages are plausible. The interpretation of the term by Schutz, however, stresses the goal, or object, of understanding—*the actor's meaning* —rather than *the process of understanding in the mind of the observer*.[30]

This revised interpretation of *verstehen*, by treating the "subjectively intended meaning" of an action as a problem in concept formation within the social sciences rather than a problem of how we know in general, by intuition or by observation, makes it possible for proponents of the necessity for taking the meaning of action into account to agree with the logical positivists about the nature of the *logic* of the social sciences. Schutz, for example, agrees with Nagel that *all* empirical knowledge depends upon discovery through processes of controlled inference, must be capable of being verified by anyone who will execute the proper experiment, and that any method that would require the scientific observer to select and interpret facts in terms of his private value system would never produce a scientific theory.[31] Nevertheless, he submits that to the best of his knowledge no ". . . social scientist of stature . . . ever advocated such a concept of subjectivity as that criticized by Professor Nagel. Most certainly this was not the position of Max Weber." And he goes on to assert that Nagel and Hempel

> . . . are prevented from grasping the point of vital concern to social scientists by their basic philosophy of sensationalistic empiricism or logical positivism, which identifies experience with sensory observation and which assumes that the only alternative to controllable and, therefore, objective sensory observation is that of subjective and, therefore, uncontrollable and unverifiable introspection.[32]

This identification of experience with sensory observation in general and of the experience of overt action in particular excludes a great many of the more significant dimensions of social reality from all possible inquiry, as Schutz makes clear by listing several examples:

a) Even an ideally refined behaviorism can only explain the behavior of the observed, and not of the observing, behaviorist. (This, he says, has been shown by George H. Mead in *Mind, Self and Society*.)

b) The same overt behavior (say, a tribal pageant as it can be captured by the movie-camera) may have an entirely different meaning to the performers of different tribes. What interests the scientist is precisely whether it *is* a war dance, a barter [trade], the reception of a friendly ambassador, or something else of this sort.

c) 'Negative actions,' that is, intentional refraining from acting, escapes sensory observation. (See Max Weber's *Theory of Social and Economic Organization*, p. 88.) Not to sell certain merchandise at a given price is doubtless as economic an action as to sell it.

d) As W. I. Thomas has shown in *Social Behavior and Personality* (E. H. Volkart, ed., 1951, p. 81), social reality contains elements of belief and conviction which are real because so defined by the participants and which escape sensory observation.

e) Finally, and this is deemed most important by Schutz, the postulate of sensory observation of overt human behavior takes as a model a particular and relatively small sector of the social world, namely, situations in which the acting individual is given to the observer in what is commonly called a face-to-face relationship. But, there are many other dimensions of the social world in which situations of this kind do not prevail—e.g., mailing a letter assumes faith in a vast system of social relationships we do not observe; as does reading an editorial about the French fearing the rearmament of Germany. In all these cases we know what is meant without observing overt behavior.[33]

In order to take into account all of the di-

Structure of Social Action (Glencoe: Free Press, 2nd Edition, 1949), esp. pp. 82 ff, 345–347, 484 ff; Felix Kaufmann, *op. cit.*, pp. 166ff.

[30] Peter Winch makes the similar claim that critics of Weber have misunderstood him. Winch claims that *Sinn* is the subjectively intended sense or meaning which an act has for the actor, and *verstehen* is to understand that, and not to have some special inner sense, or intuition, of causal relations. Weber explicitly asserted the necessity of verifying hypotheses through statistical laws based on observation of what happens. Peter Winch, *The Idea of a Social Science* (New York: Humanities Press, 1958), pp. 117–119, 112–113.

[31] Alfred Schutz, *op. cit.*, pp. 260–261.

[32] *Ibid*, p. 261.

[33] *Ibid*, pp. 262–263. According to Parsons, social interaction always involves ". . . the normative and the conditional. As process, action is, in fact, the process of alteration of the conditional elements in the direction of conformity with norms. Elimination of the normative aspect altogether eliminates the concept of action itself and leads to the radical positivistic position. Elimination of conditions . . . equally eliminates action and results in idealistic emanationism." And, as he points out, normative elements can only be conceived as existing in the mind of the actor, and are thus "inherently subjective." Talcott Parsons, *op. cit.*, p. 732.

mensions of social reality, therefore, Schutz argues that the social scientist must make use of his "common sense knowledge of everyday life." "This is so, because the world (the natural and the social one) is from the outset an intersubjective world and because . . . our knowledge of it is in various ways socialized."[34] Having nothing to do with introspection as a method opposed to scientific verification, *verstehen* is a result of processes of learning or acculturation. Moreover, according to Schutz, it is by no means a private affair of the observer which cannot be controlled by other observers. In his interpretation, *verstehen* is controllable and determinable at least to the same extent as, for example, the private perceptions of facts and the "intent" of the defendant in trials for murder.[35]

The world of nature, as explored by the natural scientist, does not 'mean' anything to the molecules, atoms, and electrons therein. The observational field of the social scientist, however, namely the social reality, has a specific meaning and relevance structure for the human beings living, acting, and thinking therein. Thus, the constructs of the social sciences are, so to speak, constructs of the second degree, namely constructs of the constructs made by the actors on the social scene, whose behavior the social scientist has to observe and to explain in accordance with the procedural rules of his science.[36]

This analysis makes it plain that there is an essential difference between the "thought objects" or mental constructs formed by the social sciences and those formed by the physical sciences.[37] The adoption of the principle of con-

[34] Alfred Schutz, *op. cit.*, p. 263. This "intersubjectivity" appears quite clearly as Schutz points out, in the self-correcting process of scientific inquiry itself. Science is a social enterprise, "but the postulate to describe and explain behavior in terms of controllable sensory observation stops short before the description and explanation of the process by which scientist B controls and verifies the observational findings of scientist A and the conclusions drawn by him. In order to do so, B has to know what A has observed, what the goal of his inquiry is, why he thought the observed fact worthy of being observed, i.e., relevant to the scientific problem at hand, etc. This knowledge is commonly called understanding. The explanation of how such a mutual understanding of human beings might occur is apparently left to the social scientist. But whatever his explanation might be, one thing is sure, namely, that such an intersubjective understanding between scientist B and scientist A occurs neither by scientist B's observation of scientist A's overt behavior, nor by introspection performed by B, nor by identification of B with A." *Ibid,* p. 262.

[35] *Ibid.*, p. 264. The "intersubjective (objective) nature of this learning process is analyzed by Schutz in "Common Sense and Scientific Interpretation of Human Action," *Philosophy and Phenomenological Research,* 14, No. 1 (September, 1953), p. 12. He relates the origin of common constructs to the understanding imparted by communication, which in turn depends on "a community of time and space" between the persons involved. And the latter implies that a certain sector of the outer world is equally within the reach of each person, and that they can interchange positions to acquire the perspective of the other. Significantly, Schutz cites Charles H. Cooley and George Mead on this matter.

The similarity, if not identity, between "social relations" and concepts in general should be obvious at this point. As even Lerner and Lasswell make clear, following Clyde Kluckhohn, in order to understand cultures we must know social relations and *social relations are meanings.* Observed action must be conceptualized in terms of their purposes in the minds of the subjects. Daniel Lerner and Harold D. Lasswell (eds.), *The Policy Sciences* (Stanford: Stanford University Press, 1951), pp. 89–90, 101. And, the meaning of concepts cannot be explained in terms of the actions of any individual person, or persons—they are presupposed, learned, and shared. See Maurice Mandelbaum, "Societal Facts," *British Journal of Sociology,* 6, No. 4 (December, 1955), p. 305; Felix Kaufmann, *op. cit.*, pp. 18–19; Peter Winch, *op. cit.*, pp. 121–123.

[36] Alfred Schutz, "Concept and Theory Formation," *op. cit.*, pp. 266–267. See also Peter Winch, *op. cit.*, pp. 83–85.

[37] This difference appears in the fact that the criteria according to which the student of politics ". . . judges that, in two situations, the same thing has happened, or the same action performed, must be understood in relation *to the rules governing sociological investigation.* But here we run against a difficulty: for whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientist's investigation itself, here *what the sociologist is studying,* as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity." Peter Winch, *op. cit.*, pp. 86–87. If, then, the scholar's criteria of concept formation must come from the rules of action found in political life, his relation

cept formation described above—namely, that classifications of social action must be in terms of concepts which are meaningful to the agent as well as to the observer—is therefore the only course of action open to the student of social behavior and institutions. But this in no way implies a non-behavioral position regarding the problem of verification. It is not that a different *way of knowing* (namely, "scientific" as against "intuitive") is at the basis of the differences between the physical and the social sciences, but that the *object* of knowledge is different. The social sciences must develop concepts of the intentional dimension of social reality. Thus, as Natanson recognizes, the "subjectivism" of Weber's postulate of interpretation does not mean that private, unverifiable elements are involved in the verification of hypotheses. As he quite correctly puts it, "it is concerned rather with the conceptual framework within which social reality may be comprehended."[38]

IV. THE PRACTICE OF CONCEPT FORMATION IN THE BEHAVIORAL SCIENCE OF POLITICS

The methodological principle that the concepts of the political scientist must be meaningful to the actor as well as to the observer is a principle that is exemplified in contemporary behavioral research without upsetting the verifiability principle. It would be impossible within the scope of this article to cite as evidence in support of this statement all relevant instances of empirical research accomplished since the turn of the century. Nevertheless, the conclusion that behavioral research conforms to this principle would seem to be justifiable if the two general *types* of inquiry, as illustrated by work which is accepted as representative by leaders of the behavioral movement,[39] were indeed found to conform to this principle of concept formation.

Attitude Studies. One broad area of inquiry which has become prominent among behavioral students of politics has been the study of orientations toward the political process. In this area socio-psychological phenomena such as motives, attitudes, and opinions are analyzed and used to explain such types of behavior as party preferences, voting participation, and the psychological bases of political extremism. The individual is the source of these types of primary data although often the analysis is developed in terms of the significant groups in a given society.

The study of attitudes and motives (relative to political ideology) is conveniently illustrated in the research done in this area by Herbert McClosky.[40] The report which describes these investigations into the relationships between personality attributes and conservative ideology succinctly and lucidly indicates that the procedures used in the formation of his basic concepts are completely in accord with the methodological principle which we have argued is a necessary assumption in social and political inquiry. Recognizing that the term "conservatism" is an important and common one in political life, and one that is used in different contexts to refer to the ideas of Burke, *laissez-faire* liberals, critics of the New Deal, or Republicans of any persuasion, McClosky admits that the definition that he has chosen to employ in his research, and the measure that was constructed from this definition, will not be satisfactory to everyone. However, the major point of interest to us is not the content of his definition but the manner in which he went about constructing it and the subsequent scales based on it. He began this construction by attempting "to extract from the tradition of self-styled conservative thought, and especially from the writings of Edmund Burke, a set of principles representing that tradition as fairly as possible. We have concentrated," he explains, "upon those attitudes and values that continually recur among acknowledged conservative thinkers and that appear to comprise the invariant elements of the conservative outlook."[41]

to the practitioners of politics ". . . cannot be just that of observer to observed. It must rather be analogous to the participation of the natural scientist with his fellow-workers in the activities of scientific investigation. Putting the point generally, even if it is legitimate to speak of one's understanding of a mode of social activity as consisting in a knowledge of regularities, the nature of this knowledge must be very different from the nature of knowledge of physical regularities." *Ibid.*, pp. 87–88.

[38] Maurice Natanson, *op. cit.*, p. 168. Compare this with Thelma Lavine, *op. cit.*, p. 150. See also Talcott Parsons, *op. cit.*, pp. 82–83 and 5, where it is made clear that the conceptual framework, though not necessarily the technique of verification, of the theory of action is couched in terms of subjective categories.

[39] See Heinz Eulau, *Political Behavior* (Glencoe: Free Press, 1956); and his article, "Political Science" in Bert F. Hoselitz, *A Reader's Guide to the Social Sciences* (Glencoe: Free Press, 1959), pp. 89–127.

[40] Herbert McClosky, "Conservatism and Personality," this REVIEW, 52 (March, 1958), 27–45.

[41] *Ibid.*, p. 30.

From these rather vague generalizations culled from conservative literature McClosky went on to isolate an "initial pool of 43 items" —that is to say, a tentative set of fairly brief and straightforward statements embodying what he thought were elements of conservative doctrine. This entire set of items was then submitted, "through survey methods, to a large general sample of persons in the vicinity of the Twin Cities who were asked to state, in relation to each item, whether they agreed or disagreed." This was done in order to select items which clustered sufficiently "to convince us that they belonged to the same universe, . . ." to reduce the number of items, and to insure consistency among them.[42] With the twelve-item conservatism measure that emerged from the analysis of the results of this survey, McClosky found that he could rank people from extreme conservatism at one end to liberalism on the other in a manner very close to the ranking obtained using the original 43-item pool.

In addition, the procedures used to validate the scale, like those used to construct it, exemplify the methodological principle previously stated. In order to determine the degree to which the scale measured what it purported to measure, McClosky submitted subsets of items from the twelve-item conservatism scale

> . . . to an advanced class in political theory, whose members had no prior knowledge of the study or its purposes. Each student was asked to supply a name or label for the group of statements and to write a paragraph explaining or justifying the label he had chosen. Of 48 students participating, 39 volunteered the word *Conservatism* as best describing the sentiments expressed in the statements, five offered names that were virtually synonymous with conservatism (e.g., traditionalism), while two supplied other names and two did not answer.[43]

Thus, just as McClosky developed his original propositions ("items" in the questionnaire which defined the operational concept of conservatism) out of statements drawn from the traditional doctrines of conservatism, so he also validated the concept of conservatism by determining whether living persons who were presumably aware of the contemporary definitions of conservatism in the tradition of political thought and in American society shared the same concept of conservatism. Undoubtedly it would be possible for a "crude behaviorist" to claim that all that this study is based on is the assumption that a conservative is someone who is defined by a series of marks on a certain sheet of paper. "Conservatism is what conservatism tests test," one might say, echoing the well-known tautology about IQ tests. This, however, was not McClosky's assumption. His introductory remarks clearly indicate his assumption that conservatism, like liberalism, is a set of ideas that are actually efficacious in orienting human behavior and have been, and are, historically existent. Conservatism is not, that is, merely a scientific construct, a classification developed in the minds of scholars in the hope that it might serve as an index of future behavior, much as a high score on an IQ examination seems to serve as a sign of future high performance in college.

On the contrary, conservatism is primarily a complex of notions that is actually operative in political life and McClosky's "Conservatism scale" is an attempt to measure, in the sense of ranking, a number of individuals along what is assumed to be a real attitude dimension. By forming his basic concepts on the basis of the expressions of conservative ideas given him by both past and present social actors or agents who are active, at least intellectually, in political life, he has insured that his eventual explanations of political preference, belief, and affiliation are based on concepts which are meaningful to those agents as well as to the scientific observer.

Attitude studies such as McClosky's thus indicate how it is possible to form, through the development of scales, objective concepts of so-called subjective meaning-structures. In the science of politics, as in all empirical sciences, the concepts we are dealing with have intersubjective validity and are not merely introspective, and the propositions we are dealing with—though there is an element of *verstehen* in their constituent concepts—are verifiable through methods that are similar in all the sciences.[44]

[42] *Ibid.*, pp. 30–31. Altogether, over 80 pools of items were submitted, the conservatism pool being only one of them.

[43] *Ibid.*, pp. 32–33.

[44] In addition to examining the contributions in the science of politics toward understanding the "orientations toward politics" which we have been discussing, Heinz Eulau's *Political Behavior* examines research done on the "agents and techniques of political power" and on activities going on in the "arena of political decision-making." The first is concerned with leadership studies, studies of the "mechanisms" by means of which influences are mobilized and transmitted. Often these studies are simply historical-descriptive analyses of social origins, religious affiliations, etc. See pp. 184–193. Where they are not, they are similar to the studies

Participation Studies. Another major type of inquiry is that which studies modes of political participation. For example, in a research study done a few years ago to determine how the Standard Oil Company (N.J.) stands with the American public, it became important to also find out who are most articulate in their feelings about big corporations and most concerned to attack or defend them. Since it was necessary to segregate the people who are relatively active in relation to national political issues from those who are relatively inactive, it was first necessary to define what "political activity" would mean for the purposes of this study.

While we are not interested in the content of the definition, the procedures followed in developing it are relevant. In defining "political activity" the authors first wrote out a large number of questions which they thought would throw light on an individual's political behavior, and then they tried these questions out "in personal interviews with a test national sample of American adults. Some of the questions were found not to work well in doorstep interviews, and were discarded. Others were found to yield results so closely intercorrelated that one question could in effect stand for several others."[45]

For our purposes, it is important to note that some questions were found to *mean* the same as several others, and could, in fact, be used in place of several other questions. The criteria of identity utilized in forming this concept of "political activity" were developed in terms that were as meaningful to the agents as to the observers on two levels. The first level is the level of language which is shared by agent and investigator; and, while this point is often neglected, this is what makes it possible for the scientist to "rough-up" a set of preliminary questions with the assurance that they will have at least a minimum of mutual significance and utility. But, secondly, we can also see in these studies that a conscious attempt was made to reduce the questions to the fewest which are most meaningful to the respondents, which most correspond to what they conceive of as "political activity." Aside from "face validity" (meaningfulness on the shared language level), the authors only claim that the resultant set of questions works well "in the sense that the questions are comprehended and answered with apparently little exaggeration by people of all educational levels and political persuasions."[46] What does this mean but that the investigator and the agent both understood the meaning of the questions, and that the investigator in some way felt that he knew that the agent understood?

Participation studies of this sort are relatively simple versions of the more complex studies which have come to be called studies of "community power structure." While some of these studies make use of a certain amount of participant observation, they all depend almost wholly on interviewing. Included in the interview schedules are always questions designed to measure what Herbert Simon calls "beliefs as to where legitimate power lies." In a recent study of participation in a small community, for example, "the sample was asked to nominate others as 'generally more influential,' and influential in regard to school matters, local government affairs and community welfare policies. . . ." In addition, in regard to these same issues, the respondents were asked to designate potential policy advisors by asking them: "Whom could you go to among your personal acquaintances, or persons you feel you could go to, for advice as to what should be done in regard . . ." to these issues?[47] The use of questions of this type indicates, again, that the political scientist wants and needs to develop concepts which are as meaningful to the agents as to the investigator. Answers to questions such as "Whom could you go to among your acquaintances for advice? . . ." obviously assume notions in the mind of the agent asked about the rule or norm of behavior that he is following in this context. "Though I am not now in the process of actually seeking such advice," the answer might be, "I *make it a rule* to consult X." Even in those cases in which the answer must be descriptive of past behavior —as in answer to the question, "whom have you consulted during the past year on issue Y?"—it

under the rubric "orientations toward politics" as far as the problem of concept formation goes. See pp. 193–204, 260–264. The second group is likewise concerned with applying the same procedures, only in the context of specific (non-community wide) loci, or arenas, of influence. The subject matter, i.e., influence, or political power, remains the same, as do the procedures involved in concept formation. Some of these studies involve merely descriptive statistics (pp. 275 ff.); others illustrate the type of concept formation analyzed (pp. 286–307).

[45] For example, Julian L. Woodward and Elmo Roper, "Political Activity of American Citizens," reprinted from this REVIEW, 44 (December, 1950), 872–885, in Heinz Eulau, and others (eds.), *op. cit.*, p. 133.

[46] *Loc. cit.*

[47] Robert E. Agger and Vincent Ostrom, "Political Participation in a Small Community," in Heinz Eulau, and others, *op. cit.*, p. 139.

should be clear that the question implicitly assumes that the principle according to which X has been consulted about Y continues to hold. For if, at the time the question was asked, the agent or respondent no longer intended to consult X, the proper answer would be, "I did see X, but now I distrust his judgment and will no longer consult him." At that time, X is no longer an influential in this respect. Clearly, in no case do we actually see the agent consulting X, nor do we know that his visit is to be identified as "a consultation about policy Y" unless he tells us that that is the meaning it has for him and for X. This "meaning," since it involves concepts (normative), can only be "discovered" by having them expressed in the form of statements or propositions—which is why interviews and questionnaires are so widely used in the social sciences, and why anthropologists entering upon the study of some exotic society must learn the language and saturate themselves in the traditions and history of the society. The latter is the only way that they can understand the patterns of action which are the institutions of the society. "Patterns of activity," therefore, actually are *normative concepts* communicated to the investigators by the agents.[48] Survey research, field studies or participant observation are techniques of applying the methodological principle that behavioral concepts, or constructs of "what is to count as doing the same thing," are derived from the normative concepts held by the subjects studied.[49]

V. THE RECENT REVISION OF BEHAVIOR THEORY

The thesis we have been supporting can be placed in a larger context by considering some of the recent changes that have occurred in behavior theory proper—that is, in the psychological theory of human behavior. These changes apparently have been neglected by both the opponents and the theoretical proponents of behavioralism in the study of politics.

Charles E. Osgood, in a recent analysis of some of the major trends in psychological theory, revealed the agreement among psychologists that the basic principles of simple stimulus-response (S-R) psychology are insufficient (but not necessarily wholly invalid) for an adequate analysis of human behavior.[50] In his lucid summary of the development of behavior theory in psychology, Osgood declares that the psychologist has always thought of himself as limited to observing what goes into the human organism, or "stimuli," and what comes out of it, or "responses." Between these two points at which observation is possible, there lies what he calls a "no-man's land of speculation." This "area" is, at least for the physiologist, the nervous system; sometimes psychologists have referred to it as "the little black box" in which stimuli are focused and in which, it is assumed, responses are created. Now, according to early behavior theory

[48] In a recent methodological study of anthropology S. F. Nadel wrote that it is invariably necessary for the anthropologist to go into the field and make himself "familiar" with the culture (usually primitive) that he wishes to study and overcome its strangeness by "something like an intellectual assimilation." Significantly, he stresses the fact that the study of the native language will probably have to precede the study of culture and social life. Since the meaning of terms and sentences of this new language cannot be gotten by counting the letters or words "scientifically" but must be *learned,* Nadel is obviously implying that before any of the "objective" controls used by anthropologists can be applied the investigator must learn the meaning of the concepts, and therefore of the institutions, of the new culture. S. F. Nadel, *The Foundations of Social Anthropology* (Glencoe: Free Press, 1951), p. 6. Underlining added.

It is also relevant to note the criteria Nadel suggests for isolating different types of so-called action patterns. "We have long held," he states, "that the order of things to which social institutions belong is built up through collecting together standardized action patterns *on the grounds of their aim contents.* . . . It might also seem that the people we observe themselves have names for these 'summaries' of *related action patterns*—'marriage,' 'family,' 'chieftanship,' 'property,' and the like. All such names . . . *stand for normative concepts;* the institution represents, for the actor, a rule or norm, *and has that kind of reality, that is, the non-spatial and in a sense timeless validity of concepts.*" *Ibid.,* p. 107. Underlining added.

[49] In addition to Peter Winch, *loc. cit.,* see Michael Argyle, *The Scientific Study of Social Behavior* (London: Methuen, 1957), pp. 14–26, where it is pointed out that the social scientist has two general types of measuring instruments all his own, namely, the different kinds of interview and questionnaire, and the techniques of controlled observation (the types we have discussed), and where it is also stated that investigators must be ". . . well steeped in the culture of the group members, . . ." to insure the correctness of their interpretations of the results of their tests and observations.

[50] Charles E. Osgood, "Behavior Theory and the Social Sciences," *Behavioral Science,* 1 (July, 1956), 167–185. Reprinted in Roland Young (ed.), *op. cit.,* pp. 217–244.

. . . the psychologist's task is to explain and predict relations between and among his two sets of observables, stimuli and responses, and to do this he must make certain assumptions about what goes on in the little black box. In other words, psychological theory, as contrasted with psychological observation, is made up of hunches about how the nervous system operates.[51]

Over the last century, however, psychologists found that a variety of phenomena appeared again and again in the midst of their traditional inquiries which proved embarrassing to the old single-stage (S-R) conception of behavior. These were symbolic phenomena, or "representational mediation processes." In their attempts to come to terms with these phenomena, Osgood points out that psychologists have produced a slow refinement in the rules of procedure by which they made and tested their "hunches" about what went on in the "little black box." His description of these developments is interesting, amusing, and succinct:

In nineteenth-century psychology the characteristic procedure in theorizing was to simply postulate a new entity or mechanism whenever some new regularity was discovered. Whenever something needed explaining, a new explanatory device was stuck inside the little black box, and it rapidly became chock-full of ill-assorted and ill-digested demons. For every nameable phenomenon of human behavior a different 'faculty' would be posited to explain it; for every nameable motive, a different 'instinct' would be listed as its explanation. And . . . Freud had big, flat-footed Super egos stomping around on red slippery Ids, while cleverly anxious little Egos tried to arbitrate. Thus . . . the little black box was filled with a wonderously diverse collection of explanatory devices, just about as many as there were things to be explained. This could fairly be called 'junk-shop psychology.'

In direct revulsion against this brand of theorizing, a group of American Behaviorists around the turn of the century went to the other extreme, claiming that the psychologist was better off if he made no assumptions whatsoever about what went on in the little black box . . . . This viewpoint toward theory has come to be known as 'empty organism psychology.' According to this view . . . there is absolutely nothing in the region between S and R, and what *is* there is none of the psychologist's business![52]

Gradually it became necessary for psychologists to postulate some sort of intervening variable between S and R. They had to put something back into the "little black box," as it were, in order to explain their observations. The contemporary behaviorist therefore has postulated an intervening or internal response-like process which produces self-stimulation. In other words, ". . . modern 'mediation psychology' sets up within the organism a replica of the S-R model, and it assumes that the same laws governing single-stage S-R process apply to both stages of the mediation model."[53]

Osgood goes on to develop much more complicated models of this mediation process, but the point remains the same. The observed behavior not only of humans, but also of animals, suggests that it is necessary to assume that often external stimuli arouse some intervening process which in turn produces a certain behavior—"problem solving," "learning," etc.,—which is partially independent of the stimulus. Osgood mentions several facts in both animal and human behavior which cannot be handled with a single-stage S-R model.[54] The term generally used to refer to this intervening process is "symbolic mechanism"—but, though the phrase seems Hobbesian, the methodology appropriate to a study of symbolic behavior indicates that a mechanistic analogy is inappropriate. As Osgood states, this type of theory

. . . places a premium on developing methods of indexing representational states, e.g., measuring *meaning*, particularly in human organisms. My own research . . . has been directed chiefly along these lines. Dealing with language responses themselves—which, after all, are supposed to be 'expressions of meaning'—we have been trying to discover a limited number of basic factors or dimensions along which meaningful reactions vary and hence can be measured.[55]

It seems to me that in "dealing with language responses" Osgood—and other modern behaviorists dealing with somewhat different problems—must operate in a context of shared meaning with their "respondents" or "subjects." That this is the case is indicated by an example given in Osgood's article of a study of abstract "assigns," that is to say, symbols or terms whose meanings are literally assigned through repeated association with simpler signs having direct behavioral reference.[56] This implies that

[51] *Ibid.*, pp. 217–218.

[52] *Ibid.*, pp. 219–220.

[53] *Ibid.*, p. 220. See also Arnold Rose, *Theory and Method in the Social Sciences* (Minneapolis: University of Minnesota Press, 1954), pp. 157, 275–278.

[54] Osgood, *op. cit.*, p. 233. See also Felix Kaufmann, *op. cit.*, pp. 154–55; Ernest Nagel, *The Structure of Science*, p. 477.

[55] Osgood, *op. cit.*, p. 239.

[56] *Loc. cit.*

meanings can have an existence independent of immediate physical stimuli. As Osgood points out, it is precisely this which makes possible imitation, identification, and the sharing of personality and culture traits. Furthermore, it is precisely this learning process that lies at the bottom of all types of social behavior. And it is this capacity of personality to construct "equivalences" (which refer to the same fact that Peter Winch called the need and the ability to formulate rules) which lies at the bottom of concept formation in everyday life as well as in the professional life of the behavioral scientist. Thus, the philosophical objection of Peter Winch to behaviorism—that the social scientist must base his judgments of identity, his classifications, and therefore his entire conceptual scheme on the judgments of identity (or the meaning) attributed to their own and others' behavior by the subjects studied themselves—is a valid criticism of what Osgood has called the "single-stage S-R process" or early behaviorism, but it provides only a philosophical clarification of modern behavior theory.

Thus, both the present practices of behavioral students of politics and contemporary behavioral theory have converged toward a rediscovery of mind in the sense of assuming representational states whose meaning is expressed by language responses. No longer is mind nothing but a "subjective entity" to be discarded from scientific inquiry according to the program of Bentley and the "radical behaviorists." And the techniques which have been developed to rank individuals along attitude dimensions according to their meaningful statements (not, it should be noted, to "measure" the concepts in their heads) do not merely apply the methods of the physical sciences to the "moral sciences" as Mill and his intellectual descendents hoped would be possible. No physical scientist is ever required in his studies to ask questions of his objects of inquiry as part of the process of concept formation; yet, this has become in practice and in theory a fundamental principle of behavioral research.

VI. CONCLUSION

The notion that it is possible and sufficient to observe overt patterns of social interaction in the same way it is possible to observe the pattern of tracks left in the snow by some winter stroller belongs to the doctrines of early behaviorism as exemplified in political theory in the writings of Bentley. Although it is no longer present in either the theory or practice of behavioral science, it remains a vestigial feature of the idea of a science of politics in the minds of some of its critics. These critics have advocated the thesis that the study of politics cannot eliminate common sense notions of events and behavior and the distinctions made in ordinary language.

However, a critical understanding of the notion of *verstehen,* which has been put forth as the method of discovering common-sense meanings, led to the conclusion that indeed the political scientist must base his judgments of identity, and therefore his classifications, on the judgments of identity (or the meaning) attributed to their own and others' behavior by the subjects or agents who are being studied. Participants in the social situation itself "define the situation," that is, define the underlying norms guiding action, and therefore also the institutions and "patterns of behavior" that the political scientist investigates. But we also concluded that developing criteria of identity and forming concepts is a problem of semantics, and thus a philosophical problem. Therefore, our procedures at this level of rational activity do not change the fact that, once those concepts have been formed and made use of in the construction of scientific hypotheses, the procedures or methods for *verifying hypotheses* are logically the same in the social as in the physical sciences.[57]

The most urgent question of this part of the

[57] We may agree with Nagel and with Schutz that all of the empirical sciences must be objective "in the sense that their propositions are subjected to controlled verifications and must not refer to private uncontrollable experience." Alfred Schutz, *op. cit.,* p. 270. We may also agree that in the empirical sciences, theory means "the explicit formulation of determinate relations between a set of variables in terms of which a fairly extensive class of empirically ascertainable regularities can be explained . . ." (and that) "neither the fact that these regularities have in the social sciences a rather narrowly restricted universality, nor the fact that they permit prediction only to a rather limited extent, constitutes a basic difference between the social and the natural sciences, since many branches of the latter show the same features." *Ibid.,* pp. 260–261. See also Ernest Nagel, *The Structure of Science,* pp. 484–485.

See also Donald Davidson's defense of the notion that explanation by motives is a species of causal explanation rather than incompatible with it: *The Journal of Philosophy,* 60 (November 7, 1963), 685–700.

A careful analysis of the various meanings of "meaning" and "understanding" which makes clear how a scientist can "understand" say, religion, as an observer without "understanding" it as a believer is presented by May Brodbeck in "Mean-

ing and Action," *Philosophy of Science,* 30 (1963), 309–324. Professor Brodbeck also argues our thesis that participation in a culture is necessary to achieve *both* these (and other) kinds of "understanding"; yet that the use of a common language for communication is not incompatible with the viewpoint of the scientific observer. Both articles are reprinted in May Brodbeck (ed.), *Readings in the Philosophy of the Social Sciences* (New York: Macmillan Co., 1968), pp. 44–78.

methodology of political science then becomes: "How is it possible to form objective concepts and an objectively verifiable theory of subjective meaning-structures?"[58] How this is possible, as well as evidence for the fact that modern behavioral scientists actually do form their concepts on the basis of concepts that are meaningful to the agents being studied, was illustrated by examining some recent behavioral research.[59]

In this sense, the foregoing criticism of the notion of *verstehen* together with the analysis of the procedures actually involved in developing criteria of identity provide a theoretical clarification of some of the methodological assumptions underlying what modern behaviorists are actually doing in their empirical research. Likewise, political theorists, traditionalists, or "anti-behavioralists" should realize that "under the pressure of trying to handle language phenomena," scholars working in behavioral science have modified their postulates and "have found it necessary to put something else back into the little black box." The "little black box," the human organism, is no longer postulated as an empty cipher. "Central integration," as well as "symbolic, representational processes," are necessary assumptions to explain language and other behavior. And it should be obvious to the "anti-behavioralists" that a discipline which assumes, again in Osgood's words, that "language responses are indicative of meaning," and that methodologically the psycho-social researcher must deal with those language responses themselves to determine "the limited number of dimensions along which meaningful reactions vary,"[60] is making an assumption about "human nature," and about language and reason, that is no different from the assumption which traditional political theorists make about the meaningfulness or intentionality of political life.

[58] Alfred Schutz, *op. cit.,* p. 270.

[59] A difficult problem arises, however, as soon as this viewpoint is accepted. That is the problem created by the possibility that an actor might himself be wrong regarding what he is doing or intending to do. Neurosis, obtuseness, malice, lack of knowledge regarding possible unintended consequences all make it difficult for the social scientist to equate an individual's expressed intention with the "real meaning" of his act. On the surface it would seem that modern research technology does cope with this problem through proper questions and analysis of answers. On the other hand, the problem of unintended consequences especially would seem to push one on from the level of concept formation to the level of explanation. In either case, the problem seems sufficiently complex and related to other aspects of theory construction to warrant separate investigation.

[60] Charles E. Osgood, *op. cit.,* pp. 221–222.

DECISION-RULES AND INDIVIDUAL VALUES IN CONSTITUTIONAL CHOICE*

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Once a political community has decided which of its members are to participate directly in the making of collective policy, an important question remains: "How many of them must agree before a policy is imposed on the community?" Only if participation is limited to one man does this question become trivial. And this choice of *decision-rules* may seem only a little less important than the choice of rules[1] in a world so largely governed by committees, councils, conventions, and legislatures. This paper is about the consequences of these rules for individual values.

Both the oral and written traditions of political theory have generally confined the search for optimal (or "best") decision-rules to three alternatives.[2] The *rule of consensus* tells us that all direct participants must agree on a policy which is to be imposed. *Majority-rule* tells us that more than half must concur in a policy if it is to be imposed. And the *rule of individual initiative* (as we may call it), holds that a policy is imposed when any single participant approves of it. These three decision-rules—"everyone," "most of us," and "anyone"—are terribly important, but they cannot be said to exhaust the available alternatives.

The list of alternatives is just as long as a committee's roster. Only for a committee of three would 'consensus,' 'majority' and 'individual initiative' exhaust the possibilities. In a committee of n members, we have n possible rules. Let the decision-rule be a minimum number of individuals (k) required to impose a policy. For a committee of five, we have five alternative values of k:

individual initiative ↓		majority rule ↓		rule of consensus ↓
$k=1$	$k=2$	$k=3$	$k=4$	$k=5$

($n=5$)

This means, of course, that the number of possible rules is often very large. The U.S. House of Representatives might, for example, choose any of 435 decision-rules; the British House of Commons could choose any of 630 rules; and the Supreme Soviet might choose any of 1300 rules. A committee of n can, in short, choose any of n possible rules.[3] And one can hardly claim to have identified the 'best' decision-rule,

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[1] Well, maybe somewhat more than a "little less important," but not trivial by comparison: for evidence in a very practical argument, consider J. J. Servan-Schreiber, *The American Challenge* (New York: Atheneum, 1968), pp. 170–178.

[2] This limitation is illustrated by Abraham Lincoln's remark: "Unanimity is impossible; the rule of a minority as a permanent arrangement, is wholly inadmissable; so that, rejecting the majority principle, anarchy or despotism in some form is all that is left." Cited in Henry B. Mayo, *An Introduction to Democratic Theory* (New York: Oxford University Press, 1960), p. 178, and in Thomas Landon Thorson, *The Logic of Democracy* (New York: Hold, Rinehart and Winston, 1962), p. 142.

[3] Rules with a value equal to or less than n over 2, of course, present special liabilities, for they permit the formation of more than one winning coalition on a single issue. But for some classes of issues this is not a serious difficulty, since multiple policies may be effected. An example is provided by *certiorari* proceedings in the U.S. Supreme Court: in these instances, the so-called "rule of four" ($k=4$, $n=9$) is applied, so that any four justices may require that a writ be granted. This produces no difficulties, since the granting of one writ does not preclude the granting of another—multiple policies are permissible.

whatever his normative standard, unless he has shown it superior to the n−1 alternatives.

This paper reports one normative analysis of the decision-rules available to a generic political committee.[4] In it, I consider a single, anonymous individual who wishes to optimize the correspondence between his own values, however selfish or altruistic, and those expressed by collective policy. This individual would like to "have his way" as often as possible, by securing the adoption of proposals he likes and the defeat of proposals he dislikes. For him, then, the "optimal" decision-rule will be the one which minimizes the frequency with which he must expect to support a proposal yet have it defeated, or to oppose a proposal yet have it adopted. This is because his values will correspond to collective policy most closely under rules which minimize the frequency with which he is "outvoted" in one of these two ways. And, for him at least, the best decision-rule will be the one which minimizes the expected frequency for these unhappy events. The resulting normative criterion is elaborated in Part I below.

The formal analysis begins (in Part II) with a simple model for the operation of political committees. This is a "null" model in the sense that it presumes very little information about the future. Under it, we know nothing about the (long-run) agenda which will confront the committee, about the ways individuals will evaluate the proposals which do arise, or about the factional structure of the committee. This model serves two purposes. First, it produces a recommendation appropriate to the circumstances of "constitutional choice": If we presume to know very little about an indefinite future, this model rigorously demonstrates the optimality of majority-rule under our individualist criterion. And second, the results of the simple model provide a "baseline" against which to consider the normative implications of additional information about the future. In Part III, we consider the consequences of information which would, for example, lead us to expect the formation of a standing minority faction, or a general prejudice for or against government action.

Let me begin by explaining my normative criterion and distinguishing it from three leading arguments about decision-rules—those which rest on the ideals of equality, economy, and "truth."

[4] The term "political committee" is used here in its broadest sense—to include all bodies which vote on collective policies. This use is illustrated by Duncan Black's book, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1963).

I. A NORMATIVE CRITERION

Let us suppose that the individual with whom we are concerned has some personal schedule of values. We know nothing about the specific content of his value schedule—its selfishness or altruism, its sophistication or naivete, its generality or specificity—since this individual, whom we may call "Ego," is a purely generic figure. We must, however, suppose that his schedule is complete enough that it leads Ego to like or dislike each proposal which comes before the committee. Accordingly, or major assertion is that Ego would like to optimize the correspondence between his preferences and the policies which are imposed by the committee:[5] he wants to have his way by defeating proposals which his values lead him to dislike and by imposing those which they lead him to like. Our major normative assumption is then:

Value Assumption:[6] Ego wishes to optimize the correspondence between his schedule of values and the list of policies which are imposed.

Now let us suppose that Ego looks ahead and contemplates four events which might occur:

A. Ego's value schedule leads him to support a proposal, but it is not imposed.

B. Ego's value schedule leads him to oppose a proposal, but it is imposed.

C. Ego's value schedule leads him to support a proposal, and it is imposed.

D. Ego's value schedule leads him to oppose a proposal, and it is not imposed.

Ego need not be much of a logician to see that our "Value Assumption" will be best served by the frequent occurrence of events C and D. Since it is also clear that the four classes of events are disjoint, Ego can be sure that a decision-rule which minimizes the summed frequencies of A and B will also maximize the summed frequencies of C and D. Hence, Ego

[5] This assumption is less innocuous than it may appear: complete indifference is not improbable even among the very active (viz. committee members).

[6] Here, I am putting aside the theoretically important "problem of intensity." In effect, the value assumption implies that all issues evoke equally intense preferences. This decision rests on theoretical intractability of the problem. Some of the major difficulties are outlined in, Willmoore Kendall and George Carey, "The 'Intensity' Problem and Democratic Theory," this REVIEW, 62 (1968), 5–24, and in Douglas W. Rae and Michael Taylor, "Some Ambiguities in the Concept of 'Intensity,'" *Polity*, 1 (March, 1969).

can see that the lower the frequency of events A and B, the higher the frequency of C and D, and the higher the degree of correspondence between his schedule of values and the list of policies imposed by the committee. This leads to a criterion by which Ego may evaluate decision-rules.

Criterion for Choice Between Decision-Rules: One should choose that decision-rule which minimizes the sum of the expected frequencies for (A) in which the committee does not impose a policy which has value schedule leads him to support, and (B) in which the committee imposes a policy which his value schedule leads him to oppose.

This is the normative criterion which underlies our analysis. Each decision-rule will be evaluated by the frequency with which we must, under given assumptions, expect these events to occur if that decision rule is adopted. Our initial commendation of majority rule will imply nothing more than this: under majority rule, the expected (summed) frequency of events A and B is at a minimum, and majority rule is therefore optimal under this criterion. This in turn suggests simply that majority rule maximizes the probability that our (anonymous) individual will "have his way" with respect to a given proposal. And this, in the long run, suggests that majority rule will optimize the correspondence between individual values and collective policies. Since "having one's way" is its central mechanism, we may call this criterion *political individualism.*

The "moral tone" of this position is exactly that of the individuals with whom we are dealing—no better and no worse. Under the criterion of political individualism, we must recommend decision-rules which optimize the correspondence between individual values and collective policies, even if we do not like these values, and even if we do not consider these values "rational" in some larger context.

The best known alternative to this criterion is also independent of the "morality" or "rationality" of individual values: this is, of course, the criterion of *political equality.* While "equality" is a word of many meanings, its most relevant use for present purposes is the rule that, ". . . the weight assigned to the choice of each individual is identical."[7] A minimum consequence of this criterion is that all decision-rules must be defined by the *number* of members who must vote yes if a policy is to be imposed, regardless of *which* members these may happen to be.[8] This criterion is the best-known basis for recommendations of majority-rule. As Neal Reimer puts the case, any alternative rule amounts ". . . to ignoring the majority: to declaring that the minority shall be considered and weighted *unequally.*"[9] In the present analysis, this is taken for granted: we are equally concerned with each individual member, and we define decision-rules without respect to the identities of the individuals voting yes and no. Here we are examining the additional requirement that a decision-rule optimize the expected correspondence between these (equally valued) individual preferences and the policies which a committee adopts.

A second and more restrictive alternative to political individualism is offered by the family of criteria based on the *truth-value* of collective policies. An example is provided by the 15th Century Council of Ferrara, which adopted a two-thirds decision-rule for proposals to alter Church doctrine, apparently on the presumption that this was a hedge against theologically fallacious changes.[10] Another example is held out be Rousseau's first criterion for the choice of a decision-rule: ". . . the more grave and important the questions discussed, the nearer should the opinion that is to prevail approach unanimity."[11] The apparent presumption is that voting

[7] See Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), pp. 67, 84.

[8] This requirement is closely related to K. O. May's condition of "anonymity," under which the decision-rule does not depend in any way upon the identification of individual members. See his "A Set of Independent Necessary and Sufficient Conditions for Simple Majority Decision," *Econometrica,* 20 (1952), 680–684. May's paper shows that majority rule alone satisfies "anonymity" and three other conditions simultaneously.

[9] Neal Reimer, "The Case for Bare Majority Rule," *Ethics,* 62 (1951-52), 16–32. The cited statement is from p. 17.

[10] Thomas Baty, "The History of Majority Rule," *The Quarterly Review,* 216 (1912), 1–28 See especially pp. 22–23. The Council also was tri-cameral: within each chamber, a two-thirds rule prevailed; between chambers the rule of consensus was followed. This is indeed a restrictive decision-rule, and a fine hedge against doctrinal error, *if* we assume the existing doctrine is altogether correct.

[11] *The Social Contract,* translated by G. D. H. Cole (New York: E. P. Dutton, 1950), p. 107. Rousseau's second rule was that ". . . the more the matter in hand calls for speed, the smaller the prescribed difference in the number of votes may be allowed to become: where an instant decision has to be reached, a majority of one should

is a sort of sampling device for the determination of the General Will: restrictive sampling criteria reduce the chance of a policy inconsistent with the General Will being adopted. Our criterion makes no claim to the optimization of truth values, mainly because such a criterion seems epistemologically indefensible, and because it suggests that past policy is necessarily "truthful," independent of changing circumstances.

Yet a third alternative, much closer to the present criterion, is offered by *economic individualism*. This criterion is embedded in the often fruitful application of economic methods to politics—the works of Olson, and Buchanan and Tullock being important examples.[12] As the latter authors put the criterion, ". . . individuals will, on the average, choose more rather than less when confronted with the opportunity for choice in the political process, with more and less being defined in terms of measurable economic position."[13] In their analysis of decision-rules, Buchanan and Tullock define two cost functions: (1) "external costs," which are defined by the costs a single individual ". . . expects to endure as a result of the actions of others," and (2) "decision-making costs," which are defined by "the time and effort . . . which is required to secure agreement . . ." among the number of people required by a decision-rule to impose a policy.[14] One then looks for a rule which minimizes the sum of these costs.[15]

While this approach is in many respects a powerful one, I have chosen not to follow it here for two major reasons. The first is most important: we are interested in a political problem in its own right, and this must not be submerged in the conceptual (sometimes even ideological) bath of economic individualism. We do not wish, therefore, to conceive the problem of having one's way as a means to success in a larger economic game, but to leave the source of individual values open to the unspecified "value schedules" of individuals. A second, less important reason is methodological: I know of no way to associate costs with decision-rules and at the same time assure that these costs are additive. So, instead, I have chosen to examine expected frequencies for having one's way without subsuming these frequencies under more general cost-functions. While this analysis owes important intellectual debts to these economic models —especially to Buchanan and Tullock—it should not be viewed as a contribution to that tradition.[16]

We may now proceed with our analysis, taking political individualism as our normative criterion. We begin with an individual who tries to evaluate decision-rules under the bewildering uncertainty of "constitutional choice."

II. A SIMPLE CONSTITUTIONAL CHOICE MODEL

We are now interested in the frequencies with which our generic number (Ego) must expect the events A and B to occur under a given decision-rule. We begin, here, with a model which generates these expected frequencies from the temporal (and epistemological) perspective of "constitutional choice." As James Buchanan ex-

be enough." Condorset actually devised a truth-function theory using probabilistic assumptions similar to those found in Part II below. See *Essai sur l'application de l'analyse à la Probabilité des Décisions Rendues à la Pluralité des Voix* (Paris, 1785).

[12] Mancur Olson, *The Logic of Collective Action* (Cambridge, Massachusetts: Harvard University Press, 1965); and James Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962).

[13] Buchanan and Tullock, *op. cit.*, p. 29. The quotation is one of two interpretations which these authors give to the more general criterion of utility maximization.

[14] *Ibid.*, pp. 64, 68.

[15] *Ibid.*, p. 70. "For a given activity the fully rational individual, at the time of constitutional choice, will try to choose that decision-making rule which will *minimize* the present value of the expected costs that he must suffer. He will do so by minimizing the *sum* of the expected external costs and expected decision-making costs. . . ." It is not clear that these costs can be added on any single scale. For a commentary on these cost functions see, Herbert J. Kiesling, "Potential Costs of Alternative Decision-Making Rules," *Public Choice*, 4 (1968), 49-66.

[16] Although Buchanan and Tullock produce no rigorous evaluations of decision-rules, two of their speculations seem especially relevant to the present analysis. First, they point out that majority-rule holds no special *a priori* promise as a cost minimization (p. 81). Second, they suggest that ". . . the low-cost point on the aggregate 'cost curve' would tend to be represented by simple majority voting," under circumstances which appear comparable to those presented in Part II of this paper (p. 129). No strict demonstration of this tendency is offered, and care must be taken not to confuse the frequency curves employed here with the cost-functions which these authors consider. If any determinate relation exists, and I am not sure one does, it is between the frequencies usd here and "external costs" in their analysis.

plains, this perspective drastically limits the information which may be incorporated in our model:[17]

> The center of attention becomes the mental calculus of the individual as he is confronted with a choice among alternative rules for reaching subsequent political decisions. The individual does not know, nor is he able to predict, what particular issues will be presented subsequent to the adoption of the rule. And, even if he [could] predict with some accuracy the sort of issues that may arise, he could hardly predict his own position vis-a-vis the other members of the group.

The model presented here assumes no knowledge of an indefinite agenda and the preferences of individuals at discrete points over that agenda

The more formal model developed below rests on six rather concrete assertions which may be stated at the outset:

1. We are dealing with a political committee, that is, one which is authorized to make collective policies.
2. This committee will deal with a succession of policy proposals, and is free to impose or reject each.
3. This list of proposals (agenda) is unknown at the time the decision-rule is chosen.
4. Since the proposals are unknown, so are the preferences of the members on each proposal.
5. The committee will contain n coequal members, and n is at least three ($n \geq 3$).
6. Each member either supports or opposes each proposal.

We suppose that one member of this committee, whom we call Ego, is interested in making a choice among the available decision-rules. His choice may be established by a means-ends inference from: (1) the normative criterion developed above, and (2) the model developed here for the purpose of predicting the frequencies of events A and B under each of the available decision-rules.

Our model assigns expected frequencies to events A and B, and associates these with each value of k in a committee of n members. This is accomplished by making certain assumptions about the way individual committee members decide whether to support or oppose each proposal. These assumptions allow us to generate a distribution of "support sets" for policy proposals, and these sets may be partitioned by two criteria: (1) their size (do they equal or exceed k?) and (2) their inclusiveness (how many of them of any size will include or exclude our generalized member, Ego?). These partitions are used to define "sets of support sets" corresponding to events A and B. We are then able to generate expected frequencies for these sets—that is, for the occurrence of events A and B at any point in the committee's agenda. This allows us to compare the expected frequencies of these undesirable events for each available decision-rule (i.e., each value of k), and to apply our criterion of choice to these expectations. Let us begin with our assumptions about individual decision-making by committee members.

Assumptions about Individual Decisions. As we have already indicated, each member either supports or opposes each proposal as it occurs. We know nothing of the proposals (or the member's reactions to them), so we must make certain assumptions about the ways in which members decide whether to support or oppose each proposal. Briefly, we assume that each member decides whether to support or oppose each proposal independently, on the basis of his own value schedule, and that he is exactly as likely as not to support each proposal.

Assumption I: Each member decides whether to support or oppose each proposal on the basis of his own value schedule. Value schedules are independently constructed. Therefore, the probability that one member will support (or oppose) a proposal is independent of that probability for any other member.

Let: P represent probability
E represent the eventuality that member (E) supports a proposal
e represent the eventuality that member (E) opposes a proposal
X,x represent these eventualities for member (X)
$[a/b]$ should be read "a, given b"

Then:

$$P[_E/X] = P_E$$
$$P[_E/x] = P_E$$
$$P[_e/x] = P_e$$
$$P[_e/X] = P_e$$

Since "Ego" is a generalized member, Assumption I was already implicit in our Value Assumption (above). Every member is trying to optimize the correspondence between the list of policies imposed and his own, independently

[17] "An Individualistic Theory of Political Process," in David Easton (ed.), *Varieties of Political Theory* (Englewood Cliffs: Prentice-Hall, 1966), pp. 25–37, quotation from p. 29. For a related statement, see also Elinor Ostrom, "Constitutional Decision-Making," unpublished paper.

constructed value schedule. It follows that the decisions of members are independent events, whose probabilities are not causally related to each other. This assumption is not, of course, meant as a descriptive generalization suggesting that members never influence or lead each other: it is simply a model assumption.

Assumption II: The probability that each member will support any proposal is exactly one-half, and, since he must either support or oppose each proposal, the probability that he will oppose any proposal is also exactly one-half.

Let: the notation remain the same.
Then:

$$P_E = \tfrac{1}{2} = .5$$
$$P_o = 1 - P_E = 1 - .5 = .5$$

This is arbitrary, but there are three reasons not to make any assumption different from Assumption II. First, no other assumption seems more "realistic" in an intuitive sense, and some seem a good deal less "realistic." That, however, is not a major concern. Second, any asymmetrical assumption would enormously complicate our model and its predictions because it would deny us the use of the very simple calculus of probabilities for combinations of equally likely, independent events. Third, and crucially, it can be shown that no alternative to Assumption II alters the location of the minimum value for the expected summed frequencies of events A and B. Our finding is general to the whole set of alternative assumptions. This is demonstrated in the appendix. So, in the absence of any expected alteration in our major prediction, there seems no reason to complicate the model.

Expected Frequencies of Support Sets. All proposals will evoke a response from each member, and, taken collectively, these responses will form a combination. We need to know the number of different combinations with various properties, and the probability for each. One essentially trivial assumption must be added before these calculations are begun:

Assumption III: Proposals do not enter the committee's agenda unless at least one member supports each of them. Therefore, there will be no combinations of rsponses in which no member supports the proposal.

Let: [n letters] form a combination of responses to a proposal
Other notation remains as above.

Then:

$$P_{[xyz\ldots n]} = 0$$

This assumption, which has no substantial effect on our major prediction,[18] is simply our way to account for "non-events" in which no member makes a proposal. Given these three assumptions (and elementary probability theory), two important deductions appear:

Deduction I:[19] The total number of possible discrete combinations of responses for the committee of n members will be two to the n^{th} power, minus one:

Let: n represent the number of members
N represent the number of possible combinations of responses to any proposal

Then:

$$N = 2^n - 1$$

For example, a committee of three generates seven possible response combinations:

XYZ
XYz
XyZ
xYZ
Xyz
xYz
xyZ

And, $2^3 - 1 = 8 - 1 = 7$

Our strategy will be to determine what proportion of this number of possible response combinations (later called support sets) are equivalent to events A and B. This, with the help of Deducation II, will allow us to arrive at predicted frequencies for these events under various decision-rules. In passing, it is worth noting that the number of possible combinations expands very rapidly:

n	N
3	7
10	1,023
20	1,048,575
100	$12{,}676 \times 10^{28}$ apx

[18] The effect of this assumption is to discard an event in which every member "has his way" since all would vote no (i.e., Event D.) This simply makes the prediction slightly less optimistic than it would otherwise have been, but it has no effect on the location of the A + B minimum in which we are interested.

[19] This follows from the rule of combinations: the number of combinations of size f which may be drawn from n objects equals $n!/f!(n-f)!$. If we allow f to take all values $0 \ldots n$, the resulting sum is 2^n. Assumption III eliminates the one case in which $f = 0$ (i.e. $n!/n! = 1$), so the result is $2^n - 1$.

This means, of course, that any effort to literally "count" response combinations (or support sets) of various types will be quite futile.

Deduction II:[20] Every possible response combination is equally probable, and that probability is always the reciprocal of the number of possible combinations for a committee of n members.

Let: $n=2$

The notation remains as above.

Then:

$$P_{XE} = P_{Xe} = P_{eX} = \frac{1}{2^n - 1} = \frac{1}{2^2 - 1} = \frac{1}{3}$$

Since this deduction is general to all values of n, it may, for example, be calculated that the probability for each combination in a committee of ten is one over 1,023.

Now, we may convert what we have been calling "response combinations" into what we will call *support sets,* which contain only the members of any combination who support a proposal. This differs from a conbination only in the matter of notation. We drop those who oppose a policy, so response combination XYz becomes support set XY. This does not mean that opposition is ignored, since (Z)'s absence here tells us of his decision quite as much as the inclusion of a small "z" would. And, since no transformation has been made, we can be sure that what we have deduced for response combinations will hold also for support sets. This is so because every response combination corresponds to one and only one support set:

Response Combination	*Support Set*
XYZ	XYZ
XYz	XY
XyZ	XZ
xYZ	YZ
Xyz	X
xYz	Y
xyZ	Z

Since assumption III eliminates uniformly negative combinations (i.e., xyz), this is general and no empty support sets occur.

Expected Frequencies for Sets of Support Sets Corresponding to Events A and B. We may now define events A and B by the "sets of sets"—collections of support sets—to which each corresponds, and compute the expected frequency of each. (A) Event A occurs when our generalized member (Ego) supports a proposal (belongs to a support set) but is unable to impose it (the size of the set is less than the decision-rule value, k). We may identify the set of support sets (A) which corresponds to event A:

Let: A be the set of support sets which defines the occurrence of event A.
k be the minimum number of members required by the decision-rule to impose a policy.
S be a support set.
S_f be a support set of size f.
$\in$ should be read "lies in"
$\approx$ should be read "is equivalent to"

Then:

$$A \approx [S_f, \text{ such that, } f < k, \text{ \& Ego} \in S]$$

This says simply that the set of sets (A) which corresponds to event A contains all those support sets of sizes (f) less than the minimum value imposed under a decision-rule (k), which also include our generalized member. In short, Ego fails to impose a policy which his value schedule leads him to support (event A).

We are now interested in finding out how many support sets belong to set of sets A. This will depend on the decision-rule value (k) for each committee of n members. First, it is apparent that whenever the value of k is one (i.e., the rule of individual initiative), A must be an empty set. Under rule $k = 1$, the mere inclusion of our generalized individual defines the set as $\geq k$ and means that f cannot have a value of less than k. In short:

$$\text{If } k = 1, \text{ A empty}$$

This means that the rule of individual initiative prevents any member from having a proposal he supports defeated (event A). This, however, is not much help with the whole range of values for k which exceed one. A more general formula may be used to determine the number of support sets belonging to A for all values of k:[21]

[20] This follows from Assumptions I and II. The combinations are compounded of independent, equally probable events (i.e., individual decisions), and are therefore themselves equally probable events. Since there are $2^n - 1$ combinations (Deduction I), and each is equally probable, each has a probability of $1/2^n - 1$

[21] This formula is derived from the rule of combinations and our set definitions. Let us begin with the rule of combinations: The number of support sets of size f is:

$$\frac{n!}{f!(n-f)!}$$

Now, we wish to find the number of combinations of any size (f) which contain a generalized member (Ego), in accordance with our set definition.

Let: N_A be the number of support sets in A
Other notation remains as above.

Then:

$$N_A = \sum_{f=1}^{k-1} \frac{(n-1)!}{(f-1)!(n-f)!}$$

This formula is designed to "count" the support sets of size (f) which contain one generalized member (Ego), and to sum those for every value of f up to $k-1$. This tells us how many distinct support sets include Ego and still don't win under any rule k. By dividing by the total number of possible support sets—winning, losing, with Ego or without Ego—we may generalize this formula to the expected frequency of Event A:

Expectation for Event A: Any member of a political committee of size n, operating under decision-rule k, must expect that a proposal he supports will be defeated in:

$$\frac{N_A}{2^n - 1}$$

proportion of the instances in which a proposal comes to a vote. The full formula may be stated:

Let: E_A represent the expected frequency of event A

Other notation as above.

Then:

$$E_A = \frac{\sum_{f=1}^{k-1} \frac{(n-1)!}{(f-1)!(n-f)!}}{2^n - 1}$$

This statement is fully general to committees of any size, and it applies equally to each member of any such committee.

Table 1 gives the expected frequency for event A (E_A) for committees for three to twelve members, and decision rules from 1 to n. We

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We may accomplish this by saying "If Ego belongs to a set of size f, there will remain $n-1$ members to be combined in a set of size $f-1$ in order to bring our set to size f with Ego included." Accordingly, we subtract 1 from n and f in the combinatory formula, obtaining:

$$\frac{(n-1)!}{(f-1)![(n-1)-(f-1)]!}$$

This may be simplified by letting the -1 terms in the lower right hand bracket cancel each other:

$$\frac{(n-1)!}{(f-1)!(n-f)!}$$

This tells us how many support sets of size f will contain Ego. Now, we also need to know the sum of these values for all instances in which the set's size (f) is less than k. This is accomplished by letting the values be summed from $f=1$ to $f=k-1$. This accounts for summation term:

$$\sum_{f=1}^{k-1}$$

And the result is the formula given:

$$N_A = \sum_{f=1}^{k-1} \frac{(n-1)!}{(f-1)!(n-f)!}$$

TABLE 1. EXPECTED FREQUENCIES FOR EVENT A (E_A)
(REJECTION OF SUPPORTED POLICY)*

k \ n	3	4	5	6	7	8	9	10	11	12
1	.00	.00	.00	.00	.00	.00	.00	.00	.00	.00
2	.14	.07	.03	.02	.01	.00+	.00+	.00+	.00+	.00+
3	.43	.27	.16	.10	.06	.03	.02	.01	.01−	.00+
4		.47	.36	.25	.17	.11	.07	.05	.03	.02
5			.48	.41	.33	.25	.18	.13	.09	.06
6				.49	.45	.35	.32	.25	.19	.14
7					.50−	.47	.43	.37	.31	.25
8						.50−	.48	.46	.41	.36
9							.50−	.49	.47	.44
10								.50−	.50−	.49
11									.50−	.50−
12										.50−

* To associate values of E_A with values of k for a committee of size n, simply read down the vertical columns of the table. The table is asymmetrical since intersects below the diagonal are impossible ($k>n$). Rule of consensus values appear just on the diagonal ($k=n$). Rule of individual initiative values are in the top row ($k=1$), and majority rule values ($k>n/2$) are underlined.

will postpone our analysis of these expectations until they can be combined with those for B (E_B).

(B) Event B occurs whenever our generalized member (Ego) opposes a proposal (is *not* a member of its support set) and the proposal is nonetheless imposed (the set contains at least k members). We may now identify the set of sets which corresponds to event B:

Let: B represent the set of support sets which defines the occurrence of event B

Other notation remains as above.

Then:

$$B \approx [S_f, \text{ such that } f \geq k \text{ and Ego} \notin S]$$

This says simply that set of sets B contains all those support sets which are large enough to impose a policy under decision-rule k and which do not contain our generalized member (Ego). In short, Ego fails to block a policy proposal which his value schedule leads him to oppose.

Under the rule of consensus ($k = n$), set B must be empty, since no support set can equal or exceed k unless it contains our generalized member:

If $k = n$, B empty

For values of k below n, however, a somewhat more complex "counting" problem arises. It is resolved by the formula:[22]

[22] This formula is also derived from the combination rule. In this instance, we wish to "count" the support sets of size f which do not include Ego and for which f is at least k. We may begin by saying, "If Ego is not to be included in a set of size f, there will be $n-1$ members to be combined in sets of size f." We therefore subtract one from n, and leave f unaltered, obtaining:

$$\frac{(n-1)!}{f![(n-1)-f]!}$$

This gives the number of sets of size f which exclude Ego. We wish to sum the value of this fraction for all values of f at or above the value of k. Hence, the summation term:

$$\sum_{f=k}^{n-1}$$

The upper limit is set at $n-1$ for two reasons: (1) we know that no sets belonging to B may be of size n, and (2) this extreme case can produce a minus factorial term, which is nonsensical. The result is the formula given:

$$N_B = \sum_{f=k}^{n-1} \frac{(n-1)!}{f![(n-1)-f]!}$$

Let: N_B represent the number of support sets in B.

Other notation remains as above.

Then:

$$N_B = \sum_{f=k}^{n-1} \frac{(n-1)!}{f![(n-1)-f]!}$$

$$(\text{If } f = n,\ N_B = 0)$$

This formula "counts" the number of support sets which are at least large enough to impose a policy under any given decision-rule value (k) and which do not include Ego. This tells how many support sets correspond to event B, in which Ego's value schedule leads him to oppose a policy, yet it is imposed. If we divide this by the total number of support sets in a committee of n members, we may generalize it to the expected frequency for the occurrence of event B:

Expectation for Event B: Any member of a political committee of size n, operating under decision rule k, must expect that a proposal which he opposes will be imposed in:

$$\frac{N_B}{2^n - 1}$$

proportion of the instances in which a proposal comes to a vote. The full formula may be stated:

Let: E_B represent the expected frequency of event B.

Other notation as above.

$$E_B = \frac{\sum_{f=k}^{n-1} \frac{(n-1)!}{f![(n-1)-f]!}}{2^n - 1}$$

$$[f = n,\ E_B = 0]$$

Like its counterpart for event A, this statement is fully general. Table 2 gives the expected frequencies for event B (E_B) in committees of three to twelve members and decision-rules (k) of 1 to n.

It may be worth pausing to examine the shapes of the curves for the expected frequencies of events A and B (see Figure 1). Both are *monotonic:* the direction in which expected frequencies for events A and B change with k in a committee of any size does not change. The curves have no "humps" or "dips." Second, the expected frequencies for events A and B are *curvilinear:* the rate at which they change with k varies over the values of k. Notice that the rate of change for each curve is greatest around the middle of the k values (i.e., where $k = n/2$). This means, for example, that the expected frequency of event A increases most

TABLE 2. EXPECTED FREQUENCIES FOR EVENT B (E_B)
(IMPOSITION OF OPPOSED POLICY)

k \ n	3	4	5	6	7	8	9	10	11	12
1	.43	.47	.48	.49	.50−	.50−	.50−	.50−	.50−	.50−
2	.14	.27	.36	.41	.45	.47	.48	.49	.50−	.50−
3	.00	.07	.16	.25	.33	.35	.43	.46	.47	.48
4		.00	.03	.10	.17	.25	.32	.37	.41	.44
5			.00	.02	.06	.11	.18	.25	.31	.36
6				.00	.01	.03	.07	.13	.19	.25
7					.00	.00+	.02	.05	.09	.14
8						.00	.00+	.01	.03	.06
9							.00	.00+	.01	.02
10								.00	.00+	.00+
11									.00	.00+
12										.00

sharply as the decision-rule rises from slightly below $n/2$ to slightly above it, and that the rate of change is slowest near the extreme values of k, 1 and n. Third, the expected frequencies for the two events form curves which are perfect mirror images of each other; as intuition would lead one to expect, the curves are *opposed*. Last, both curves vary between *limits of zero and one-half:* no decision rule (k) makes either event more than an "even bet."[23] And, since no event can be both A and B—the two corresponding sets are disjoint—the summed frequency can never exceed half. Let us turn to the summed expected frequencies and an application of the choice criterion.

Evaluation of Decision-Rules under Constitutional Choice Model. We began by looking for a decision-rule which would optimize the expected correspondence between individual values and collective choices. Under such an optimal rule, a generalized member would expect to have a favored proposal defeated or a disfavored proposal imposed less often than under any other rule. This line of reasoning leads us to the choice criterion:

> One should choose that decision-rule which minimizes the sum of the expected frequencies for (A) in which the committee does not impose a policy which his value schedule leads him to support, and (B) in which the committee imposes a policy which his value schedule leads him to oppose.

Our model generates expected frequencies which confirm our intuitive suspicion that no location of the decision-rule will minimize the expected frequencies for both events A and B taken singly. This is because the two curves are opposed; as the expected frequency for A goes down, the expected frequency for B goes up, and *vice versa*. Indeed, the minimum for B is the maximum for A, and the minimum for A is the maximum for B. Under the rule of consensus ($k = n$), one can be sure that no proposal he dislikes will be imposed ($E_B = 0$), but he must also expect that his own proposals will often be defeated ($E_A = .5$). Under the rule of individual initiative ($k = 1$), our member can be sure that proposals he supports will always pass ($E_A = 0$), but he must also expect that many he opposes will also pass ($E_B = .5$). He cannot avoid being outvoted by choosing an extreme decision-rule.

But, as we said, the object is to minimize the *sum* of the expected frequencies for these two events. The optimal decision-rule must then be selected on the basis of the summed expected frequencies for events A and B. The 'best' rule will correspond to the point at which the sum of these frequencies is minimized for a committee of size n. Table 3 gives the summed frequencies in which we are interested.

The outcome of our search for an optimal decision-rule is apparent in these summed values: *majority rule corresponds uniquely to the minimum summed frequency of events A and B in committees with an odd number of members, and shares the minimum with the value n over 2*

[23] This upper limit occurs because there is only a .5 probability that Ego will vote yes (or that he will vote no.) These expected frequencies must not, then, be confused with the conditional probability that A or B will occur once Ego has voted yes or no.

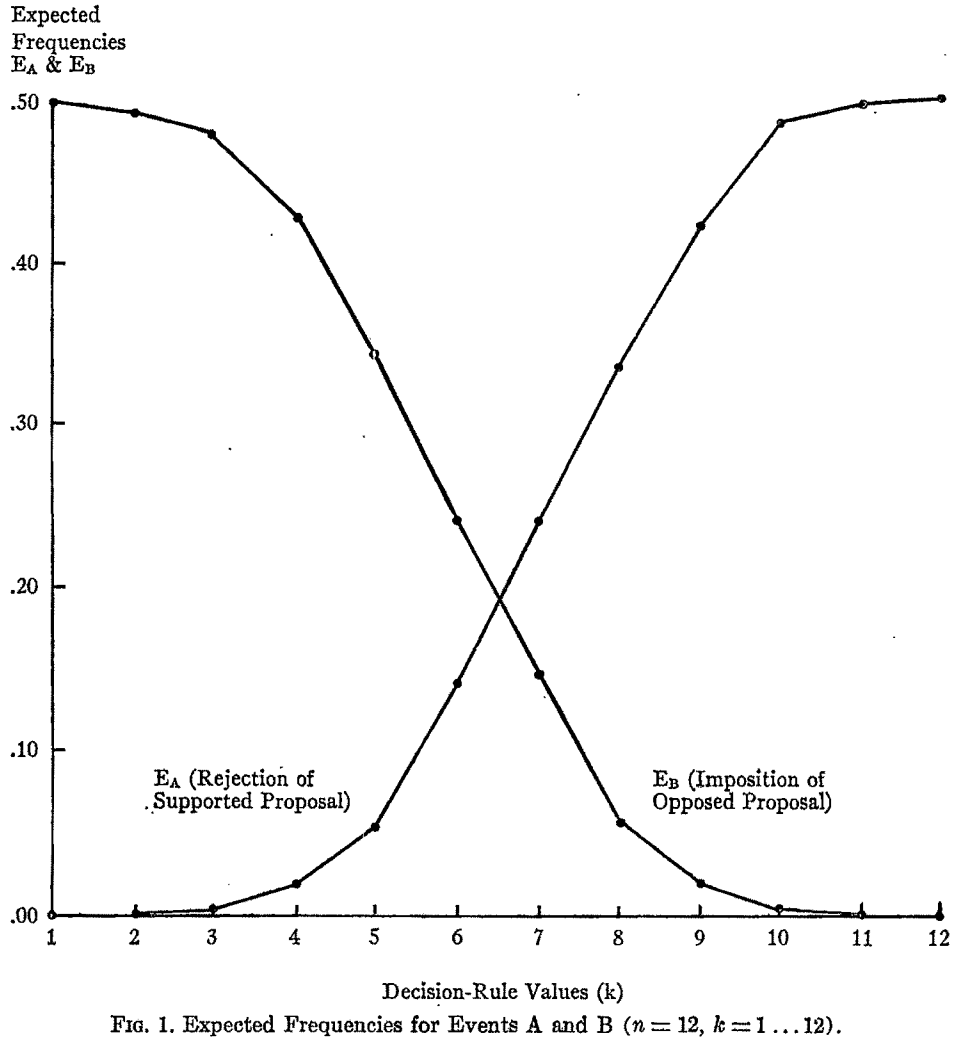


FIG. 1. Expected Frequencies for Events A and B ($n = 12$, $k = 1 \ldots 12$).

NOTE: These expected frequencies apply to long agenda, and may be equated with probabilities for single items on agenda. The reader may use the tables to draw curves for other values of n and k.

TABLE 3. EXPECTED SUMMED FREQUENCIES FOR EVENTS A AND B

k \ n	3	4	5	6	7	8	9	10	11	12
1	.43	.47	.48−	.49−	.50−	.50−	.50−	.50−	.50−	.50−
2	.28	.34	.39	.43	.46	.47+	.48+	.49+	.50−	.50−
3	.43	.34	.32	.35	.39	.38	.45	.47	.48	.48+
4		.47	.39	.35	.34	.36	.39	.42	.44	.46
5			.48	.43	.39	.36	.36	.38	.40	.42
6				.49	.46	.38	.39	.38	.38	.39
7					.50−	.47+	.45	.42	.40	.39
8						.50−	.48+	.47	.44	.42
9							.50−	.49+	.48	.46
10								.50−	.50−	.48+
11									.50−	.50−
12										.50−

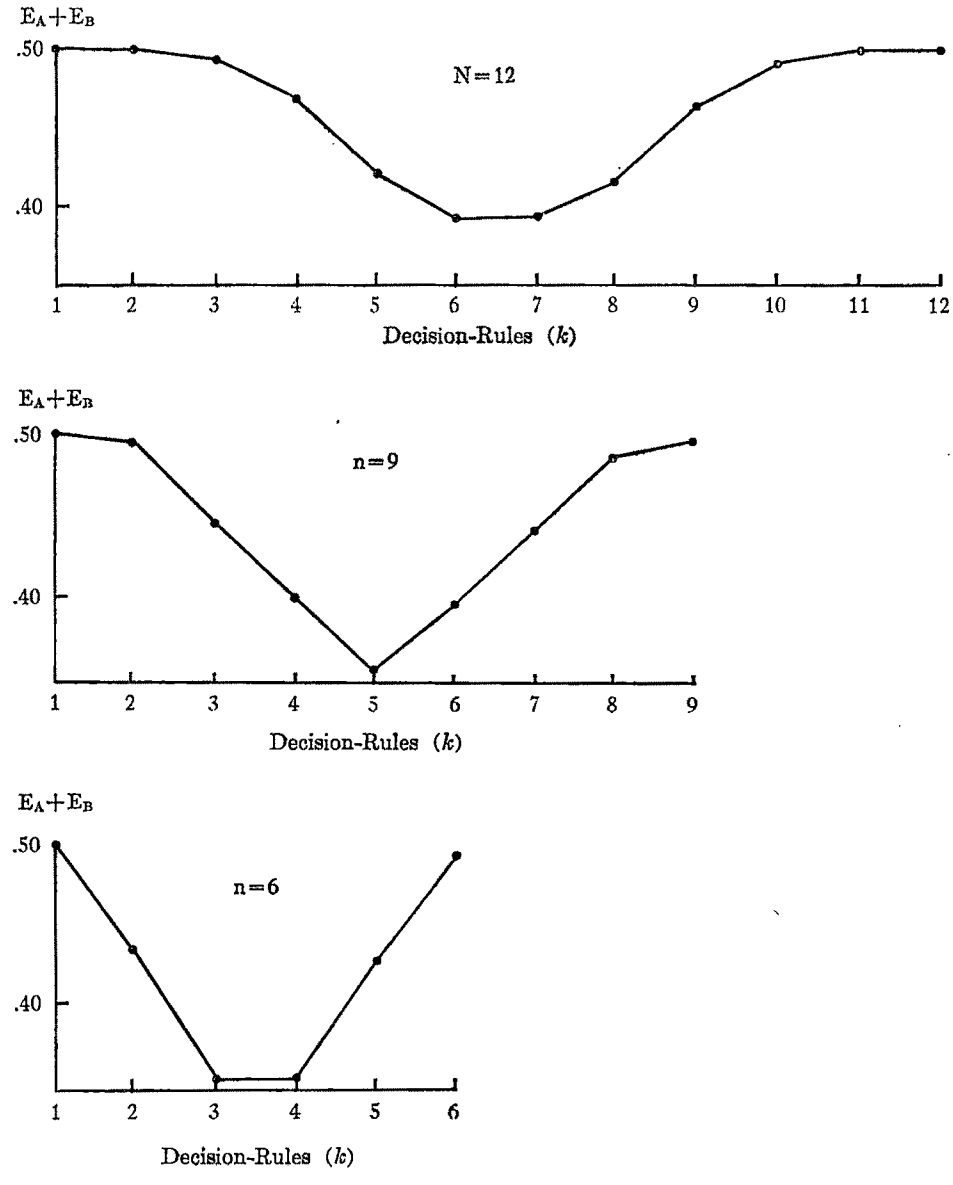


FIG. 2. Expected Summed Frequencies for Events A and B (n as indicated, $k = 1 \ldots n$).

in committees with an even number of members. If, under our model, one applies the choice criterion to the range of possible decision-rules, he will be compelled to plump for majority-rule or an adjacent value in every instance. As one proceeds toward the rule of consensus or toward the rule of individual initiative, the expected summed frequencies for events A and B increase. These relations may be seen graphically in Figure 2, and our findings may be stated as follows:

Proposition 1: In committees with an odd number of members, the optimal decision-rule is majority-rule: n over two, plus one-half.

$$k\ [\text{optimal}] = \frac{n}{2} + \frac{1}{2}, \text{ or } \frac{n+1}{2}$$

Proposition 2: In committees with an even number of members, the optimal decision-rule is either majority-rule, or majority-rule minus one: n over two plus one, or simply n over 2.

$$k\ [\text{optimal}] = \frac{n}{2} + 1, \text{ or } \frac{n}{2}$$

In so far as the model holds, these proposi-

tions are general, and our choice criterion requires that we select one of the values specified. If one confronts a choice similar to the one confronted in our model by Ego, and if he accepts the major tenets of the model itself, he will find no k value which improves on majority rule:

Proposition 3: In any political committee, majority-rule is as good (i.e., optimal) as any alternative decision-rule, given the model proposed here.

This assertion is, in a sense, more general than existing arguments for majority-rule, since it rests on a comparison with all of its alternatives. And, since the value premise is only an open preference for an optimal correspondence between individual values and collective choices, whatever the individual values may be, the argument may be somewhat less brittle than those which rest on more dogmatic and overarching premises.

The recommendation is also somewhat more general than our (admittedly restrictive) assumptions suggest. These assumptions are meant to represent the problem of "constitutional choice," not to maximize the generality of our conclusion. *Majority-rule is the one decision-rule which precludes the possibility that more people will be outvoted by less people.*[24] This accounts for the present conclusion, and it drives us to comparable judgement under any set of assumptions, so long as we can speak of a generic individual[25] (as we cannot in the "standing minority" problem considered in Part III below.) This generality is illustrated by the appendix to this paper and is proved by Michael Taylor in a forthcoming article.[26] It may, however, be more interesting to consider two sets of assumptions under which our conclusion does *not* apply.

III. SOME ALTERNATIVES

While this recommendation has a certain power of its own, we must remember that "models are models," and that we have quite consciously put aside important aspects of experience. The pure case of constitutional choice is probably a very rare empirical event, for the choosers of a decision-rule are apt to at least think they know enough about the future to make more determinate guesses than the ones we have made in our model. Let us examine some of the implications which follow from added knowledge about the future.

Let us suppose, first, that a member entertains a *positional* (as opposed to substantive) *preference,* which leads him to think bad actions (Event B) are worse than bad inactions (Event A). This might be so, for example, if he were a "conservative" in Samuel Huntington's sense of that term;[27] or if there were some *a priori* difference in the consequences of action and inaction, as with criminal juries in which Ego might consider a bad conviction worse than an objectionable acquittal. And, second, let us suppose that we may anticipate the formation of *factions* whose members possess identical preferences over some class of issues. If this is so, we must suppose that a "permanent minority" may arise and be exploited by a permanent (or rotating) majority. In the following paragraphs, we consider the effects of these guesses about the future on our recommendation for majority-rule.

Positional Preferences. Let us suppose that a member considers it worse for the committee to act against his wishes (Event B) than for it to refuse action in accordance with his wishes (Event A). It appears, for example, that a majority of delegates (including the articulate Madison) at the Philadelphia convention thought it would be worse if Congress expelled a member over a substantial number of objections than if it failed to do so under comparable circumstances.[28] Beliefs of this kind—usually related to specific classes of proposals—lead us to assign different "weights" to Events A and B. We may begin thinking about the consequences of these beliefs by defining the admittedly arbitrary quantities which correspond to these

[24] This is because all more restrictive rules allow negative minorities to outvote positive majorities, while less restrictive rules allow positive minorities to outvote negative majorities. Under all such rules, it is therefore possible that Event A or Event B will occur for more than half the members on a single vote: under majority-rule (and under $k = n/2$ in committees of even size) this is not possible.

[25] And so long as we do not consider "positional preferences," as discussed in Part III below.

[26] "A Proof for Rae's Majority-Rule Theorem," forthcoming, *Behavioral Science* (Summer, 1969).

[27] In "Conservatism as an Ideology." this REVIEW, Vol. 51, Huntington describes conservatism as a "positional ideology," leading its proponent to an ". . . articulate, systematic, theoretical resistence to change." (p. 461.) This suggests that a conservative would be apt to weight Event B (unwanted change) more heavily than Event A (unwanted stability).

[28] This is an extrapolation from James Madison's *Journal of the Federal Convention,* Vol. 2 (Chicago: Albert Scott and Co., 1893) p. 500. Since the premises of the argument are inexplicit, one cannot be sure that any interpretation is historically correct; the interpretation offered here is, however, at least as plausible as any alternative.

weights:

1. Let α represent the weight attached by an individual to Event A, failure to impose a proposal he likes.
2. Let β represent the weight attached by an individual to Event B, adoption of a proposal he dislikes.

We must note that these weights are conceptually distinct from the individual's preference on a given proposal. They rest instead on the assertion that, for all proposals within some class, an A outcome may be assigned a weight distinct from that assigned to a B outcome. Given these weights, our criterion of choice is no longer dependent upon the simple sum of the expected frequencies for Events A and B, but on the weighted sum of these frequencies. This revised value assumption recommends the decision-rule which minimizes the value of the weighted frequency:

The optimal rule minimizes: $\alpha \cdot E_A + \beta \cdot E_B$

Drawing the expected frequencies from our simple model (or some other model) and the weights by "fiat" from our hypothetical individual, we may now reevaluate a given set of possible decision-rules. Putting aside the enormous difficulty of supplying meaningful quantities for α and β, let us illustrate this alteration of our calculations for a seven-member committee. In this example, we assign weights of one and two to α and β. The question then becomes, "Do these weights justify the adoption of a decision-rule more restrictive than the majority?" On the assumption that the weights themselves make sense, we may answer this question with the weighted frequency term set forth above. In our example, we may answer this question by calculating this term for $n = 7$, $k = 1 \ldots 7$, $\alpha = 1$, and $\beta = 2$. The results are shown in the right-hand column of Table 4.

We see that a two-thirds (i.e., $k = 5$) rule is to be preferred in this instance over the simple majority (i.e., $k = 4$) and over all other alternatives. This illustrates the plausibility of restrictive rules where we can say that bad action is worse than bad inaction, and the "calculations" may very well be analogous to those which underlie the argument for such rules in, say, criminal juries or in bodies which decide upon proposals for constitutional revision. This is not to say that actors have literally made calculations like those given here, but rather that they may have thought the question out in less formal terms and relations similar to those considered here.

TABLE IV. "WEIGHTED" FREQUENCY CALCULATION
($n=7$, $k=1\ldots7$, $\alpha=1$, $\beta=2$)

k	α	E_A	$\alpha \cdot E_A$	β	E_B	$\beta \cdot E_B$	$\alpha \cdot E_A + \beta \cdot E_B$
1	1	.00	.00	2	.50	1.00	1.00
2	1	.01	.01	2	.45	.90	.91
3	1	.06	.06	2	.33	.66	.72
4	1	.17	.17	2	.17	.34	.51
5	1	.33	.33	2	.06	.12	.45
6	1	.45	.45	2	.01	.02	.47
7	1	.50	.50	2	.00	.00	.50

This brings us to the suggestion that the simple model presented in Part II is a special case of this more general model: it is the case in which α equals β. In a formal sense, this is self-evident, but we should remember that it is an especially interesting case, for the assumption of equal weights is in general more plausible than any alternative. This is especially evident for political committees whose agenda are open to all types of proposals and which confront dynamic environments which must be expected to undermine any special argument for the *status quo*. In the absence of special evidence to the contrary—like that cited for juries—it seems prudent to accept the equal-weighting case.

Factions. Let us put the problem of factions as bluntly as possible.[29] Suppose that we know enough about the committee and its long-term agenda to say that there will, for some class of proposals, be two factions, one of which will contain a majority and one of which will contain a minority of the members. And, suppose further, that we can expect a conflict of interest[30] between these factions for some class of issues, so that their preferences will be opposed on each proposal within that class. If we assume all this —as we might in some important cases—then we confront a bald case of the classic difficulty in majoritarian democracy: the exploitation of standing minorities by standing majorities. In our model, Assumption I sheltered us from this prospect, and we may now consider its implications briefly. To begin with, say that:

1. A majority faction contains q members ($q > n/2$).
2. A minority faction contains $n - q$ members ($< n/2$).

[29] The following example states the problem in the simplest possible way, and it is just one of many sorts of examples which might be devised. If one considers more than two factions, and partial degrees of conflict, the argument is vastly more complex, but the essential conclusion of this section is probably unaffected: the prospect of factional exploitation is not a unique attribute of majority-rule.

[30] One useful analysis of this concept is offered by Robert Axelrod, *Conflict of Interest: Theory and Political Applications* (unpublished Ph.D. thesis, Yale University, 1968).

3. These two groups have opposed preferences for all issues within the class to be considered.

If we consider a "random" individual, one with q/n chances of belonging to the standing majority, it is obvious that majority rule is at least as desirable as any alternative. This, however, avoids the problem, so we must consider two discrete individuals, one a member of the standing majority and one a member of the standing minority. As Figure 3 suggests, there are only three classes of decision-rules which need concern either of these individuals: (1) those which require no more votes than the number commanded by the minority $[k \leq (n-q)]$; (2) those which require more votes than the number commanded by the minority but no more than are commanded by the majority $[k > (n-q),\ k \leq q]$; and (3) those which require more votes than are commanded by the majority $(k > q)$. Given our assumption that the members of these two factions disagree for all proposals within the class considered (3, above), we have only these three kinds of rules to consider.

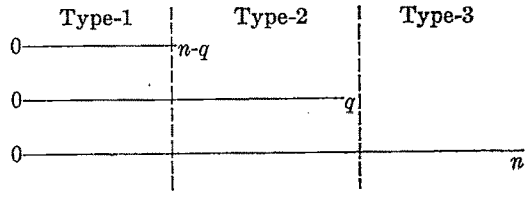


FIG. 3. Three classes of decision-rules for a committee with exactly two consistently opposed factions.

For a member of the standing majority, it is obvious that Type-2 decision-rules are preferable to types 1 and 3. Under Type-2 rules—majority-rule is an example—this member will expect to have his way on all issues belonging to the class considered here. This is because the majority faction can impose all proposals its members like $(E_A = 0)$, and veto all proposals its members dislike $(E_B = 0)$. Under Type-3 rules, however, he must expect not to have his way on proposals which he and his majority fellows like, for the opposed minority will surely veto them (E_A = the proportion of proposals he likes). And under Type-1 rules, he must expect not to have his way on proposals he dislikes, since the minority will certainly impose them (E_B = the proportion of proposals he dislikes). We would therefore expect the majority member to prefer decision-rules of Type-2—majority-rule, for example.

In contrast, we must expect a minority member to prefer either Type-1 or Type-3 rules, and not Type-2 rules. This is obvious, since Type-1 allows him to impose policies he likes $(E_A = 0)$, and Type-3 rules allow him to veto policies he dislikes $(E_B = 0)$, while Type-2 rules allow him neither of these opportunities. And, given our assumptions, there is no possibility of compromise—save the adoption of different rules for various sub-classes of issues.

This bald example of the problem raised by standing majorities and minorities leads to the conjecture that *the problem of the exploited faction is not a special difficulty of majority-rule, but a generic difficulty of all simple-decision-rules*. This is so because *any* rule will leave either the minority or the majority subject to one of three unhappy prospects: the unchecked imposition of unwanted policies; or the invariable veto of wanted policies; or both. It may, of course, be said that Type-1 and Type-3 rules are less open licenses to exploitation than Type-2 rules, since they at least open the prospect of strategic bargaining by the minority. Yet it is hard to imagine the majority which would willingly accept such rules.

Even this much-too-brief consideration of factional conflict suggests that the effort to optimize individual values in collective choice must, in such instances, look beyond the choice of simple decision-rules. No such rule will produce a joint optimization (as did majority-rule under the simple model). Separatism—be it complete, as with the Pakistani withdrawal from India, or partial, as in federal and confederate constitutions—may well be the only prospect for a joint optimization through constitutional choice. Still, separation itself raises the serious prospect that neither faction will, in the long run, fare very well.[31] Admitting that this consideration of the problem is far from exhaustive, I suspect that one must look away from formal institutional choices and toward the standard civic virtues if individual values are to be optimized in factional strife.

IV. CONCLUSIONS

We arrive at three main conclusions. First, majority rule can be expected to optimize the correspondence between individual values and collective choice under the assumptions of our

[31] In the terms of this analysis, we might say that partition introduces a special class of events in which policies which Ego favors, and which would have been adopted before the partition, are rejected because the required resources are no longer available. These events are analogous to Event A—the rejection of proposals which Ego favors. These events are singularly relevant to proposals for separation of minorities, as with French Quebec or Black America, since the minority communities would operate with vastly restricted resources.

simple model. Only under a model of this kind can we expect unanimous *a priori* agreement on decision-rules, for the introduction of other information produces determinate conflicts of interests—as illustrated by our second and third conclusions. The second conclusion is that positional preferences constitute a logically legitimate basis for the choice of non-majoritarian decision-rules. It follows that, if there is a conflict over positional preferences, there must also be a conflict over decision-rules, even if other assumptions are unaltered. Third, if we consider the "bald case of factional conflict," it appears that the problem of the exploited faction is general to all simple decision-rules, and not a special property of majority rule. In this case, we cannot expect factions to agree on any given decision-rule.

These conclusions lead to one last conjecture: a constitutional choice can be made consensually only if uncertainty about the future makes appropriate a model analogous to our simple one. Once decision-rules are conceived as instruments in a determinate conflict of interests, then the incentive for agreement on majority-rule (or any alternative to it) is destroyed, and unanimous consent seems improbable. There may be many ways to obtain agreement on rules in the face of disagreement on issues, but a self-conscious ignorance of the future is probably not the least important among them.

APPENDIX

A mathematical proof devised by Michael Taylor demonstrates that the outcome of the simple models—the optimality of majority-rule—depends only on Assumption I and not on Assumptions II and III.[32] This means that the recommendation is general to *all* probabilities for yes votes, regardless of whether or not we take "non-proposals" opposed by all members into account. In effect, Taylor's proof suggests that Assumptions II and III are theoretically trivial, having simplicity of exposition as their sole purpose. This may be illustrated here by giving a few computational examples which violate Assumptions II and III but nevertheless lead to confirmations of Propositions 1, 2, and 3 (the optimality of majority-rule). In each, we disregard Assumption III and give a specific alternative to Assumption II.

example one:

Consider the following alternative to Assumption II:

$$P_X = \frac{1}{3},\ P_z = \frac{2}{3}.$$

[32] Taylor, *op. cit.*

Under our new assumptions, there are eight possible support sets in a committee of three (one with three members, three with two, three with one, and one empty). Using the probabilities assigned under this alternative we calculate:

$$P_{XYZ} = \frac{1}{3} \times \frac{1}{3} \times \frac{1}{3} = \frac{1}{27}$$

$$P_{YZ} = P_{XZ} = P_{XY} = \frac{1}{3} \times \frac{1}{3} \times \frac{2}{3} = \frac{2}{27}$$

$$P_X = P_Y = P_Z = \frac{1}{3} \times \frac{2}{3} \times \frac{2}{3} = \frac{4}{27}$$

$$P_{empty} = \frac{2}{3} \times \frac{2}{3} \times \frac{2}{3} = \frac{8}{27}$$

$$\left[\frac{1}{27} + \frac{(3 \times 2)}{2} + \frac{(3 \times 4)}{27} + \frac{8}{27} = 1\right]$$

As before, we may define Events A and B as sets of support sets, and, letting X (or Y or Z) represent Ego, we may calculate the expected frequencies for these events:

Decision-Rule (k)	*Expected Frequency Event A*	*Expected Frequency Event B*	*Sum of A & B*
1	0	$\frac{10}{27}$	$\frac{10}{27}$
2	$\frac{4}{27}$	$\frac{2}{27}$	$\frac{6}{27}$
3	$\frac{8}{27}$	0	$\frac{8}{27}$

Notice that the summary frequency for the two events is no longer symmetrical, but, more important, notice that the minimum for that curve remains at $k=2$, and our finding is unaltered. This is not changed if we reverse the probability assumption, saying that members are twice as apt to support as to oppose each proposal; the curves rotate 180°, but the minimum has the same value and the same location. Or, if we return to Assumption III, barring empty support sets from occurrence, the devisor in each fraction becomes 19 instead of 27, but the location of the A+B minimum is unaltered.

example two:

Now consider an extreme case, in which each of three members is nine times as apt to support as to oppose any proposal:

$$P_X = \frac{9}{10},\ P_z = \frac{1}{10}.$$

This activist committee produces support sets with the following probabilities:

$$P_{XYZ} = \frac{9}{10} \times \frac{9}{10} \times \frac{9}{10} = \frac{729}{1000}$$

$$P_{XY} = P_{XZ} = P_{YZ} = \frac{9}{10} \times \frac{9}{10} \times \frac{1}{10} = \frac{81}{1000}$$

$$P_X = P_Y = P_Z = \frac{9}{10} \times \frac{1}{10} \times \frac{1}{10} = \frac{9}{1000}$$

$$P_{empty} = \frac{1}{10} \times \frac{1}{10} \times \frac{1}{10} = \frac{1}{1000}$$

$$\left[\frac{729}{1000} + \frac{(3 \times 81)}{1000} + \frac{(3 \times 9)}{1000} + \frac{1}{1000} = 1\right]$$

Expected Frequencies for Events A and B may be computed from these probabilities under our set definitions, and the result is:

Decision-Rule (k)	*Expected Frequency Event A*	*Expected Frequency Event B*	*Sum of A & B*
1	0	$\frac{99}{1000}$	$\frac{99}{1000}$
2	$\frac{9}{1000}$	$\frac{81}{1000}$	$\frac{90}{1000}$
3	$\frac{171}{1000}$	0	$\frac{171}{1000}$

The minimum expected frequency for Events A and B still falls at $k=2$. Admittedly, under these extreme assumptions, it is only slightly lower than the value for $k=1$, but its location is nevertheless stationary. Notice too, that the range of values for this summed frequency is quite low for all three values of k: our activist assumption has placed most of the members on the winning side of most issues (i.e., in 730 of 1000 instances, all three would vote together). Turning this activist committee into a meeting of moss-backs (i.e., reversing the assumption) simply reverses the curves, leaving their shapes and the location of the A+B minimum where it was under Assumption II. And returning to Assumption III simply reduces the denominator in each fraction to 999 instead of 1000.

example three:

As a last example, let us consider an absurd extreme, in which members support proposals with a probability of .999, and oppose them with a probability of .001. Here, we obtain:

$$P_{XYZ} = \frac{997,002,999}{1,000,000,000}$$

$$P_{XY} = P_{XZ} = P_{YZ} = \frac{998,001}{1,000,000,000}$$

$$P_X = P_Y = P_Z = \frac{999}{1,000,000,000}$$

$$P_{empty} = \frac{1}{1,000,000,000}$$

$$\left[\frac{997,002,999}{1,000,000,000} + \frac{(3 \times 998,001)}{1,000,000,000} + \frac{(3 \times 999)}{1,000,000,000} + \frac{1}{1,000,000,000} = 1\right]$$

Decision-Rule (k)	*Expected Frequency Event A*	*Expected Frequency Event B*	*Sum of A & B*
1	0	$\frac{999,999}{1,000,000,000}$	$\frac{999,999}{1,000,000,000}$
2	$\frac{999}{1,000,000,000}$	$\frac{998,001}{1,000,000,000}$	$\frac{999,000}{1,000,000,000}$
3	$\frac{1,997,001}{1,000,000,000}$	0	$\frac{1,997,001}{1,000,000,000}$

It should be clear from these examples that the optimum is stable, at least for very small committees. Taylor's forthcoming proof generalizes this stability to all alternatives for Assumptions II and III, for committees of all sizes. It is quite clear that the degree to which majoritarian decision-rules are superior to their alternatives declines with size[33] and with the asymmetry of the probability assumption, but the nominal fact of their optimality remains in all cases.

[33] This suggests an incentive for the formation of minimum winning coalitions, even if we assume that preferences are independently arrived at so that there is no *a priori* basis for group cohesion. This is because the expected frequency for events A and B is lower, for any given k, in small committees than in large committees. By forming a coalition of k members, and operating under the analogous decision-rule internally, members can always have their way slightly more often than they would by voting independently. In a committee of five, with independent voting, members can expect to have their way 11/16ths of the time if $k=3$; by forming a coalition of three, however, it is possible to raise this expectation to 12/16ths, if $k=2$ is adopted by the coalition. This suggestion is not taken into account by our simple model, which is based on sincere individual voting. This suggestion is an interesting parallel to William Riker's prediction of minimum winning coalitions on the basis of a quite different model. See *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962.)

THE PARTY VARIABLE IN JUDGES' VOTING: CONCEPTUAL NOTES AND A CASE STUDY*

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I. INTRODUCTION

Since the writings of the judicial realists in the 1920's and 1930's almost all social scientists have accepted the concept that the judicial process is basically political. One line of reasoning points out that judges inevitably are policy makers because of their functions, such as constitutional and statutory interpretation. Politics is conflict among interests or values or demands, and any activity, including judicial decisions, which advantages one interest as opposed to others is political activity. Since judges decide cases involving interest conflicts, theirs is "interest activity not as a matter of choice but of function."[1] Robert H. Jackson made the point cogently when he argued that the "Supreme Court has, from the very nature of its functions, been deep in power politics . . ."[2] Of the power of judicial review, he said, "The ultimate function of the Supreme Court is nothing less than the arbitration between fundamental and ever-present rival forces or trends in our organized society."[3] Although these remarks were addressed to the Supreme Court, they are true to a substantial degree of judicial power throughout the American system. Many of the conflicts adjudicated by the judiciary advance some interests or values and disadvantage others.

At the same time that the judicial function has been recognized as political in its consequences, the process of judicial decision making has come to be viewed as involving wide opportunities for discretion by the judges. The mechanical theory of judging, enunciated most clearly by the late Justice Owen Roberts,[4] is now generally in disrepute, and realists prefer to cite Charles Evans Hughes who stated "We are under the Constitution, but the Constitution is what the judges say it is."

This view of the judicial process has led in recent years to an exhaustive search for determinants of judges' conduct. The importance of the value preferences of the justices is commonly noted. Judicial conduct may be affected by forces such as the litigants, interest groups which sponsor test cases and file amicus curiae briefs,[5] law professors, law reviews, the bar associations, community groups which bestow honors and recognitions,[6] and even law clerks, although their influence is hotly denied.[7] The judicial role as judges perceive it may modify and guide their conduct. The selection process may also shape judicial decision making by installing in judicial office men of certain kinds of philosophic and policy outlooks.[8] Certainly the thrust of Robert Dahl's analysis of the Supreme Court

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[1] Jack Peltason, *Federal Courts in the Political Process* (New York: Random House, 1955), ch. 1, esp. p. 3.

[2] Robert H. Jackson, *The Struggle for Judicial Supremacy* (New York: Random House-Vintage, 1941), p. vii.

[3] *Ibid.*, p. 311.

[4] *United States v. Butler*, 297 U.S. 1, 79 (1936).

[5] Clement E. Vose, "Litigation as a Form of Pressure Group Activity," *The Annals of the American Academy of Political and Social Science*, 319 (September, 1958), 20–31; and his "Interest Groups, Judicial Review, and Local Government," *Western Political Quarterly*, 19 (March, 1966), 85–100. The Court's reaction in 1949 to the excessive use of amicus briefs is discussed in Fowler V. Harper and Edwin D. Etherington, "Lobbyists Before the Court," *University of Pennsylvania Law Review*, 11 (1953), 1172–1179. See also Samuel Krislov, "The Amicus Curiae Brief: From Friendship to Advocacy," *Yale Law Journal*, 72 (1963), 694–721.

[6] On these points, see Jack Peltason, *op. cit.*, pp. 22–25, for a review of some of the sources and findings.

[7] Various views on the influence of law clerks in the decisional process of the United States Supreme Court are presented in Glendon Schubert, *Constitutional Politics* (New York: Holt, Rinehart and Winston, 1964), pp. 138–146.

[8] John Schmidhauser, *The Supreme Court: Its Politics, Personalities and Procedures* (New York: Holt, Rinehart and Winston, 1963), chs. 2–3.

as a national policy-maker is that eventually, through the process of appointment by the President and confirmation by the Senate, the Court is brought into line on policy matters with the long-term law-making majority representing the fairly stable popular majority coalition in the country.[9]

There has developed, in this search for determinants of judicial conduct, a growing and oft-cited body of literature linking the party affiliations of judges to their votes in cases before them.[10] This literature has invited an increasing number of generalized statements that party is a significant variable in judicial behavior. It is the purpose of this paper to comment briefly on the various conceptualizations of partisanship as a determinant of judicial behavior, to replicate with Wisconsin data the studies which seem conceptually sound, and to suggest additional factors which might modify the tendency to generalize the hypothesis that party identification per se is an influence on judges' behavior.

II. THE PARTISAN VARIABLE: REVIEW AND COMMENT

Glendon Schubert found through simple roll call analysis a sharp cleavage along party lines on the Michigan Supreme Court on the issue of workmen's compensation claims, with Democratic justices far more friendly to workers' claims than their Republican brethren.[11] For the 1954–1957 Terms of the Court, he also found clear evidence of cohesive dissenting blocs drawn along party lines.[12] In a subsequent study relying primarily on similar roll call analysis and on modified scalogram techniques, S. Sidney Ulmer affirmed a sharp division of Michigan Supreme Court justices along party lines on the issues of workmen's and unemployment compensation for the 1958–1960 Terms of that Court.[13] He concluded that "Democratic justice is more sensitive to the claims of the unemployed and the injured than Republican justice."[14]

Stuart S. Nagel studied the party affiliations and voting records of state and federal high court judges in fifteen areas of public and private law for the year 1955.[15] He computed a "decision score" for each judge on those high courts whose members were divided, by previous affiliation, between adherents of the two major parties. The "decision score" was the percentage of votes in each area cast for the "liberal position" which he defined as "the viewpoint associated with the interests of the lower or less privileged economic or social groups in one's society and (to a less extent) with acceptance of long-run social change . . ."[16] Thus, for instance, votes for the defense in criminal cases, the administrative agency in business regulation cases, the claimant in unemployment cases, the wife in divorce settlement cases, and the employee in employee injury cases were regarded as "liberal" votes. On this basis, Nagel found that, to a statistically significant degree, Democratic judges cast more votes for the "liberal" position than their Republican colleagues in nine of the fifteen areas of law analyzed.

The correlation between prior party affiliation and judicial voting was found to be less significant in states with appointive high courts than in states in which high court judges were elected. Among the elected judges, however, the length of term (shorter or longer than eight years) and the format of the ballot (partisan or nonpartisan) did not affect the relationship between party affiliation and judges' policy preferences.[17]

Sheldon Goldman[18] used techniques similar to Nagel's. His study of voting by U.S. Court of Appeals judges from 1961 to 1964 led him to conclude that Democrats on these courts "appear relatively more 'liberal' than Republicans in cases which involve what might be called 'economic liberalism.' "[19] He did not find a rela-

[9] "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," *Journal of Public Law*, 6 (1957), 279–295.

[10] Glendon Schubert, *Quantitative Analysis of Judicial Behavior* (Glencoe, Illinois: The Free Press, and East Lansing, Michigan: Michigan State University Bureau of Social and Political Research, 1959), pp. 129–142, esp. pp. 132–134. S. Sidney Ulmer, "The Political Party Variable on the Michigan Supreme Court," *Journal of Public Law*, 11 (1962), 352–362. Stuart S. Nagel, "Political Party Affiliation and Judges' Decisions," this REVIEW, 55 (December, 1961), 843–850. Sheldon Goldman, "Voting Behavior on the United States Courts of Appeals, 1961–1964," this REVIEW, 60 (June, 1966), 374–383. A study of both regional and party background variables at a much earlier period in American history is John Schmidhauser's "Judicial Behavior and the Sectional Crisis of 1837–1860," *Journal of Politics*, 23 (November, 1961), 615–640.

[11] Schubert, *op. cit.*, pp. 132–133.

[12] *Ibid.*, pp. 135–142.

[13] Ulmer, *op. cit.*, pp. 352–362.

[14] *Ibid.*, p. 362.

[15] Nagel, *op. cit.*, pp. 843–850.

[16] *Ibid.*, p. 846.

[17] *Ibid.*, pp. 848–849.

[18] *Op. cit.*

[19] *Ibid.*, p. 381.

tionship between party affiliation and judges' voting in civil liberties or criminal cases.[20]

These studies raise a major conceptual question: what is the link between party and judicial conduct? Most of the studies show nothing more than that certain decisional propensities are associated with the characteristic of prior party affiliation. While it is useful to know of this association, both Schubert[21] and Grossman[22] have recently warned that background characteristics are an inadequate explanatory device because they ignore the existence of a range of other factors which operate upon the judge.

Political scientists have developed no evidence that party operates in the judicial process through the formal mechanisms common in other branches of government—through legislative caucuses or the leadership of executives, for instance. The link between party and decision-making need not rest on such devices, however. Party identification and the socializing process of party activism may impart issue orientations which affect the decisions made by officeholders, including judges.

Nagel has suggested that party affiliation and judges' votes may "correlate with each other because they are frequently effects of the same causes," and he argues that the common causes are the "personal standards of value" held by judges.[23] This view ignores the role that party identification and party activism play in developing issue orientations. Although recognizing that the recruitment process may tend to bring into a party those who agree with its principles or values, Fred I. Greenstein and Elton F. Jackson argue that party affiliation may shape values and thus influence voting in the legislature.

> . . . party affiliations often are acquired early in the individual's life, prior to the formation of attitudes on issues; they are extremely stable; and they evidently are a major influence on issue and candidate evaluations. But when voters are asked to explain their electoral choices, they normally refer to issues and candidates rather than party. It is likely that party identification has similar effects on legislative voting behavior.[24]

Their comments may be pertinent to judicial behavior as well.

If judges are party identifiers before reaching the bench, there would be a basis for believing that they—like legislators—are affected in their issue orientations by party. Nagel has shown that 271 of the 304 justices of state high courts in 1955 had clear prior party affiliations.[25] Furthermore, judges are generally well educated and the vote studies show that the more educated tend to be stronger party identifiers, to cast policy preferences in ideological terms, to have clearer perceptions of issues and of party positions on those issues, to have issue attitudes consistent with the positions of the party with which they identify, and to be more interested and involved in politics. For judges, even more than for the general population, party may therefore be a significant reference group on issues.

Party activism is an even greater force than party identification in shaping policy orientations, and the available evidence indicates that judges have usually been party activists before taking the robe. Schmidhauser shows that United States Supreme Court justices have, with only a single exception, held some political post before going to the bench and that most of them were active in highly partisan forums, such as the state and national legislative and executive branches.[26] The study by Mott *et. al.*, which is now regrettably out of date, shows that in a thirty year period most judges of the highest courts in thirty-two states had prior experience in public legal offices, public administrative offices or legislative offices, most of them of a kind filled by appointment or election on a partisan basis.[27]

[20] *Ibid.* He hedges this finding somewhat by pointing out that his data permit an interpretation which suggests that "on the really 'important' cases that force the judges to stand up and be counted," the Democrats have higher overall liberalism scores and that these might include a higher degree of liberalism on some civil liberties and criminal matters.

[21] Glendon Schubert, *Judicial Policy-Making* (Chicago: Scott, Foresman and Co., 1965), p. 113.

[22] Joel B. Grossman, "Social Backgrounds and Judicial Decisions," *Journal of Politics,* 29 (May, 1967), 334–351, esp. pp. 344–347.

[23] Nagel, *op. cit.,* p. 847.

[24] Fred I. Greenstein and Elton F. Jackson, "A Second Look at the Validity of Roll-Call Analysis," *Midwest Journal of Political Science,* 7 (May, 1963), p. 161.

[25] Stuart S. Nagel, "Unequal Party Representation on State Supreme Courts," *Journal of the American Judicature Society,* 45 (1961), 62–65.

[26] John Schmidhauser, "The Justice of the Supreme Court—A Collective Portrait," *Midwest Journal of Political Science,* 3 (February, 1959), p. 37.

[27] Rodney Mott, Spencer D. Albright and Helen Semmerling, "Judicial Personnel," in Glendon Schubert (ed.), *Judicial Behavior: A Reader in*

McClosky has shown that on public issues political activists divide more sharply along party lines than do the voters as a whole, and he argues that party plays an important socializing role which affects the policy orientations of activists.[28] Party activism leads to a greater party spirit, to a greater sense of belonging to and identifying with a party, and thus very often establishes a motivation to accept the doctrines common in the party and to learn how those doctrines differ from the doctrines of the opposition party.[29] Furthermore, "since politics is more salient for leaders than for followers, they develop a greater stake in the outcome of the political contest and are more eager to discover the intellectual grounds by which they hope to make victory possible."[30] Also, a belief in party doctrine may serve as a self-justification for political activity; it casts the commitment of time, money, and energy in terms of serving significant social goals.[31] Finally, the number of political activists is small and their interpersonal contacts are almost entirely with those who agree with the party's doctrines and do so articulately, thus developing or greatly re-enforcing commitment to party policy orientations within the group.

It seems likely that party identification is one of the psychological dispositions through which judges view issues before they go to the bench. This disposition is strengthened by the socializing process of their situational circumstances, especially their party activity. The query remains whether other psychological dispositions and socializing processes become important in the new situational circumstance, that of being a judge, which significantly diminish the importance of party in making policy choices.

At the outset, judges may perceive their role on the bench as quite different from that of party activist. Their understanding of the "rules of the game" may be such as to make party an improper consideration in decision-making. They may become responsive to new reference groups —such as the bar and the press—which hold the same view. The socializing influences of party may be weakened for the judge if he is isolated from party activists by his judicial duties. In addition, there are the new socializing influences of work in a collegial court; the expectations of the other members may alter the attitudes of the new judge. Also, there is the influence of task requirements, the need to get the court's work done, which may cause men to act in a way quite distinct from the ways in which they acted when the job of task fulfillment involved party activists and had quite different objectives. Finally, there is the nature of judicial business; the complexity of the concrete issues which are the ordinary business of the highest state courts may make applications of party-shaped policy orientations less clear and may introduce other sets of values for consideration.

An understanding of the judge's situation then becomes quite important in understanding the relationship between party and decisional propensity. His situation might continue to include a high degree of contact with party. This could easily occur in the selection, tenure and promotion process. Indeed, it is argued in Section VI of this paper that the degree of partisan influence in their constituencies is a significant variable in explaining the sharply different conduct of Wisconsin and Michigan supreme court justices in deciding cases. Furthermore, the effects of membership in a collegial court may depend on whether justices have extensive personal contacts in the disposition of their work. Section VII of this paper suggests that the existence of such contacts may make the conduct of Wisconsin justices less partisan than that of Michigan justices.

In summary, the effect of party on judicial behavior depends on an interplay of factors. Foremost is a complex of psychological factors, including party identification and perception of the judicial role, which shape judges' policy orientations. The socializing processes which result from situational circumstances—party activism and judicial work are especially pertinent in the present study—may alter or re-enforce policy orientations, either strengthening or diminishing the significance of party in judges' psychological sets. The situational circumstances may vary significantly from state to state and these socializing processes depend therefore on the nature of the circumstances in each jurisdiction.

In addition to the conceptual question of the link between party and judicial behavior, the studies of the party variable in judicial decision-making raise an important methodological

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Theory and Research (Chicago: Rand McNally, 1964), pp. 197, 200.

[28] Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," this REVIEW, 44 (June, 1960), 406–427. Thomas A. Flinn and Frederick M. Wirt, "Local Party Leaders: Groups of Like Minded Men," *Midwest Journal of Political Science*, 9 (February, 1965), 77–98. Also, Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964), pp. 184–185.

[29] McClosky, *et al., op. cit.*, p. 421.

[30] *Ibid.*

[31] *Ibid.*

problem: what test should be applied to judicial conduct to infer with reasonable certainty the existence of party influence? Students of legislative behavior have generally applied tests which emphasize the extent to which members of the same party vote together as a bloc. In his pioneering work on party influence in legislative bodies A. Lawrence Lowell described a "party vote" as one "in which more than nine-tenths of those of its members who voted were on the same side of the question."[32] Similarly, Stuart Rice's widely used index of cohesion measures the extent to which all members of a group vote together on an issue.[33]

To isolate issues on which party influence is significant Rice also developed his index of likeness between groups (often called the index of party likeness or IPL) which shows the extent to which members of different groups vote differently.[34] The familiar *Congressional Quarterly* test of party voting requires that a majority of voting Democrats oppose a majority of voting Republicans on an issue.[35]

The major studies of partisan influence in legislative bodies have employed these indexes, all of which emphasize bloc voting by party members either within a single party or by contrast between the two parties.[36] While some objection has been raised to the undiscriminating use of roll call data, especially to the IPL which might mask party influences where the leaders of both parties are in agreement or friendship influences where the party leaders take no positions,[37] it seems clear that such data, carefully used and qualified, can be helpful in suggesting the influence of party on legislative behavior.[38]

The recorded votes of judges in cases before their courts provide roughly the same kind of data that roll calls provide for the study of legislative bodies. Furthermore, the same criterion seems relevant for inferring party influence: that is, the extent to which judges of each party vote together as distinct blocs in opposition to one another. Where party identification is largely coterminus with a voting bloc, there is a strong likelihood that party plays a significant role in the voting. Research beyond the voting records may, of course, reveal other factors at work in both judicial and legislative bodies.

Although Schubert and Ulmer present some of their data in simple roll call type analysis (e.g. the percentage of votes by each judge for the worker in workmen's compensation cases), they also study the Michigan Supreme Court with scalogram and bloc analysis techniques. As indicated above, their findings show sharp cleavages on that bench between Democrats and Republicans acting as separate blocs.

Nagel and Goldman, on the other hand, employ techniques which, in the opinion of this author, yield far weaker inferences of party influence. Both regard a "liberal vote" by a Democratic judge in a given case as related to his party, even though other Democratic judges on the same bench in the same case are voting for a contrary disposition of the matter. If party affiliation were the primary variable, it should affect most or all of the members of the same party in approximately the same way. Where judges of the same party differ to a significant extent on the disposition of issues before their court, it is as plausible to infer that party, as previously conceptualized, is not a variable in the judicial process as it is to infer that a "liberal" or "conservative" vote by a judge is linked to party. In the face of equally plausible inferences, judges' conduct ought not to be explained by the party variable.

III. A WISCONSIN CASE STUDY: SETTING AND METHOD

The testing of the party variable with additional data seemed necessary since the Schubert and Ulmer studies showing with reliable techniques strong evidence of party influence in judicial voting rested on data from only a single state, Michigan. The use of Wisconsin data for this retesting was dictated by several factors. First was the impressionistic evidence that in

[32] "The Influence of Party Upon Legislation in England and America," *Annual Report of the American Historical Association,* 1901 (Washington: Government Printing Office, 1902), Vol. 1, p. 323.

[33] *Quantitative Methods in Politics* (New York: Alfred Knopf, 1928), p. 209.

[34] *Ibid.,* pp. 210–211.

[35] See *Congressional Quarterly Weekly Report,* No. 49 (1966) p. 2989 for an explanation of the technique which CQ has consistently used in its congressional voting analyses.

[36] See Julius Turner, *Party and Constituency: Pressures on Congress* (Baltimore: The Johns Hopkins Press, 1955), pp. 27–29, 36–71. Also, David Truman, *The Congressional Party* (New York: John Wiley and Sons, 1959), esp. chs. 3, 5. And Duncan MacRae, Jr., "The Relationship Between Roll Call Votes and Constituencies in the Massachusetts House of Representatives," this REVIEW, 44 (December, 1952), 1046–1055.

[37] Wilder Crane, Jr., "A Caveat on Roll-Call Studies of Party Voting," *Midwest Journal of Political Science,* 4 (August, 1960), 237–249.

[38] Fred I. Greenstein and Elton F. Jackson, *op. cit.*

Wisconsin party did not play a key role in a series of highly visible and intensely partisan disputes before the Court in 1964 and 1965. Conflicts over the appointment power[39] and reapportionment[40] between Democratic Governor John W. Reynolds and Republican majorities in both legislative houses and a dispute between Democrats and Republicans in the state assembly over the seating by the Republican majority of a claimant to a lower house seat as against his Democratic rival whose election had been certified by the canvassing board and a lower court[41] were finally settled by the Wisconsin Supreme Court. In each case the decision was unanimous despite the close division on that Court between adherents of the two parties.

Second, one of the variables to be tested in this study is the extent of partisanship in judicial campaigns, and this required on-the-scene research. Wisconsin was convenient for this purpose. Third, there are significant similarities between the Wisconsin and Michigan political systems other than mere geographical proximity. The use of Wisconsin for further testing was a rough attempt at holding constant some variables in the political system. In both states, for instance, the parties are organized on a mass membership basis. In the post-war years, the Democratic party in both states was taken over by liberal political activists and was closely aligned with the labor movement. The Republican party in both states was dominated by activists of a clearly conservative ideology (at least until the early 1960's).[42] Financial support for the Democrats in both states was drawn primarily from union labor, liberal intellectuals, and a variety of professional and small business people, while the largest single source of Republican financial support was manufacturing and financial interests.[43] In short, both states were marked by close interparty competition between highly issue-oriented party organizations divided along contemporary liberal-conservative lines and financed from sources compatible with such a division.

There were also substantial similarities between the structural arrangements of the highest courts of the two states.[44] Each state, for instance, elects supreme court justices on a nonpartisan ballot for a fixed term, eight years in Michigan and ten in Wisconsin. Each selects judges in spring election rather than during the November balloting. And in both states the governor is empowered to fill vacancies. A striking difference is found in the nominating process, which in Michigan is by party convention and in Wisconsin by nonpartisan primary.

Another similarity between the two states was the changing membership of their supreme courts. In 1954, the Michigan Supreme Court was composed of six Republicans and two "old line" conservative Democrats.[45] By 1958, the process of appointment and election had converted the Court's membership to five "Williams" or liberal Democrats and three Republicans. The Wisconsin Supreme Court began with a division of six Republicans and one Democrat in 1957 and was altered by appointments and elections to a division of four Democrats and three Republicans by 1965. The Michigan studies show that a change in judicial conduct accompanied the change in party division on the Court. Since Wisconsin underwent similar alteration of the party division in a quite similar party environment, Wisconsin seems an appropriate place to attempt a validation of the party variable hypothesis.

The period studied here is from January 7, 1957, to January 1, 1966. During that time nine Republicans and four Democrats held posts on

[39] *State ex rel. Thompson v. Gibson et al.*, 22 Wis. 2d 275 (1964); *State ex rel. Reynolds v. Smith*, 22 Wis. 2d 516 (1964).

[40] *Stat ex rel. Reynolds v. Zimmerman et al.*, 22 Wis. 2d 544 (1964); *State ex rel. Reynolds v. Zimmerman*, 23 Wis. 2d 606 (1964).

[41] *State ex rel. Elfers v. Olson*, 26 Wis. 2d 422 (1965).

[42] The ideological and programmatic aspects of the parties are described in John Fenton, *Midwest Politics* (New York: Holt, Rinehart and Winston, 1966), chs. 2–3; Stephen B. and Vera Sarasohn, *Political Party Patterns in Michigan* (Detroit: Wayne State University Press, 1957); Leon D. Epstein, *Politics in Wisconsin* (Madison: University of Wisconsin Press, 1958), pp. 36–38, 46–56, 82–86; and Frank Sorauf, "Extra-Legal Political Parties in Wisconsin," this REVIEW, 48 (September, 1954), 692–704.

[43] Michigan party finance is thoroughly described in John P. White and John R. Owens, *Parties, Group Interests and Campaign Finance: Michigan '56* (Princeton: Citizens' Research Foundation, 1960). The financial bases of the Wisconsin parties are described in Epstein, *op. cit.*, pp. 79–80; H. Gaylon Greenhill, *Labor Money in Wisconsin Politics, 1964* (Princeton: Citizens' Research Foundation, 1966), esp. pp. 22–39; and David Adamany, *Money in a State Political System: Wisconsin* (unpublished Ph.D. dissertation, Department of Political Science, University of Wisconsin, 1967), esp. ch. 5.

[44] Information about the selection of judges in Michigan is derived from S. Sidney Ulmer, *op. cit.*, unless otherwise noted.

[45] *Ibid.*, p. 268.

TABLE 1. MEMBERSHIP CHANGES AND PARTY DIVISION ON THE WISCONSIN SUPREME COURT: JANUARY 7, 1957 TO JANUARY 1, 1966

Period	I	II	III	IV	V	VI	VII
Time Span: From	Jan 7, 1957	May 1, 1958	Jan 5, 1959	Jan 2, 1962	June 5, 1962	Jan 6, 1964	Aug 18, 1964
To	Apr 30, 1958	Jan 4, 1959	Jan 1, 1962	June 4, 1962	Jan 5, 1964	Aug 17, 1964	Jan 1, 1966
Republicans	6	6	6	5	4	4	3
Democrats	1	1	1	2	3	3	4

the Wisconsin Supreme Court. Six of the nine Republicans and two of the four Democrats went to the bench initially upon the appointment of governors of their respective parties; the remainder obtained their offices through election. Of the nine justices with Republican affiliations, six had sought elective office on the party ticket, two had served in appointive positions in a Republican gubernatorial administration, and the ninth had no history of active partisanship. Two of the four Democratic-affiliated justices had campaigned and been elected to office on the party ticket, one had served in high party office and as an appointee in a Democratic administration, and the fourth had been active as an advisor and contributor to several prominent Democratic leaders. All four had been dues-paying members of the Democratic Party. Twelve of the thirteen justices had therefore engaged in the kind of party activity which should have exposed them to the influence of party as conceptualized in this paper.

It is difficult to analyze the degree of partisanship in the Court's deliberations during a particular calendar year or Term of Court because of changes in membership on the bench. Judicial tenure begins in January, and Terms of the Court begin in August. There is no fixed period of stable membership on the court because appointments are made whenever a vacancy occurs through death or resignation. For purposes of analysis, the nine year period under study was therefore divided into seven periods, each representing the time during which the same seven justices served together. Whenever membership on the Court changed, a new period began. The length of these periods varies widely, the shortest lasting only six months and the longest almost three years. Table 1 reports the seven time periods and the partisan division on the Court during each.

IV. WORKMEN'S AND UNEMPLOYMENT COMPENSATION CASES

Both Schubert and Ulmer used workmen's compensation cases to test the relationship between party affiliation and judicial conduct in Michigan, and Ulmer used unemployment compensation cases as well. These policy areas are also well suited to testing the party variable on the Wisconsin Supreme Court because, as in Michigan, the contesting interests in these cases are clearly identified with different parties. The business community has traditionally been tied to a conservative Republican Party organization and the labor movement to a liberal Democratic Party. These issues thus invite policy divisions along party lines.

Although simple roll call techniques do not show party bloc cohesion on issues, Table 2 is a roll call statement of the voting of Democratic and Republican judges on the Wisconsin Supreme Court on workmen's compensation cases for the seven periods here under study. This table replicates with Wisconsin data Ulmer's presentation of the voting records of Michigan judges on this issue. Replication is useful here because it shows that, even by the least rigorous cirterion, the sharp party line division of judges which occurred in Michigan on this issue is not found in Wisconsin.

The Ulmer data show that in no Term of Court from 1954 to 1960 did any Republican justice vote for the claims of injured workers as frequently (measured by persentages) as the Democratic justice with the record least favorable to the worker-claimants. The narrowest difference between the most pro-worker Republican justice and the least pro-worker Democratic justice was nine percent in 1958 and the widest margin was 36 percent in 1960. The average difference between the two parties for the six Terms studied was 23 percent.

A note of caution is required about the Wisconsin data prior to their comparison with those from Michigan. There appears to have been a number of instances of nonparticipation which affected the pro-worker scores of two Democratic justices. Justice Fairchild and Justice Wilkie disqualified themselves in cases which had originated in lower courts presided over by their father and brother, respectively. More instances of disqualification occurred in cases decided for the worker without dissent than in cases which divided the Court, causing certain of the Fairchild and Wilkie pro-worker percent-

TABLE 2. PERCENTAGE OF VOTES IN FAVOR OF WORKMEN'S COMPENSATION CLAIMANTS: WISCONSIN SUPREME COURT, 1957–1966**

Period	I	II	III	IV	V	VI	VII
Number of Cases	11	15	39	4	7	3	16
Heffernan (D)							56.2
Wilkie (D)					57.1*	33.3*	43.8*
Gordon (D)				25	85.7	66.6	50
Fairchild (D)	63.9*	40*	33.3	25	100	66.6	56.2
The Court	*90.9*	*60*	*33.3*	*25*	*85.7*	*66.6*	*56.2*
Currie (R)	90.9	60	33.3	25	71.4	66.6	56.2
Hallows (R)		53.3	30.7	25	85.7	33.3	43.8
Brown (R)	90.9	26.6*	33.3	25	85.7		
Broadfoot (R)	100	53.3	33.3	25			
Martin (R)	90.9	33.3*	30.7*				
Dieterich (R)			35.8	25	100	66.6	
Beilfuss (R)						66.6	50
Steinle (R)	81.8						
Wingert (R)	81.8	60					

R = Republican; D = Democrat.

* These percentages are affected by nonparticipation in some workmen's compensation cases decided by the Court during the Period described. See explanatory discussion in the text.

** This Table adopts the format used by Ulmer to facilitate comparison of these Wisconsin data with those he developed for Michigan.

ages to be somewhat lower despite the fact that they cast no more anti-worker votes than their brethren. Occasional nonparticipation by other justices, usually because of illness, affected their scores, although not to the same extent. The scores affected by nonparticipation are designated in Table 2 with an asterisk.

The notable feature of Table 2 is the lack of difference along party lines in the pro-worker scores of the justices. In each of the seven periods, Republican justices made records as favorable to the worker as their Democratic counterparts. A possible exception is Justice Hallows (R) who was somewhat less favorable to the worker-claimants than his brothers of both parties. Although a Republican, Justice Hallows came to the Court with a much less party activist background than that of most justices, and so even in his case it is not clear that party was a significant variable.

More impressive than this type of roll call analysis was Ulmer's finding that in the 1958–1960 Terms of the Michigan Supreme Court there was clear bloc voting along party lines on workmen's compensation and unemployment compensation cases. He found three patterns of voting: all justices voting for the worker's claim, all justices voting against the worker's claim, Democratic justices (with an occasional deviation) voting for the claim and Republican justices voting against it.[46] Where division on a case did occur, it always featured Democratic justices taking the pro-worker position and Republican justices taking the anti-worker position; this pattern looks very much like the cleavages between cohesive party blocs found in some analyses of legislative behavior.

Tables 3 and 4 report the votes of Wisconsin Supreme Court justices on workmen's and unemployment compensation cases from 1957 to 1966.

Although laid out in scalogram form for convenience, the data cannot be tested for scalability because of the large number of nonparticipations. The tables show, instead, the lack of party bloc voting on the same kinds of issues which sharply split the Michigan court, operating in a similar political environment, along party lines. The left hand column in each table records the number of times each justice cast a vote for the worker's claim in a case decided without dissent. Similarly, the right hand column records the number of times each justice cast a vote against the worker's claim in a case decided without dis-

[46] *Ibid.*, pp. 360–361.

TABLE 3. VOTES IN WORKMEN'S COMPENSATION CASES: WISCONSIN SUPREME COURT: 1957-1966

	+ without dissent	6-1	6-1	6-1	6-1	5-1	5-1	5-2	5-2	2-4	1-6	1-6	− without dissent
Dieterich (R)	19		+	+		+				+		+	28
Broadfoot (R)	29	+	+				−	+			+	−	34
Fairchild (D)	36	+	+	+	+	+	+	+	+	+	−	−	38
Currie (R)	42	+	+	−	+	+	+	+	+	−	−	−	42
Gordon (D)	13			+	+	+			+	−			9
Wilkie (D)	9			+	+	NP			+	NP			6
Heffernan (D)	7				+				+				7
Wingert (R)	15	+					+	+			−		6
Martin (R)	24	+	+				+	−			−	−	27
Beilfuss (R)	8				+	+			−				8
Brown (R)	31	+	+	+			NP	−		−	−	−	30
Hallows (R)	32		−	+	−	−	+	+	−	−		−	42
Steinle (R)	9	−									−		
N = 95													

sent. The description of a case as "decided without dissent" encompasses some cases in which one of the justices did not vote. The cases presented individually are those on which the Court divided.

Two aspects of the data presented in Tables 3 and 4 distinguish judicial voting patterns on the Wisconsin Supreme Court in workmen's and unemployment compensation cases from the patterns found in Michigan. First, the frequency of disagreement on the Wisconsin Court was much less than on the Michigan Court. Of the 95 workmen's compensation cases decided by the Wisconsin Supreme Court during the period under study, 88 percent (84 cases) were decided without dissent. Schubert's data for the 1954–1957 Terms of the Michigan Court are not presented in a manner which permits the computation of the percentage of workmen's compensation cases on which that court divided, but Ulmer's data show that the Michigan justices decided only 64 per cent (19 of 29 cases) of the workmen's compensation matters without dissent during the 1958–1960 Terms. Of the unemployment compensation cases, the Wisconsin justices decided 68 per cent (13 of 19 cases) without dissent, while their Michigan counterparts decided only 50 per cent of (6 of 12 cases) without dissent. The greater percentage of cases decided without dissent by the Wisconsin Supreme Court suggests that party conflict did not play the major role in that Court's deliberations that it did in the Michigan Court, even though the party systems in both cases were characterized by sharp polarization of labor and management interests into program-oriented Democratic and Republican parties respectively.

TABLE 4. VOTES IN UNEMPLOYMENT COMPENSATION CASES: WISCONSIN SUPREME COURT: 1957–1966

	+ without dissent	6-1	6-1	5-2	5-2	3-4	3-4	− without dissent
Fairchild (D)	5	+	+	+	+	+	+	8
Dieterich (R)	2	+				+	+	4
Brown (R)	2	+		+	+	+	−	4
Wingert (R)	1			+	+			2
Martin (R)	1	+		+	+	−	−	3
Currie (R)	5	+	+	−	−	−	−	8
Gordon (D)	4		+					4
Wilkie (D)	3		+					2
Heffernan (D)	2		+					2
Beilfuss (R)	3		+					4
Broadfoot (R)	1	+		−	−	−	+	4
Hallows (R)	4	−	−	+	+	−	−	7
Steinle (R)	0							1
N = 19								

A second aspect of the data in Tables 3 and 4 raises an even stronger inference that party affiliation was not a significant factor among Wisconsin Supreme Court justices in these two policy areas. In none of the 17 split decisions in the workmen's and unemployment compensation fields did the Democratic justices vote as a bloc on one side of the issue and the Republicans as a bloc on the other. This was, by contrast, the predominant pattern in the split decisions of the Michigan Supreme Court. In 13 of the 17 split decisions in Wisconsin a majority of the justices affiliated with each of the parties joined to make the Court majority. None of the more numerous Michigan split decisions brought together a ma-

TABLE 5. DISAGREEMENT ON THE WISCONSIN SUPREME COURT: 1957–1966

Time Period	Justices Dem. N	Justices Rep. N	Total Decisions	Split Decisions	Dissenting Votes	Ratio of Split to Total Decisions	Average No. of Dissenting Votes Per Split Decision
I	1	6	305	43	79	.14	1.84
II	1	6	210	30	43	.14	1.43
III	1	6	727	68	117	.09	1.72
IV	2	5	113	14	22	.12	1.57
V	3	4	382	67	115	.18	1.71
VI	3	4	182	27	45	.15	1.67
VII	4	3	342	51	98	.15	1.92

jority of each party to make a Court majority; the divisions all reflected sharp cleavages between tightly knit party blocs.

V. PARTY AFFILIATION AND DISSENTING BLOCS

In addition to sharp party-line divisions on workmen's and unemployment compensation, the Michigan Supreme Court was also characterized by a clear pattern of dissenting bloc voting along party lines. At the outset, this kind of analysis is open to some question. Unlike the workmen's and unemployment compensation issues, the total agenda of judicial business does not consist of matters on which the parties are naturally divided. Notwithstanding this qualification, Schubert's evidence shows such a high degree of division along party lines on the general business of the Michigan Supreme Court that he raises a reasonable inference that party affiliation was a significant variable which pervaded most of that Court's deliberations.[47]

At the outset, Schubert found in the overall business of the Michigan Court "a rising curve of disagreement" which was illustrated by a rising ratio of split to total decisions and an increase in the average number of dissenting votes per split decision.[48] ". . . [T]he infusion of the Williams Democrats," he argued, "led directly to a considerable increase (260 per cent) in disagreement among the justices."[49]

Similar analysis of the dissenting votes cast in all cases decided by the Wisconsin Supreme Court during the rise of its Democratic membership from a minority of one to a majority of four does not reveal such "a rising curve of disagreement." Table 5 reports the data for Wisconsin.

In Michigan the ratio of split to total decisions rose from .09 for the 1955 Term in which only one Democratic justice served on the Court to .16 for the 1957 Term in which the Court was divided evenly between the parties. By contrast, the ratio in Wisconsin was about the same in the early periods (.14) when Justice Fairchild was the lone Democrat and in the later periods (.15) when the Court shifted from a four to three Republican majority to a four to three Democratic majority. The average number of dissenting votes per split decision on the Michigan Court increased steadily from 2.33 to 2.95 from 1955 to 1957. In Wisconsin the average was 1.84 in the early period and 1.92 in the last period, with the intervening periods showing a fluctuation among somewhat lower averages.

It is significant that the average number of dissenting votes per split decision did not rise in Wisconsin as the membership of the Court became more evenly divided between justices of different party backgrounds. Party as a significant determinant of judicial voting on the general business of the Court should be shown by an increase in the average number of dissenting votes per split decision in such circumstances because the minority party, acting as a bloc, would have more votes to cast in dissent in cases which divided the Court. The absence of such an increase suggests the absence of party as an important variable in voting on the Wisconsin Supreme Court.

The second phase of Schubert's investigation of party affiliation as a determinant of division on the Michigan Supreme Court consisted of dissenting bloc analysis for the 1954–1957 Terms of that Court. Schubert developed the Index of Cohesion for describing the "average frequency of dissenting votes cast by bloc members in conjunction with other members of the same bloc."[50] An Index of Cohesion of .50 or greater is considered high, an Index of Cohesion

[47] Glendon Schubert, *Quantitative Analysis of Judicial Behavior*, pp. 135–142.

[48] *Ibid.*, p. 134, esp. Table 3.15.

[49] *Ibid.*, p. 134.

[50] *Ibid.*, p. 89.

TABLE 6. EXISTENCE AND COHESION OF DISSENTING BLOCS ON THE WISCONSIN SUPREME COURT: 1957–1966

Time Period	Justices		Blocs by Index of Cohesion		
	Dem. (N)	Rep. (N)	Low	Moderate	High
I	1	6	Currie (R)–Steinle (R)–Wingert (R) (.30)	Currie (R)–Fairchild (D) (.43)	
II	1	6			
III	1	6		Martin (R)–Broadfoot (R) (.46)	
IV	2	5	Currie (R)–Dieterich (R)–Hallows (R) (.31)		Fairchild (D)–Brown (R)–Gordon (D) (.50)
V	3	4	Wilkie (D)–Hallows (R)–Currie (R) (.39)	Dieterich (R) Gordon (D) (.44)	
VI	3	4	Wilkie (D)–Gordon (D)–Fairchild (D) (.33) Hallows (R)–Currie (R)–Beilfuss (R) (.32)		
VII	4	3	Hallows (R)–Currie (R)–Beilfuss (R) (.30)	Hallows (R)–Beilfuss (R) (.44)	

Indexes of Cohesion: Low = .30 – .39; Moderate = .40 – .49; High = .50 or higher. R = Republican; D = Democrat.

of .40–.49 is moderate, and any Index of less than .40 is treated as low.[51]

Schubert discovered distinct Democratic and Republican blocs, each with a high Index of Cohesion. In the 1955 Term, a "right bloc" of Republicans had an Index of Cohesion of .64. There was only a single Democrat, so obviously there was no "left bloc" pattern of voting. However, the lone Democrat did cast the largest number of solo dissents. In the 1956 Term, a "right bloc" consisting of four Republicans had a .55 Index of Cohesion and a "left bloc" consisting of the three Democrats had a .50 Index. In the 1957 Term, with the Court split evenly between four Democrats and four Republicans, the Indexes of Cohesion rose sharply, with the Democratic bloc achieving a .83 Index and the Republican bloc a .68 Index. In no instance during these three Terms were any Democrats and Republicans on the Michigan Supreme Court joined together in a bloc.

The pattern of judicial behavior in Wisconsin does not demonstrate this kind of partisanship. As Table 6 indicates, there were no stable blocs on the Wisconsin Supreme Court and such blocs as did exist in a given time period tended to be of low cohesion. Table 6 reports the existence on the Wisconsin Supreme Court of all dissenting blocs with a minimum Index of Cohesion of at least .30.

In Period I, Democratic Justice Fairchild and Republican Justice Currie constituted a moderately cohesive bloc, but Currie was also part of a three-man Republican bloc of low cohesion. In such circumstances, it must be concluded that party lines were almost non-existent, especially

[51] *Ibid.*, p. 90.

since the Court's three other members were Republicans and the existence of a three man Republican bloc would show some division within the party rather than along party lines. In Period IV a three-man Republican bloc of low cohesion and a three-man bloc of two Democrats and a Republican with a high cohesion index can be identified. In the next period, however, one of the members of the Republican bloc (Dieterich) and one of the Democratic members (Gordon) of the mixed bloc were found voting together with moderate cohesion. Meanwhile, Dieterich's place in the Republican bloc was filled by newly appointed Justice Wilkie, a Democrat.

In Periods VI and VII there are hints of partisanship among dissenting blocs. A three-man Democratic bloc and a three-man Republican bloc are clearly visible in Period VI, and the Republican bloc is again found in Period VII. The suggestion of partisanship might appear stronger with the report in Period VII of a three-man Democratic bloc (although one of the members has changed) with a cohesion index of .29, just below the .30 minimum level set for presentation in Table 6. It must be noted, however, that these blocs are characterized by very low cohesion scores which contrast them sharply to the cohesion scores of .83 (Democratic) and .68 (Republican) found in Michigan when its Court was closely divided between the parties.

A more cogent critique of interpreting these kinds of dissenting blocs as signs of party cohesion can be found in Joel Grossman's recent paper arguing that cohesion scores may be misleading as to the actual existence of bloc voting.[52] Grossman notes that high cohesion scores may be yielded when two members of a bloc frequently vote with a third member but not with one another. Furthermore, other justices may join bloc members in a number of dissents which count toward the cohesion score without themselves being counted as members of the bloc. Thus the apparent existence of a bloc may be refuted by a closer analysis of the actual frequencies with which various members vote together. This is especially relevant for analysis of partisan blocs whose existence is predicated on the assumption that members of each party act as a cohesive group whose objectives are contrary to those of the opposition party.

Table 7 is modeled on Grossman's presentation of the voting of members of blocs on the United States Supreme Court.[53] The presentation is designed to test more precisely than the cohesion index the actual degree of group voting by bloc members in Periods VI and VII when slight suggestions of party voting appear in the dissenting bloc analysis. It is obvious that Grossman's point is pertinent to the blocs found on the Wisconsin Supreme Court. Seldom were all members of the bloc found voting together; the more common pattern was for two of them to vote together, sometimes as a pair and at other times in conjunction with a non-bloc member. Furthermore, these non-bloc members were frequently members of the opposing party bloc. Thus in Period VII, all five instances in which a pair of Republican bloc members voted together as part of a larger group found them in alliance with a member of the opposing Democratic bloc. Similarly, a member of the Republican bloc provided the additional vote in the three cases in which a pair of Democrats voted together as part of a larger group.

TABLE 7. ACTUAL PATTERN OF JOINT DISSENT AMONG MEMBERS OF DISSENTING BLOCS: WISCONSIN SUPREME COURT

Bloc As Reported	Dissented Alone: Number of Times	Dissented as Part of Larger Group: Number of Times
Period VI		
Currie-Hallows-Beilfuss (all Republicans)	1	0
Currie-Hallows	2	0
Hallows-Beilfuss	2	0
Currie-Beilfuss	0	0
Fairchild-Wilkie-Gordon (all Democrats)	1	0
Fairchild-Wilkie	2	0
Fairchild-Gordon	1	0
Wilkie-Gordon	0	0
Period VII		
Currie-Hallows-Beilfuss (all Republicans)	1	0
Currie-Hallows	2	1
Hallows-Beilfuss	5	3
Currie-Beilfuss	0	1
Fairchild-Heffernan-Gordon (all Democrats)	2	0
Fairchild-Heffernan	0	1
Fairchild-Gordon	2	2
Heffernan-Gordon	0	0

Several different aspects of this dissenting bloc analysis add to our previous evidence that party was not a significant determinant of judicial behavior in Wisconsin. First, the addition of members to the minority party on the bench neither increased the frequency of division on the Court, nor increased the average number of dissenting votes in those cases on which the Court divided. Second, such dissenting blocs as existed were often bipartisan. Third, dissenting blocs tended to be highly fluid, changing mem-

[52] Joel B. Grossman, "Dissenting Blocs on the Warren Court: A Study in Judicial Role Behavior," (unpublished paper).

[53] *Ibid.*, p. 24.

bership from period to period. Fourth, such dissenting blocs as did exist were marked by low cohesion scores. Fifth, even in the last periods where such low cohesion blocs were apparently partisan in their composition, a close analysis reveals frequent joint dissent by bloc members and justices of the opposite party.

VI. JUDGES, PARTISANSHIP AND CONSTITUENCY

The conceptualization of the role of party in judicial behavior which has been developed in this paper has emphasized the socializing processes which occur in certain situational circumstances. This section suggests that party continues to be an important reference group and socializing agency for judges who rely on party for election to and continued tenure on the bench. On the other hand, where judges' constituencies are essentially bipartisan, their situational circumstances are significantly different and party will be much less significant in the decisional process. The available evidence supports the view that the constituencies of Michigan supreme court justices are highly partisan and that those of Wisconsin supreme court justices are bipartisan. This would be an adequate explanation of the sharp difference in the role that party plays in the decisional process in those two states.

In both Michigan and Wisconsin, judges are elected on nonpartisan ballots in the spring elections; but that is an inadequate measure of the role of parties in judicial elections. Instead it is necessary to examine the actual forces at work in the election process for

> in some political settings nonpartisanship is essentially norminal. Removing party labels from the ballot seems to have made little political difference, since local Republicans and Democrats are sufficiently strong to take over the nonpartisan elections and nominate and elect their own candidates.[54]

It is the actual relationship between judges and parties in elections, rather than the legal forms, that may affect the policy responses of judges.

One element of partisanship in Michigan elections not found in Wisconsin is nomination by party convention. This increases the likelihood that judges will see themselves as part of and will remain in active contact with political parties. In Wisconsin, where nomination is by nonpartisan primary balloting, the nominees need not view themselves as party candidates.

We do not have systematic data on the degree of partisanship in Michigan judicial elections, but Ulmer's brief description of the election process in that state stresses the influence of the party affiliations of judicial candidates.[55] Until 1939, Michigan nominated judicial candidates in party conventions and elected them on party ballots. In that year the state constitution was changed to make the ballot nonpartisan because "as the strengths of the two parties . . . approached a relative balance in state politics, competition for judicial office . . . involved that office in the basest kind of political struggles."[56] The party convention method of nomination was retained, however. Alteration of the ballot form did not, according to Ulmer, take party politics out of the election process. The press gives considerable coverage to the party convention during the nominating process and identifies candidates by party affiliation throughout the campaign. Furthermore, the candidates themselves recognize the partisan nature of both the election contest and the office of justice.[57] In at least one recent year, the Democratic candidates announced that they would campaign together as a party team after the nominating convention and warned their fellow partisans that an entire branch of the state government was up for capture in the judicial election.[58]

To compare Wisconsin judicial elections with the highly partisan judicial elections in Michigan, it is necessary to ascertain the extent to which these Wisconsin contests are in fact characterized by party-line divisions in the electoral and campaign supports given the candidates. To test the extent of partisanship in the campaign, the partisan affiliations of committee members, prominent endorsers, and campaign contributors are examined. The conduct of the newspapers, whose party affiliations can generally be identified, and of organized labor is also considered.

Although there were seven judicial elections between 1957 and 1966, the analysis of partisanship in the campaisn process will be restricted to the three which were fought between pairs of nominees who were clearly identifiable by past

[54] Fred I. Greenstein, *The American Party System and the American People* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), p. 57.

[55] Ulmer, *op. cit.*, pp. 354–355.

[56] *Ibid.*, p. 354. Despite the evidence in Ulmer's article and the advocacy of change by some justices, the Michigan Constitutional Convention did not alter the institutional arrangements for the election of supreme court justices. The responsibility for nominating judicial candidates remains in the hands of party conventions, *Michigan Statutes Annotated,* Section 6.1392, and the general election ballot maintains its nonpartisan form, *ibid.,* Section 6.1393.

[57] Ulmer, *op. cit.*, pp. 354–355.

[58] *Ibid.*, p. 334.

affiliation with opposing parties. These were the contests between Circuit Judge Myron Gordon, a Democrat, and former Republican attorney general Stewart Honeck in 1961; between Justice Horace Wilkie, a Democrat, and Howard Boyle, a conservative Republican, in 1964; and between Justice Nathan Heffernan, a Democrat, and Boyle in 1965.[59]

The Gordon-Honeck Campaign. Circuit Judge Myron Gordon's successful campaign for the Supreme Court appears to have been based on a bipartisan constituency. Gordon was able to announce a committee of 657 Milwaukee county lawyers, almost half of whom were Republicans. A committee of past bar association presidents supporting Gordon consisted of five known Republicans and four lawyers with no clear party identification. A similar committee of "well-known attorneys" whose names graced a series of newspaper advertisements and campaign letters included three Republicans, two Democrats and three independents. While his opponent received the endorsement of Chief Justice John Martin, a former Republican attorney general, Gordon was able to claim the endorsement of Justice Timothy Brown, also a Republican.

Among the state's 21 daily newspapers in cities of medium or large size, 16 can be counted normally Republican, two Democratic and three independent based on past endorsements for governor and U.S. senator. Twelve of the 16 Republican papers preferred Democrat Gordon over his Republican rival, as did both Democratic papers and one of the independent journals. Honeck drew a single endorsement, from a Republican paper, and five papers did not act or their action could not be ascertained from the available research sources.

Five committees supporting Gordon reported to the secretary of state's office aggregate spending of $33,451. Labor, which endorsed Gordon, accounted for $1450. Seventy-six contributors of $100 or more gave $11,196. Of these, seven Republicans gave $1796 and 18 Democrats gave $1900. The remaining 61 contributors could not be identified as known partisans. The list suggests, however, that most were Jews, and that the strategy of Gordon's fund-raising effort was to solicit his co-religionists rather than regular party contributors.

The election results gave Gordon a margin of 33,000 votes in a polling of almost 766,000. The correlation between his percentage of the vote by county and the percentage by county of the Democratic nominee for governor five months before was .23, too low to be regarded as significant.[60] A similar correlation between his opponent's vote and that of the Republican gubernatorial nominee was —.02, despite several attempts by his opponent to take advantage of his Republican party activity and affiliation.[61] Gordon's own comment aptly described the campaign: "Party politics played no part in the election. I had bipartisan support. I belong to no party; I am no longer a dues paying Democrat."[62]

The Wilkie-Boyle Campaign. Like the Gordon campaign, that of Justice Wilkie showed a high degree of nonpartisanship. Boyle announced that his campaign would "include a criticism of Wilkie's record as a justice, and in particular his vote with the majority of the high court not to ban Henry Miller's *Tropic of Cancer* as an obscene book in Wisconsin."[63] Wilkie responded by "marshalling . . . bipartisan support of powerful allies—the bar, the bench, law professors, politicians, labor, businessmen and the press."[64]

Wilkie's statewide lawyers and citizens committees numbered among their leaders 37 prominent Republicans, 22 Democrats and 11 independents or others. The local committees supporting Wilkie showed a similar balance.

Wilkie's active workers were generally political and bar notables—men of high public office, particularly in the state bar, with years of state and

[59] Other judicial elections were the unopposed re-election of Justice George Currie (R) in 1957; the election of William Dieterich (R) over sitting Justice Emmert Wingert (R) in 1958; the election of sitting Justice Harold Hallows (R) over Stewart Honeck (R) in 1959; the election of Circuit Judge Bruce Beilfuss (R) over Harry E. Larsen, a Democrat so nominal that a number of state Democratic party leaders had never heard of him, in 1963.

[60] The author is aware of the dangers of using aggregate vote data, especially for as large a political unit as the county. Unfortunately this is the only data available for one of the three elections. Cf. Austin Ranney, "The Utility and Limitations of Aggregate Data in the Study of Electoral Behavior," in Austin Ranney (ed.), *Essays on the Behavioral Study of Politics* (Urbana: University of Illinois Press, 1962), pp. 91–102.

[61] John Wyngaard, "Wisconsin Report: Non-Partisan Pretense in Judicial Elections is Being Exploded in State," *Janesville Daily Gazette,* March 17, 1961, p. 6.

[62] *Milwaukee Journal,* March 8, 1961, p. 8.

[63] Jack Ladinsky and Allan Silver, "Popular Democracy and Judicial Independence," *University of Wisconsin Law Review* (Winter, 1967), p. 148.

[64] *Ibid.,* p. 149.

national, partisan and nonpartisan political experience.[65]

Survey data showed that about one-third of Wilkie's active supporters were Republicans.[66]

Wilkie gained the endorsement of 13 of the 16 Republican dailies, both Democratic papers and two of the three independent papers. Boyle received no endorsements from these papers. Wilkie also gained the endorsement of labor. He received the endorsement of three retired chief justices and two retired justices, all Republicans. Nine of his former colleagues in the state senate, including five Republicans, publicly supported his candidacy. Democratic Governor Reynolds and former Governors Nelson (D), Kohler (R), and LaFollette (Prog.) recommended his election. The most unusual declaration was that of five of his fellow justices, three Republicans and two Democrats, who severely criticized Boyle's discussion of specific court decisions as improper and urged the election of Wilkie.

The Wilkie forces reported spending $33,451.[67] Sixty-four people gave sums of $100 or more, their total gifts amounting to $12,698. Among them were 16 Republicans who gave a total of $6398, of which $4998 came from a single source, and 31 Democrats who contributed a total of $4150. Labor's contributions amounted to $7600.

On election day, Justice Wilkie was returned to the high court with a margin of 67,000 votes in 1,046,000 cast. Survey data demonstrate the bipartisanship of Wilkie's electoral base. They show that among Republican identifiers, Wilkie received 16.7 per cent to Boyle's 9.5 per cent, the remainder either not voting, not answering or not remembering. Among Democrats, Wilkie received 10.4 per cent to Boyle's 9.9 per cent, while the independents gave Wilkie 12 per cent and Boyle 8.4 per cent.[68]

[65] *Ibid.*, p. 156.

[66] *Ibid.*, p. 157.

[67] The discrepancy between this figure and the $56,000 figure cited by Jack Ladinsky and Allan Silver, *op. cit.*, p. 150, is due to their inclusion in the total of certain transfer payments from one committee to another which swelled the reported total but did not actually represent new campaign spending. A similar disparity occurs between their report (p. 153) and mine concerning the amount spent in the Heffernan campaign.

[68] These survey data and those used in the subsequent section on the Heffernan-Boyle campaign were collected by Ladinsky and Silver for their previously cited study. I am grateful to Professor Ladinsky for his courtesy and cooperation in making them available for use in the present paper.

The Heffernan-Boyle Contest. In many ways, the 1965 Heffernan-Boyle contest was a rerun of the 1964 judicial election. Like Wilkie, Heffernan was a Democratic appointee attempting to retain his seat. The thrust of Boyle's campaign was that the Court was too liberal, that it was legislating, and that it was usurping the powers of the legislature and the people. To respond to this campaign Heffernan was able to rally many of the same forces that had assisted Wilkie.

Heffernan was endorsed by all 16 Republican dailies, both Democratic dailies, and by two of the three independent papers. His statewide committee included 64 known Republicans, 82 known Democrats and 63 members who could not be identified. His county committees were similarly nonpartisan. His campaign committee was headed by former Republican Governor Oscar Rennebohm, and he drew endorsements from sitting Justice Myron Gordon, a Democrat, and retired Chief Justices Timothy Brown and John Martin, both Republicans. Heffernan also had the endorsement of organized labor.

The various committees supporting Heffernan reported spending $36,575, of which $11,225 was contributed by labor and $11,369 by donors of $100 or more. Seventeen of these donors, accounting for $6009 (although one contributor gave $3774), were Republicans and 19, whose aggregate gift was $3835, were Democrats.

On election day, Heffernan won by a slim margin of 13,500 in a total vote of 739,000. As in previous elections, the voters did not appear to divide along party lines. Survey data show that Republicans preferred Heffernan by 20.8 per cent to Boyle's 18.5 per cent; Democrats followed suit with 14.9 per cent for Heffernan and 14.4 per cent for Boyle; independents gave Heffernan 18.7 per cent and Boyle 15.7 per cent.

The foregoing data provide considerable evidence that members of the Wisconsin Supreme Court may accurately view their constituencies as bipartisan and do not need to remain actively engaged with their parties as a method for election or re-election to the bench. One objection to these data is that, since a Democrat was the winner in each instance, the support of labor and of Democratic politicians does not demonstrate the willingness of these forces to ignore partisan considerations in judicial contests. In 1967, after the termination of the present study, Chief Justice George Currie, a Republican, was opposed for re-election by Circuit Judge Robert Hanson, a Democrat. Although Currie failed of re-election, he was able to muster labor's endorsement and financial help as well as the endorsements and assistance of United States Circuit Judge Thomas Fairchild, a Democrat, Fed-

eral District Judge John W. Reynolds, a former Democratic governor, Democratic Attorney General Bronson La Follette, former Democratic Lieutenant Governor Patrick J. Lucey, and a host of Democratic party leaders and lawyers. It seems, therefore, that judicial candidates with prior Republican affiliations, as well as those with Democratic backgrounds, can reasonably hope for the support of a broadly bipartisan campaign constituency.

On the basis of limited data about judicial elections in two states, constituency emerges as a variable which has relevance to the study of party affiliation as a determinant of judges' voting. The situations of judges in Michigan continue to include party as a strong force, either because judges act directly to retain the support of their highly partisan constituencies or because there is a continuing socialization which occurs through their contacts with party during campaigns and the expectations of such contacts during the interelection period. The situations of judges in Wisconsin diminish the significance of party; judges may act consciously to retain the support of their bipartisan constituencies or their decision making may merely reflect a more permissive situation in which party is not a consideration.

VII. A PROCEDURAL CONSIDERATION: OPPORTUNITY FOR ACCOMMODATION

One additional factor in the judicial processes of Michigan and Wisconsin must be mentioned: the procedural arrangements which may hinder or encourage accommodation of views among members of a judicial body. Where judges do not have partisan constituencies, the existence of such arrangements may diminish even further the likelihood of party orientation in judicial decision making. This seems to be the case in Wisconsin. On the other hand, an absence of institutional arrangements encouraging comity and compromise may heighten even further the tendency for judges' voting to divide along party lines in states where judicial constituencies are partisan.

One Wisconsin justice suggested that the existence of a resident court, with members living in Madison and spending their working hours in adjacent offices, may be an important reason for the sharp contrast in the degree of partisanship between the Wisconsin court and the Michigan court, which is non-resident. His reasoning points out that in the period between the assignment of cases and the initial conference, on the one hand, and the final conference, on the other, there is ample opportunity for the justice to whom the case has been assigned to consult informally with other members of the Court of both parties to try to find a common ground for decision, or at least to obtain as large a consensus as possible. This varies sharply from the Michigan practice in which justices of a non-resident court feel free to write opinions and circulate them to their colleagues even while the justice assigned the case is submitting his draft. Only when the opinions are actually signed by the judges does a majority opinion emerge.[69] Little opportunity for accommodation is provided by such an institutional framework.

A further arrangement which may affect the decisional process in the two states is the time limitation on dissent in Wisconsin. Michigan justices may write and submit opinions during a fairly extended period prior to the final conference. Their Wisconsin counterparts, on the other hand, meet at conference on Friday before a Tuesday decision day to review the draft opinion submitted by the justice assigned the case, and then to vote. If a justice wishes to dissent, even after the prior opportunities for accommodation, he must normally prepare his dissenting opinion by the following Tuesday, although delays are sometimes requested and granted.

These procedural arrangements provide opportunity for accommodation and may result in the reconciliation of conflicting viewpoints before the actual decision is rendered; or at least the time pressures may discourage dissent in some cases. Such factors will not eliminate the play of more important determinants, such as policy orientation or party or constituency, but the frequency and intensity of the impact of these determinants may be affected by the procedural arrangements which guide a court's decisional process.

VIII. CONCLUSION

The argument that prior party affiliation is a determinant of judicial conduct has normative implications of substantial importance. It suggests, for instance, that in jurisdictions which elect judges, the ballot should be partisan inasmuch as party label would accurately identify for voters the kinds of policies a judge is likely to make on the bench. Furthermore, a party variable would suggest that, even where elections are legally nonpartisan, both voters and campaign activists should choose candidates with whom they share a party preference since party affiliation is a good forecaster of judicial conduct. The combination of these normative

[69] The procedure on the Michigan court is described by S. Sidney Ulmer, "Leadership in the Michigan Supreme Court," in Glendon Schubert (ed.), *Judicial Decision-Making* (New York: The Free Press of Glencoe, 1963), pp. 16–17.

implications and the standing requirements of scientific accuracy should cause the party variable in judicial behavior to be treated quite gingerly.

Assertions of party influence in judicial decision making based solely on the association of judges' prior party identifications and their propensity to cast "liberal" or "conservative" votes are inadequate, both conceptually and methodologically. A conceptual framework for understanding the influence of parties in judges' decisions ought to give weight to the role of party identification and party activism in shaping policy outlooks. Situational factors such as family, socio-economic group, etc., influence the development of an individual's party identification which then becomes an important psychological factor in shaping his issue orientations. Party activism is a second situational factor, and one common among judges, which influences issue orientations, both because parties tend to recruit those who are in agreement with their general policy stances and because of the socializing process among party activists which tends to shape and re-enforce policy outlooks congruent with those of the party. Thus, through party identification and activism judges are influenced by party. However, a change in situational circumstances, such as that involved in becoming a judge, may invoke a new set of identifications and a new socializing process which affects policy outlooks. To understand the importance of party in judicial conduct it is therefore important to know the extent to which party remains salient for judges after they take office. The election process is one significant aspect of a judge's situation in which party may continue as an important reference group for him.

A second problem in the studies to date has been the adequacy of the methodological devices used to infer the role of party from the voting records of judges. The roll-call techniques of the legislative behavior studies might fruitfully be used by students of judicial behavior. Measures which emphasize the difference between members of the two parties acting as cohesive blocs more convincingly raise an inference of party influence than do techniques which show only the number of votes cast by a judge in a particular ideological or partisan direction.

Although the conceptual and methodological discussions in this study are general, the evidence used is limited to that adduced from a case study of judicial behavior in Wisconsin. This case study evidence is valuable because it occurs in a political system quite similar to the Michigan political setting which has been the basis for the best previous studies. In both states the justices had clear prior party identifications and were recruited from situations in which party was a significant factor. The Wisconsin evidence, however, shows none of the vigorously partisan conduct by the justices which is demonstrated by the Michigan studies. The different situational circumstances of judges in the two states is a reasonable explanatory variable. In Michigan, party continues to be an important factor in judicial behavior because of the partisanship of judicial constituencies and the absence of a resident court with procedural methods which encourage accommodation. In Wisconsin, the influence of party diminishes because judicial constituencies are bipartisan and the Court's procedures create a considerable personal interaction between the justices which maximizes the opportunities for accommodation and compromise.

Nevertheless, it must be recognized that the Wisconsin evidence in no way concludes the inquiry into the role of party in judicial behavior. It does, in my view, provide additional data which complement those arising from the Michigan study. The whole of the existing literature does not, however, establish a party variable in state judicial decision making. Its methods and conceptualization are for the most part not rigorous enough; the number of jurisdictions examined with adequate methods is insufficient; and the range of variables considered in most studies is too narrow. All that has been established is the need for a more careful conceptualization and more rigorous testing of a provocative hypothesis.

COMPARING POLITICAL REGIONS: THE CASE OF CALIFORNIA*

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One of the more fertile sources of data for systematic comparative political studies is in the regional differences that abound within political systems.[1] When such differences are large and politically significant, explaining them becomes intriguing and important, especially if their causes cannot be found in the more familiar classes of socioeconomic variables.[2]

Our purpose here is to present a preliminary analysis of a widely discussed intrasystem political difference—that between Northern and Southern California. This regional split has received particularly wide attention since the 1964 Republican primary, when Senator Goldwater's landslide majority in the South overcame his resounding defeat in the San Francisco Bay Area and insured his presidential nomination. Attempts to explain this pronounced regional variation have generated propositions about the political consequences of those social and economic conditions thought to be characteristic of Southern California. Since that area's most striking feature is its continuous rapid growth and economic development, many writers have been led to speculate that anxieties resulting from such changes lead to ultraconservative political preferences. These propositions are of considerable interest to students of politics, since neither economic growth nor its presumed attitudinal consequence is unique to Southern California, nor, for that matter, to the United States. We will examine various explanations for California's regional variation, with special emphasis on propositions about economic growth. Our data are from the 1960 Census, 1964 and 1968 election returns, and a series of statewide sample surveys conducted during the 1964 campaigns.[3] This article is not a comprehensive exploration of the problem; we have not, for example, systematically investigated historical evidence. But we have analyzed a good deal more current information than other writers on this subject and hence can advance the discussion beyond its present stage.

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[1] For a general statement of this theme see Sidney G. Tarrow, *Peasant Communism in Southern Italy* (New Haven: Yale University Press, 1967), pp. 10–11 and *passim*. Other examples of intrasystem comparisons include Juan Linz and Amando de Miguel, "Eight Spains," in Richard Merritt and Stein Rokkan (eds.), *Comparing Nations* (New Haven: Yale University Press, 1966); Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW, 60 (March, 1966), 73–82; and Norval D. Glenn and J. L. Simmons, "Are Regional Cultural Differences Diminishing?" *Public Opinion Quarterly*, 31 (Summer, 1967), 176–193.

[2] An example of an exercise very similar to ours is V. O. Key, Jr. and Frank Munger, "Social Determinism and Electoral Decision: the Case of Indiana" in Eugene Burdick and Arthur J. Brodbeck (eds.), *American Voting Behavior* (Glencoe: The Free Press, 1959), pp. 281–299.

I. REGIONAL POLITICAL DIFFERENCES IN CALIFORNIA

The conservative tone of political life in Southern California is well known. Radical right organizations flourish in the area, which is the prime source of out-of-state rightist mail received by Congressmen and Senators.[4] But the

[3] The survey data used in this article are from four sample surveys conducted by the Field Research Corporation's California Poll. These studies are more fully described in our "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," this REVIEW, 62 (September, 1968), 753–769.

[4] This statement is based on one of the authors' observations while working in several offices on Capitol Hill. Twenty-four per cent of the letters received by one national magazine in a radical right letter-writing campaign were from Southern California. See James McEvoy, III "Letters from

prophets of these movements find a cool and limited reception in the Bay Area, a celebrated center of liberal and radical political action. The distinctive character of political life in each of the two sections is illustrated by the comparative success of two new parties formed for the 1968 elections. By the summer of 1968 the left-wing Peace and Freedom party had 32,261 registered voters in the Bay Area, compared to only 6807 for George Wallace's American Independent party. The story was different in the Los Angeles metropolitan area, which had 27,012 people registered in the Peace and Freedom party and 51,290 under Wallace's banner. The same regional division is found at the elite level. For example, seven of the nine appointed regents of the University of California from Southern California voted to dismiss Clark Kerr as President of the University, compared to two of the seven from Northern California.[5]

Electoral behavior, although it does not do complete justice to the richness of either area's characteristic political style, provides the most convenient and precise measure of the cleavage and will be used hereafter as an index of the dependent variable. In 1964 Lyndon B. Johnson received 66 per cent of the presidential vote in the San Francisco Bay Area and 55 per cent in Southern California. Proposition 14, which repealed all fair housing legislation in the state (and subsequently was declared unconstitutional by the United States Supreme Court), was opposed by 42 per cent of the voters in the Bay Area and only 31 per cent in the South. In the 1964 Republican primary Governor Nelson Rockefeller won almost two-thirds of the vote in the North but only about two-fifths in the South. Four years later the liberal Republican incumbent Senator Thomas H. Kuchel was denied renomination by an even bigger conservative vote in the South, despite his three-to-two victory in the Bay Area. In the fall of 1968 Hubert H. Humphrey carried the San Francisco region handily but lost Southern California and the state[6] Table 1 presents returns for these five contests for the two major metropolitan areas in each of the two regions: San Francisco-Oakland and San Jose in the North, and Los Angeles-Long Beach and San Diego in the South.[7]

The importance of region as an independent variable is further demonstrated in an analysis of Proposition 14 returns from all incorporated Bay Area and Southern California cities of more than 2500 population. By assigning a numerical value of three to each Bay Area city and a value of one to each town in Southern California, we were able to compute simple and partial correlation coefficients to measure the relationship between region and votes against Proposition 14. The simple correlation coefficient (Pearson's r) is .55. When the vote for Johnson and Pierre Salinger (the Democratic candidate for Senator), the percentage of the labor force employed in manufacturing, median family income, and percentage non-white population are all partialled out, the coefficient is still .39.

The regional differences in the Republican primaries are somewhat larger than in the Proposition 14 vote and considerably larger than in the presidential balloting. This suggests that the split is greater among Republicans.[8] Other

the Right: Content-Analysis of a Letter Writing Campaign" (Ann Arbor: University of Michigan Institute for Social Research, 1966), p. 16.

[5] *San Francisco Sunday Examiner and Chronicle,* January 22, 1967, p. 8.

[6] While Southern California's greater conservatism has been fairly persistent in the recent past, the size of the regional differences in voting returns has not always been as great as in 1964 and 1968. For example, only four percentage points separated the two regions in the 1960 presidential election.

[7] The eight counties of Southern California contain 58 per cent of the state's population. The Los Angeles-Long Beach Standard Metropolitan Statistical Area (Los Angeles and Orange Counties) itself accounts for 43 per cent of all Californians. The other six southern counties resemble these two socially, economically, and politically. We will include respondents from all eight counties when analyzing survey data. With Census data and voting returns, it will be more useful to examine separately the two major metropolitan areas in the South: Los Angeles-Long Beach and San Diego.

We will compare Southern California not to the rest of the state, but just to the San Francisco Bay Area, which has 24 per cent of California's residents. With survey data, we define the Bay Area as one seven-county unit. For purposes of aggregate data analysis, we divide it into the San Francisco-Oakland SMSA and the San Jose SMSA. The reasons for this procedure will be apparent later in the article.

[8] Totton J. Anderson and Eugene C. Lee, "The 1964 Election in California," *The Western Political Quarterly,* 18 (June, 1965), p. 467. Fully a third of Bay Area Republicans intended, in late October, to vote for Johnson, compared to a 24 per cent defection rate among Southern California Republicans. The difference between the regions in Democratic defection was the same (but in the opposite direction): four per cent and thirteen per cent, respectively.

TABLE 1. VOTING RETURNS FOR SELECTED CALIFORNIA METROPOLITAN AREAS

	Northern California		Southern California	
	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
1964				
Per cent for Johnson	67	63	56	50
Per cent "No" on Proposition 14[a]	41	47	31	30
Per cent for Rockefeller	62	61	39	45
1968				
Per cent for Humphrey[b]	57	51	47	39
Per cent for Kuchel[c]	59	60	39	39

[a] "No" on Proposition 14 was a vote *for* fair housing.

[b] Per cent of the two-party vote. Percentages in this line were computed from the unofficial returns compiled by the Associated Press and published in the *San Francisco Chronicle*, November 7, 1968, p. 16.

[c] Per cent of all votes cast for Republican senatorial candidates. Both Senator Kuchel and his successful opponent, Max Rafferty, are from Southern California.

data point to the same conclusion: 44 per cent of Bay Area Democrats and 32 per cent of those in the South planned to vote against Proposition 14; the corresponding figures for Republicans are 33 per cent and 14 per cent, respectively.[9]

[9] These data, from the California Poll, are limited to white gentiles who had made up their minds how to vote by late October.

II. HYPOTHESES ABOUT SOUTHERN CALIFORNIA'S CONSERVATISM

We begin by noting (in Table 2) an array of demographic characteristics that do not distinguish between the two regions, or that differ in such small and inconsistent ways that they need not be referred to in the remainder of this article. Educational, occupational, and income levels

TABLE 2. POPULATION CHARACTERISTICS OF THE FOUR METROPOLITAN AREAS[a]

	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
Median family income in 1959	$7092	$7417	$7066	$6545[b]
Per cent in white collar occupations	51	51	49	50
Median school years[c]	12.1	12.2	12.1	12.1
Per cent aged 65 or older	9	7	9	7
Per cent foreign stock[d]	30	27	27	20
Per cent Negro	9	1	7	4
Per cent Oriental	4	2	2	1
Per cent Mexican-American[e]	6	12	9	6
Per cent white "anglo"[f]	82	85	82	89

[a] All data are from the 1960 Census.

[b] There was a considerable slump in the aerospace industry, San Diego's main source of manufacturing employment, in the late 1950's.

[c] For persons 25 or older.

[d] Persons born abroad or with one or both parents born abroad.

[e] The source for this line: U.S. Bureau of the Census, *U.S. Census of Population*: 1960. *Subject Reports. Persons of Spanish Surname. Final Report PC*(2)–1*B*. The Census Bureau estimates that virtually all people in California with Spanish surnames are of Mexican descent. Twenty percent were foreign born, 34 per cent native-born of foreign or mixed parents, and 46 per cent were native-born of native parents.

[f] People who are not Negroes, Orientals, or of Mexican descent.

are strikingly similar, as is the proportion of the population over 65 (in spite of the emphasis in some popular writings on Southern California's retired folk as a source of its political tone). The various indicators of ethnic composition—per cent foreign stock, per cent Negro, per cent Oriental, per cent Mexican-American, per cent white "anglo"—do not differ in ways that suggest even impressionistically satisfactory explanations for the kinds of political variations reported in Table 1.[10]

We now turn to the proposition, advanced with varying degrees of explicitness by numerous scholarly and popular writers, that a major key to the regional political variation is Southern California's explosive population growth and its socio-political consequences. A number of observers have noted the prevalence of right-wing political sentiment in Southern California on the one hand and the area's phenomenal expansion on the other, and concluded that the latter explains the former. These propositions about California take on added significance in that the state is commonly said to foreshadow trends in much of the rest of the country; they often are closely related to more general ideas about the sources of various contemporary political movements which seem to be particularly popular in Southern California.[11]

Southern California's population explosion, which has been in process for all of this century and shows no signs of abating, is said to produce various manifestations of right-wing behavior for several different reasons. These can be divided into propositions referring to the preferences of individual citizens in the aggregate, which we will call "mass propositions," and ones referring to community leaders, political activists, and, in general, richer and better-educated people. For shorthand purposes we will call the latter "elite propositions."

One familiar mass proposition suggests that the new arrivals who continue to flood into Los Angeles and vicinity are lonely, bewildered, and isolated. Having left communities in which they had roots, unable to establish meaningful new social relationships, frustrated by the absence of familiar institutions, they turn in their trauma to the certainties of patriotism and political fundamentalism. In this view it is the newcomers whose votes and activity give Los Angeles its conservative cast. Seymour M. Lipset has presented as evidence for this proposition the finding from a 1962 California Poll that 39 per cent of the one hundred Californians who said they supported the John Birch Society had lived in the state for less than fifteen years, compared to 29 per cent of the respondents who opposed it.[12] Perhaps because of the size of the subsample of Birch supporters, he did not control for region, an important consideration because Southern California has more Birch supporters and more newcomers than Northern California.

None of our 1964 data support the proposition that people new to California are more likely to be conservative. Statewide, 27 per cent of white Republicans who had lived in California at least fifteen years preferred Goldwater to all other Republican presidential candidates, compared to 24 per cent of the more recent arrivals.[13] Controlling for region produced no meaningful relationship; newcomers to Southern California were slightly (six per cent) *less* likely to be for Goldwater.

The presidential preferences of brand-new Californians in the general election can be learned from the votes of "new residents", which are counted separately.[14] In Southern California there was no difference between the voting of new residents and all other voters: both groups gave 55 per cent of their votes to Johnson. In the Bay Area 60 per cent of the new voters and 66 per cent of the older residents were for Johnson. This does not give much support to the proposition and clearly does not help explain the regional difference.

Our survey data show that recent arrivals

[10] While there is a slightly larger proportion of persons of foreign stock in the Bay Area, this difference is compensated for by the bigger Jewish community in Los Angeles.

[11] See, for example, Daniel Bell, "The Dispossessed—1962," in Bell (ed.), *The Radical Right*, Anchor edition (Garden City, N.Y.: Doubleday, 1963), p. 24; Seymour M. Lipset, "Three Decades of the Radical Right: Coughlinites, McCarthyites, and Birchers—1962," in *ibid.*, pp. 373–446; Lipset, "Beyond the Backlash," *Encounter* (November, 1964), pp. 11–24; Michael Miles, "Reagan and the Respectable Right," *The New Republic* (April 20, 1968), pp. 25–28; Talcott Parsons, "Social Strains in America: A Postscript—1962," in Bell, *op. cit.*, p. 234; Samuel C. Patterson, "The Political Cultures of the American States," *Journal of Politics*, 30 (February, 1968), p. 203; and Theodore H. White, *The Making of the President 1964* (New York: Atheneum, 1965), pp. 116–117.

[12] Lipset, "Three Decades," *op. cit.*, p. 437.

[13] Respondents were given their choice of all Republican presidential contenders, not just Rockefeller and Goldwater.

[14] "New residents" are people who have lived in the state for less than a year, were qualified to register in their previous state, and would be eligible to vote in California but for their recent arrival. They can vote only for President and Vice President. They probably are more interested in politics than the common run of registered voters.

TABLE 3. POPULATION AND POPULATION GROWTH IN THE FOUR METROPOLITAN AREAS

	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
1960 population	2,783,359	642,315	6,742,696	1,033,011
Increase in population, 1950–1960	24%	121%	54%	86%

to California have slightly more liberal attitudes toward Negro neighbors, particularly in the Bay Area. But there were no differences on attitudes toward the Rumford Fair Housing Act, which precipitated Proposition 14, or on voting intentions on the Proposition itself.

Californians characteristically move around a great deal from one residence to another. Perhaps this intrastate mobility contributes to those insecurities that presumably lead to ultraconservatism. If so, one might expect people who had lived in their present homes for a while to be less conservative. Our data do not support this idea either. White Republicans who had moved within the past five years were *less* likely (22 per cent to 30 per cent) to prefer Goldwater among all Republican presidential candidates. There was no difference between the two groups in attitudes toward the Rumford Act.

A second mass proposition about the effects of population growth is the reverse of the first one: the anxieties and hostilities caused by rapid, prolonged growth are felt more by the older residents, who resent not only the physical dislocations produced by expanding population, new traffic patterns, and changing land use, but also the social adjustments resulting from an influx of people of different cultural tastes, economic status, and, perhaps, ethnicity. The findings on newcomers and old-timers described above are faintly kinder to this proposition than to the first one, but given the small differences, these data do not support the argument that such outraged old settlers are the seedbed of Southern California conservatism.[15]

A third mass proposition holds that the "tensions of population growth" impinge on residents of booming areas irrespective of how long they have lived there. Since many of the social, geographic and economic consequences of rapid growth are felt generally by the population, their political corollaries may also be. Perhaps the association between rapid growth and conservatism is manifested in the impact of the supposedly fragmented social system in such areas on individual residents. To test this proposition one should compare all residents of rapidly growing areas with those of other areas, without distinguishing them as to duration of residence.

We can do this more satisfactorily with aggregate data than survey data. As Table 3 shows, the Los Angeles-Long Beach metropolitan area (Los Angeles and Orange Counties) grew more than twice as fast as the San Francisco-Oakland metropolitan area during the 1950's; and the greater conservatism of the Los Angeles region is clear.

But the relationship between growth and conservatism vanishes when we look beyond the two major urban centers to the pair of smaller metropolitan areas in the South and North. The San Diego metropolitan area, consisting of San Diego County, had a 1960 population of just over a million people. Despite a slump in its aerospace industries during the latter part of the decade, its population almost doubled from 1950 to 1960. The San Diego area's ten-year population increase was 86 per cent, considerably greater than the 54 per cent growth of the Los Angeles area. San Diego also exceeded Los Angeles in enthusiasm for Senator Goldwater and Proposition 14, as Table 1 shows.

During the same decade, however, the Northern California city of San Jose and its surrounding metropolitan area (Santa Clara County) grew at an even faster rate than San Diego. The

[15] A related proposition is that various recent manifestations of ultraconservatism reflect resentment by people from small towns and farms at the eclipse of older, simpler virtues and verities by the complex, cosmopolitan big city (see Lipset, "Beyond the Backlash," *op. cit.*, p. 20; and Bell, *op. cit., The Radical Right, passim.*). Despite its popularity, no data have been presented to support this proposition and systematic empirical research on followers of the radical right provides no support for it. See Raymond E. Wolfinger, Barbara Kaye Wolfinger, Kenneth Prewitt, and Sheilah Rosenhack, "America's Radical Right: Politics and Ideology," in David Apter (ed.), *Ideology and Discontent* (New York: The Free Press, 1964), p. 281; and Sheilah R. Koeppen, "The Radical Right and the Politics of Consensus," in Robert A. Schoenberger (ed.), *The American Right Wing* (New York: Holt, Rinehart & Winston, 1969).

area's population increased by 121 per cent from 1950 to 1960, yet Santa Clara County voted like the rest of the Bay Area, giving 63 per cent of its vote to Johnson, 61 per cent to Rockefeller, 60 per cent to Kuchel, and voting 47 per cent "no" on Proposition 14. Thus conservatism does not appear to be associated with massive population growth.[16]

These data suggest two alternative verdicts on the proposition that rapid regional growth produces conservatism. The stronger conclusion is that the proposition is just wrong, for, as Tables 1 and 3 show, San Jose is last or next to last on all three indices of conservative voting, yet it is a strong first among the four metropolitan areas in rate of growth. A second interpretation is that, despite the fifty miles separating them, San Jose is really "part of" San Francisco and its spectacular economic boom has not had the customary political consequences on its residents because of the moderating influence of the older metropolis.[17] This line of explanation requires an important modification of the flat proposition that rapid growth produces ultraconservative political behavior, by introducing the notion that "older civilizations" can ameliorate the results of this growth. It is difficult to imagine this civilizing effect occurring at the mass level, although its effects would be felt there. If it exists, it must be mediated through elite interaction.

This brings us to another type of economic-expansion explanation for the California cleavage, the argument that economic booms produce leadership groups characteristically lacking some of the qualities of elites in more stable areas. The superior political virtue of old wealth has been a familiar theme in speculative writings on McCarthyism and the radical right.[18] As Lipset puts it:

> Although Northern California has continued to increase in population, its major center, San Francisco, has grown little for many decades. There are many old families in the Bay Area who represent four and five generations of wealth. . . . Wealth in Los Angeles, on the other hand, is almost exclusively *nouveaux riches,* and the well-to-do there possess the attitudes toward politics and economics characteristic of this stratum.[19]

New, rapidly expanding centers of population typically lack a leadership structure accustomed to the responsibilities of running community institutions, reducing community tensions, and supporting the rights of various groups to share in community decisions and authority.[20]

As we will show, what we loosely call "elites" in Southern California are far more conservative than their opposite numbers in the North. We question, however, Lipset's explanation of the causes of this difference.

For one thing, his comparison of San Francisco and Los Angeles is somewhat overstated. San Francisco has stopped gaining population in good measure because the city limits, which are firmly defined by the physical setting, were reached by construction almost two generations ago. The Los Angeles city limits, on the other hand, are not so constrained by geography and have included vast tracts of undeveloped land. Los Angeles first eclipsed San Francisco in population in the 1920 Census. It would seem, therefore, that the present generation of wealthy families there cannot be "almost exclusively *nouveaux riches.*" By the same token, the Bay Area's economy has hardly stagnated in the past half century, and so one would expect to find a good deal of first-generation wealth in San Francisco, since the city has continued to be the business center of the area, as well as the financial headquarters of the West.

The conventional wisdom holds that the old rich are likely to have a variety of characteristics conducive to the formation of moderate political attitudes. The rough-hewn, first-generation captain of industry is said to be followed by descendants who indulge the opportunity to acquire the finest educations and consciences and are moved to noblesse oblige by guilt and fashion. These offspring are likely to turn their productive enterprises over to professional managers who, since they are disinterested experts, view social reform with more equanimity than the possessive, insecure men who actually make fortunes. The more bureaucratic, older corporations hire trained experts, use rational planning techniques, and, accustomed to dealing with the government, do not regard it with hostility. The *nouveaux riches* are the opposite of all this. Having come up from the bottom, they tend to be less educated; having had to scramble hard for their money, they find it difficult to modify their acquisitiveness with social conscience and

[16] "Right-wing extremism, and presumably Goldwater Republicanism as well, is related to the tension of population growth and lack of community integration" (Lipset, "Beyond the Backlash," *op. cit.,* p. 20.).

[17] Aside from a few people in the extreme northern fringe, residents of Santa Clara County (San Jose) do not commute to work in San Francisco.

[18] See, e.g. Bell, *op. cit., The Radical Right, passim.*

[19] Lipset, "Three Decades," *op. cit.,* pp. 436–437.

[20] Lipset, "Beyond the Backlash," *op. cit.,* p. 20.

TABLE 4. REGIONAL DIFFERENCES IN OPPOSITION TO PROPOSITION 14 AMONG THE BETTER EDUCATED AND LESS EDUCATED, BY PARTY[a]

		Per Cent Voting No:		
		Bay Area	Southern California	Association of Region & Vote (Kendall's Q)
Respondents with 3 or more years of college:	Democrats	79%	57%	.485
	Republicans	44%	18%	.560
Respondents with less than 3 years of college:	Democrats	36%	28%	.176
	Republicans	26%	13%	.396

[a] This table includes only white gentile respondents who had made up their minds on Proposition 14 by late October.

are likely to see any political disagreement as a threat to their status and a prelude to revolution.[21]

The foregoing line of thought, which Lipset uses in explaining elite conservatism in Southern California, is in fact quite irrelevant to the subject. Economic growth there, as in the rest of California, is based on "scientific" industries like electronics and aerospace. Firms in these fields typically have very large proportions of professional employees, lay heavy emphasis on rational calculation in decision making, and deal extensively, if not exclusively, with the federal government. The principals in these enterprises often are scientists and engineers. None of this is to say that such businessmen are more liberal, but only that they are a far cry from the traditional image of the *nouveau riche*.[22]

Much more than Los Angeles, San Jose would seem to be a city whose leaders are not "accustomed to the responsibilities of running community institutions." Yet as we have seen, San Jose's voters were no more conservative than those in the presumably staid remainder of the Bay Area; and on Proposition 14 they were more liberal. This suggests that, at the very least, San Jose's inexperienced "leadership structure" was somehow socialized by its proximity to San Francisco's Brahmins. The more drastic—and more plausible—conclusion is that rapid expansion is not a determinant of conservatism.

While we doubt Lipset's hypothesis about the

[21] *Ibid.*, pp. 19-20. While these presumed political characteristics of *nouveaux riches* may seem to resemble those often attributed to all upward mobile groups, it may be useful to view them separately. One assumes that a new millionaire may be somewhat different from a member of the middle class with working-class parents. In any event, the familiar contention about upward mobile members of the business and professional classes appears to be unfounded. Data showing that the upward mobile are not more ethnically prejudiced have been available for some time (see, e.g., Bruno Bettleheim, *Dynamics of Prejudice* (New York: Harper & Row, 1950), p. 59); more recently the vast preponderance of published research has cast doubt on the notion that upward mobility produces ethnic prejudice. The most extensive study of this point is James A. Barber, Jr., *Social Mobility and Political Behavior* (Chicago: Rand-McNally, forthcoming). Analyzing data from the University of Michigan Survey Research Center's studies of the 1960, 1956, and 1952 elections, Barber found that upward mobile members of the middle class were, compared to status-stable members of the same class, *less* likely to be Republicans and to have conservative attitudes on various political issues. For data showing that supporters of the radical right are, if anything, *less* upward mobile, see Wolfinger *et al.*, *op. cit.*, p. 278; and Koeppen, *op. cit.* The same seems to be the case with Senator Goldwater's followers. See James McEvoy, III, *Radicals or Conservatives: A Study of the Contemporary American Right* (Chicago: Rand McNally, 1969).

[22] Propositions about elites cannot easily be verified with data from surveys of the general population since there are seldom enough elite respondents. The same is true here, but we can use these data to examine the related proposition that self-employed individuals tend to be more conservative than people who do not work for themselves. Examining Republicans and Democrats separately (and excluding non-whites and Jews), we found that the self-employed were no more likely to vote for Proposition 14. Self-employed Republicans showed a slightly greater preference for Goldwater among all Republican presidential possibilities (30 per cent to 24 per cent).

TABLE 5. VOTING BEHAVIOR OF HIGH-INCOME CITIES IN SOUTHERN CALIFORNIA AND THE BAY AREA

	Per Cent Registered Democrats[a]	Per Cent for Johnson	Per Cent "No" on Prop. 14[b]
Southern California			
Mean for region[c]	20	24	20
Palos Verdes Estates	21	28	25
Rolling Hills Estates	29	29	22
San Marino	11	16	14
Beverly Hills[d]	53	65	50
Bay Area			
Mean for region	27	45	40
Atherton	21	42	42
Hillsborough	21	38	35
Los Altos	32	51	43
Los Altos Hills	32	52	52
Piedmont	24	35	29
Ross	30	50	42
Woodside	30	45	40

[a] Registration figures are for the 1964 general election, computed on a base of all registered voters.
[b] A vote against Proposition 14 was a vote *for* fair housing.
[c] The regional means exclude Beverly Hills.
[d] The population of Beverly Hills is predominantly Jewish; 51 per cent of the city's residents were foreign stock in 1960.

causes of the greater conservatism of elites in Southern California, there is no question at all that they are indeed more conservative. If we define "elites" very loosely as people with more money or education, this assertion is easily demonstrated empirically. Table 4 shows regional differences in opposition to Proposition 14 of respondents with three or more years of college and those with less education, with party identification controlled. The regional differences between the better educated are 22 per cent for the Democrats and 26 per cent for the Republicans. They are far smaller among respondents with less education: eight and thirteen per cent, respectively. The strength of the relationship between region and the Proposition 14 vote for each of the four groups can be measured more precisely by means of Kendall's Q. With this measure a value of 1.0 (or -1.0) describes a perfect relationship.[23] As Table 4 shows, the Q scores for the better-educated respondents are considerably larger than for the rest of the sample. These data show that the North-South split in California is much more pronounced among the better educated. They also show that the split is greater among Republicans than Democrats, a point we will return to later.

We cannot explore this subject further with the available survey data,[24] but a combination of Census data and voting returns gives persuasive support to the proposition that regional differences are far greater among upper status groups. Table 5 compares the voting of the most fashionable suburbs in Southern California and the Bay Area, those cities in the two regions with median family incomes in excess of $10,000. Looking at the table, it is difficult to escape the conclusion that these two sets of cities have very different prevailing political styles. Not only are the southern towns far more conservative, but, as a comparison of Tables 5 and 1 shows, the gap between them and their northern counterparts is enormously greater than the overall difference between the two regions. This aggregate data comparison, like the contrasting survey data, suggests that the elite regional split is most strongly affected by differences among Republicans. Residents of both sets of towns are overwhelmingly Republican: four to one in the

[23] See Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill, 1960), pp. 225–231.

[24] The surveys did not include questions on respondents' income and we found the occupation code unsatisfactory for purposes of the present analysis. There were too few cases to permit valid analysis of primary preferences for Goldwater among the highly educated. There was no discernible tendency for a greater proportion of better-educated respondents in the Bay Area to be Democrats (but see Table 5).

South and almost three to one in the Bay Area. The regional difference in Republican registration is not very great, but the northern towns were twice as liberal on Proposition 14 and in the presidential election. Given the small difference in Democratic registration, it is very likely that most of this represents Republicans forsaking Goldwater and Proposition 14.

III. TOWARD A SATISFACTORY EXPLANATION

Drawing on the preceding analysis of socioeconomic variations in the regional political split, we can now point to a line of reasoning and investigation that might be pursued in a more satisfactory explanation of California's two political regions.

First, it needs to be emphasized that from a simple statistical standpoint upper-income (or upper-education) groups contribute disproportionately to the regional cleavage. The regional differences in liberalism-conservatism exist at *all* income levels, but if the mean differences were no greater than the gap between poorer people in the two regions, the subject of this article would have attracted far less attention.[25]

Second, we may ask how these regional differences in political behavior are maintained, putting aside for the moment the question of how they arose historically. A good explanation might take the following general form (and we emphasize that the necessary research has yet to be done): The environment of political communication is more conservatively "colored" in Southern than in Northern California. By and large, newspapers in the South have been more conservative than in the Bay Area.[26] Right-wing organizations are far more active, numerous, and vociferous in the South—although in all areas of the state the percentage of the people engaging in such activity is not large. Given the class distribution of exposure to the mass media and political information-seeking, we would expect richer individuals (and communities) to be disproportionately influenced by these environmental effects. But there would be some environmental impact in poorer communities and, in addition, the conservative inclinations of wealthier Southern Californians would become diffused because political and civic leaders and activists are drawn mostly from such groups and because their views spread to other people through business relationships, community organizations, prestige suggestion in interpersonal relations, letters to the mass media, and political activity and contributions. The reciprocal of this argument would, of course, apply to the Bay Area.

The regional split appears to be smallest in partisan general elections, where the party label is a strong guide to voting choices. In primaries and referenda, where political stimuli are not so mediated by party identification, those influences associated with the regional political cultures are likely to have more impact.

Third, there is the question of the historical origin of the regional differences. One not uncommon argument is that San Francisco has long been a stronghold of labor unions, while Los Angeles, with a far different early economic history, was notoriously an "open shop town" until the Second World War. While this is clearly true, regional political differences among working-class voters are relatively small. As a caution to hasty generalization, we should also point out that the strong San Francisco labor movement was responsible for much of the anti-Oriental agitation in that city and in the state from the latter nineteenth century through the First World War.

A second line of historical explanation points to the different sources of immigrants to the two

[25] Bay Area cities with 1959 median incomes of less than $6000 were only six percentage points more hostile to Proposition 14 than comparable cities in Southern California, while the parallel difference among cities in the over-$8000 category was 19 per cent.

[26] The *Los Angeles Times* has a position of dominance in Southern California far exceeding that of any newspaper in the Bay Area. The *Times* has become relatively more moderate recently (although it favored both Proposition 14 and Goldwater over Johnson), but until the last few years it followed an ultraconservative line in both news and editorial columns.

> Regional differences in newspapers' political orientation are particularly noticeable in the suburban press, as is illustrated by the following passage from a *Newsweek* story on Orange County, described as "a monument of monolithic conservatism":
>
> Pervasive, rather than powerful, is the word for the county's conservative journalism. R. C. Hoiles' "Freedom Newspapers" . . . reflect the creaky views of their 87-year-old owner. "Tax-supported schools violate the Ten Commandments and the Golden Rule," he says, and then advises that the entire public-school system be handed over to "private enterprise."
>
> But Hoiles' papers, and a few other conservative journals, are all that is available in local journalism. Conservatives have the ball game almost to themselves. Liberals—not to mention moderate Republicans or Democrats—are almost literally nowhere. (*Newsweek*, November 14, 1966, p. 37).

TABLE 6. SOURCES OF NEW ARRIVALS IN THE FOUR METROPOLITAN AREAS—1955–60

	San Francisco–Oakland	San Jose	Los Angeles–Long Beach	San Diego
1960 population	2,783,359	642,315	6,742,696	1,033,011
Total new arrivals, 1955–60[a]	416,647	203,066	1,048,175	326,699
Per cent of new arrivals from southern and border states[b]	19	11	22	25
Per cent of new arrivals from the Midwest[c]	15	12	28	23
Per cent of new arrivals from elsewhere in California	36	56	19	26
Per cent of new arrivals from the Northeast	10	8	16	10

[a] 1960 residents of the given metropolitan area who lived elsewhere in 1955.

[b] 1960 residents of the given metropolitan area who lived in the southern and border states in 1955. The border region includes Delaware, Maryland, the District of Columbia, West Virginia, Kentucky, Oklahoma, and Missouri.

[c] The largest regional differences in out-of-state migrants are found in the contribution of the Great Lakes states, which is far bigger to Southern California.

Source: 1960 Census.

regions, now and/or at some crucial period in the past. After the polyglot Forty-Niners, Northern California was settled by migrants from the northeast and Europe, while Los Angeles' first sizable influx included many more Midwesterners, presumably more Protestant, agrarian, conservative, and intolerant.[27] Some historians attribute the great Progressive election victory of 1910 to a continued flow of moralistic Midwesterners to Southern California.[28] The same distinctive sources of immigration continued in the period between the wars.[29] The last United States Census to include questions about religious affiliation, taken in 1936, revealed that two-thirds of the church members in the Bay Area were Catholics, compared to 36 per cent in the Los Angeles metropolitan area and 44 per cent in San Diego County. Moreover, Bay Area Protestants were evenly divided between fundamentalist and more liberal denominations, while in Southern California half again as many Protestants belonged to the more fundamentalist churches. In 1964 Catholics comprised 40 per cent of the white California Poll respondents in the Bay Area and 24 per cent in Southern California.[30] However there seems to have been something of a convergence in the *sources* of out-of-state immigrants. Table 6 shows the locations in 1955 of those people who moved to the four major metropolitan areas between 1955 and 1960. By and large the Southland has somewhat more new settlers from *both* the South and Northeast, considerably more from the Midwest, particularly from the Great

[27] Daniel J. Elazar makes particularly imaginative use of the differential migration theme. See his *American Federalism: A View from the States* (New York: Crowell, 1966), pp. 104–105. He does not, however, explain why these different sources would produce conservatism in the South and liberalism in the North.

[28] See, e.g., Franklin Hichborn, *California Politics—1891–1939*, vol. 2 (Los Angeles: Haynes Foundation, no date), pp. 904–905, 1072, ch. 50. The tendency for yesterday's progressives to become today's radical right has been discussed—perhaps beyond the available evidence—by Richard Hofstadter and other social commentators. Nevertheless, this points to the need for a careful examination of indices of conservative and liberal behavior in California in the past two generations. Some measures that might be considered "liberal," such as the Townsend Plan, fared better in Southern California. *Cf.* Carey McWilliams, *Southern California Country* (New York: Duell, Sloan and Pearce, 1956), chap. 14.

[29] James Q. Wilson, "A Guide to Reagan Country: The Political Culture of Southern California," *Commentary* (May, 1967), p. 38.

[30] Since many nominal Protestants, unlike Catholics, are not formally members of a church, measures of religious affiliation which rely on church membership inevitably inflate the proportion of Catholics in the population. Therefore the survey data are a more accurate measure of the absolute number of Catholics and Protestants.

Lakes states, and far fewer migrants from other parts of California itself. Half the residents of both northern metropolitan areas were born in California, compared to a third for each of the two southern areas.

Granted that there are more Catholics and native Californians in the Bay Area, and more fundamentalists, Southerners, and Midwesterners in the Southland, what are the political consequences? Using Census data and voting returns, we correlated voting on Proposition 14 and the percentage of the population born in California for all incorporated cities in the state, and found no relationship between these two variables. We have no data on individual respondents' original places of residence, which would let us see if this variable is related to contemporary political behavior, but we can compare our survey respondents' political preferences and religious affiliations. The data, while not adequate for anything but suggestive purposes, do indicate that part of the regional political difference *may* be a result of different backgrounds of residents of the two areas. As we would expect, California Catholics are more likely to be Democrats than are Protestants.[31] In the Bay Area 51 per cent of white Protestants are Democrats, compared to 78 per cent of the white Catholics. In Southern California the corresponding figures are 44 per cent and 74 per cent.[32] When we examined these respondents' attitudes toward the Rumford Fair Housing Act, controlled for party identification, religion, and region, the number of Republicans dwindled so as to preclude meaningful analysis. The number of Democratic cases in each cell is almost as small, but the data indicate that both religion and region were associated with support of the Rumford Act. In Southern California 26 per cent of the Protestants and 42 per cent of the Catholics favored it; in the Bay Area, 39 per cent of the Protestants and 49 per cent of the Catholics.[33]

[31] For data on Catholics' Democratic proclivities see Bernard R. Berelson, *et al., Voting* (Chicago: University of Chicago Press, 1954), pp. 61–71; Angus Campbell, *et al., The Voter Decides* (Evanston: Row, Peterson & Co., 1954), p. 71; Angus Campbell, *et al., The American Voter* (New York: Wiley, 1960), ch. 12; and Scott Greer, "Catholic Voters and the Democratic Party," *Public Opinion Quarterly*, 25 (Winter, 1961), 611–625.

[32] These percentages are computed on a base of respondents who identified with one major party or the other.

[33] These percentages are computed on a base of respondents who had an opinion on the Rumford Act.

In short, there are proportionately more Catholics in the Bay Area than in Southern California, and the more Democratic, liberal inclinations of the former section may be accounted for to some extent by this demographic difference. It seems likely to us that the regional religious variation reflects a more general difference in the background of settlers in the two sections: more cosmopolitan and urban in the North, more rural in Los Angeles. Yet, tempting as this conclusion is, we mention it with considerable trepidation. It is based on a scanty number of cases and involves only a single, possibly misleading index of liberalism. The Catholic Church exerted great efforts in behalf of the fair housing cause in the 1964 election; we have no way of knowing how much of the Catholic voters' support of the Rumford Act was due to this pressure and consequently cannot be interpreted as a sign of more general liberal attitudes. Indeed, in the country as a whole Catholics do not seem to be more favorably inclined toward fair housing. A Gallup Poll taken in 1967 showed no differences between Catholics and Protestants in their opinions of a federal fair housing bill (both groups were decisively against it);[34] if the southern respondents were removed from this sample, doubtless the result would be to make the remaining Protestant respondents considerably more liberal than the Catholics. Finally, it should be noted that the regional difference in the views of white Protestant Democrats was considerable: 26 per cent in Southern California and 39 per cent in the Bay Area favored the Rumford Act. Hence religion (and whatever regional differences in population characteristics it represents) is not the sole explanation of the regional split we have been attempting to analyze.

In addition to the propositions about historical origins, there are what might be called "intermediate historical propositions" which may prove to fit into whatever causal explanations are eventually developed. Thus once a region gains a reputation for a particular life style—in politics or whatever—its cultural and political climate may be reinforced by selective migration. Business and professional men have more freedom to pick their own homes than most other people, and "the kind of people" who find Southern California more congenial may be more conservative than those who, also able to choose their home, like the Bay Area better.

While we cannot provide firm affirmative propositions explaining the roots of the striking regional differences in California politics, we can be fairly sure about three statements: (1)

[34] Helen Erskine, "The Polls: Negro Housing," *Public Opinion Quarterly*, 31 (Fall, 1967), p. 491.

Southern California's conservative orientation is not a consequence of its rapid economic development and population growth. (2) The differences in conservatism between Southern and Northern California are at least partially independent of gross differences in the present population characteristics of the two regions. (3) These political differences are determined in good measure by the different political preferences of leaders and activists in the two regions.

These findings should reassure those who fear that illiberal political movements are the consequences of vertical and horizontal social mobility, economic growth, and suburbanization—trends which are likely to be with us for some time to come.

ALIENATION AND POLITICAL BEHAVIOR*

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I. INTRODUCTION: ALIENATION

Alienation is both one of the most popular and vague concepts used by contemporary social scientists. Scholars often cite Robert Nisbet's statement that alienation is basically a perspective.[1] The current age is said to be one of alienation, or writers declare alienation to be the fundamental interpretive concept for explaining deviant behavior. One author has even gone so far as to say that definition is unnecessary because we can all feel what "it" is in our very bones. Indeed, if we don't understand it intuitively we are alienated by definition.[2]

Recently, there have been a series of attempts to clarify the meaning of the term. Daniel Bell, commenting on the uses of the concept alienation in the works of Marx, distinguishes *estrangement* ("a socio-psychological condition") from *reification* ("a philosophical category with psychological overtones").[3] For research purposes, the fundamental difference between these meanings lies in the criteria which are applied in determining whether an individual is alienated. The existence of estrangement is determined by investigating the attitudes of individuals; reification is measured against "objective" standards about the quality of human life established by the investigator.

The reification (objective) tradition has many strong exponents.[4] It offers a potentially powerful concept to an analyst wishing to evaluate the human condition in terms of explicitly stated criteria of what man *ought* to be in his social and personal relationships. Most of the contemporary scholarly work, however, is concerned with estrangement, and my own interest also lies in the individual's perception of the situation he faces.

Merely limiting ourselves to determining the existence or degree of alienation by gauging the attitudes of individuals is but a small step forward in clarifying what we mean by the term. Fortunately, two contemporary scholars have laid a solid foundation for a more adequate conceptualization. Melvin Seeman's "On the Meaning of Alienation" systematically isolates five "basic ways in which the concept of alienation has been used" in the literature.[5] He suggests an effort to explore the interrelationships between operational measures of these variants and also the causes and consequences of them.[6] Kenneth Keniston, in a very concise appendix to his study of alienated youth, suggests that[7]

> . . . while the concept of alienation in every variation suggests the loss or absence of a previous or desirable relationship, it requires further specification in at least four respects:

* This article is adapted from my doctoral dissertation prepared at Yale University under the supervision of Professor Robert E. Lane. I am grateful to Professor Lane for his invaluable help at every stage of the research and I would also like to thank Paul Conn and my colleagues M. Kent Jennings, Robert A. Schoenberger and Jack L. Walker for comments on an earlier draft of this paper. The data used here are from national surveys conducted by the Survey Research Center at The University of Michigan and made available through the Inter-University Consortium for Political Research.

[1] Robert A. Nisbet, *Community and Power* (New York: Oxford University Press, 1962), p. 15.

[2] Gerald Sykes (ed.), *Alienation: The Cultural Climate of Our Time* (New York: George Braziller, 1964), Volume 1, p. xiii.

[3] Daniel Bell, "The Rediscovery of Alienation: Some Notes Along the Quest for the Historical Marx," *Journal of Philosophy*, 56 (1959), pp. 933-934.

[4] See, for example, Erich Fromm's introduction to Erich Fromm (ed.), *Marx's Concept of Man* (New York: F. Ungar Publishing Co., 1961); and C. Wright Mills, *White Collar* (New York, Oxford University Press, 1951).

[5] Melvin Seeman, "On the Meaning of Alienation," *American Sociological Review*, 24 (1959), p. 783. The five "variants" (as Seeman calls them) are: 1) Powerlessness; 2) Meaninglessness; 3) Normlessness; 4) Isolation; and 5) Self-Estrangement.

[6] Several of Seeman's students and others inspired by him have been particularly interested in empirical investigations of the dimensionality of these variants. See, in particular, Dwight G. Dean, "Alienation: It's Meaning and Measurement," *American Sociological Review*, 26 (1961), 753-758; and Arthur G. Neal and Salomon Rettig, "On the Multidimensionality of Alienation," *American Sociological Review*, 32 (1967), 54-64.

[7] Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York: Harcourt, Brace and World, Inc., 1965), p. 454.

1. *Focus:* Alienated from what?
2. *Replacement:* What replaces the old relationship? (This might more crisply be referred to as the *form* of the alienation.)
3. *Mode:* How is the alienation manifest?
4. *Agent:* What is the agent (cause) of the alienation?

It is evident, as Keniston points out, that these four questions "provide a basis for a virtually limitless number of varieties of alienation."[8] A person must naturally wonder if it might not simply be better to discard the term. This solution is quite appealing, but Keniston speaks of the certainty in this case "that the same problems of definition would crop up again and again with some cognate term like estrangement, disaffection, or detachment."[9] An even more important reason for at least temporarily retaining the term, I think, is that it has been employed in a great number of widely used theories and one task of scholars at this point should be to clarify when and how various forms of alienation do serve as important independent or intervening variables and whether these attitudes are meaningfully structured in people's minds. The Keniston scheme is valuable for empirical and theoretical purposes because it stimulates definitional and measurement refinements (i.e., looking at alienation in terms of form [subject] and focus [object]), and emphasizes the place of the various types of alienation in a chain of sources and consequences.

In this paper I have taken operational measures of two commonly accepted forms of alienation (trust and efficacy),[10] in each case provided two foci for each form, and examined the relationship between individuals' scores on these indicators and their political behavior. I am trying to discover the value of measuring alienation in this way—i.e., do the various forms of alienation lead to different types of behavior and does the focus of the measurement make a difference?

While a test of the utility of Keniston's scheme in empirical research is valuable in itself, there is a broader set of questions raised in the existing literature on alienation and political behavior. Basically, these concern the conditions which stimulate the political mobilization of the alienated. Therefore, before presenting the data, I will review the relevant literature, giving special attention to discussions of the political settings in which attitudes of alienation are important determinants of behavior and to contradictions in research findings which appear to result from inadequate consideration of the forms and foci of alienation. This review emphasizes the literature on American political behavior since it is the subject matter of the most systematic research and also the setting for the data employed here. I begin with authors who do not use direct measures of both alienation and behavior in establishing relationships and then consider those who do.

[8] *Ibid.*, p. 454.

[9] *Ibid.*, p. 452.

[10] For an extended discussion of the forms of alienation commonly used in the literature and a critique of Seeman's article, see Joel D. Aberbach, *Alienation and Race* (unpublished Ph.D. Dissertation, Yale University, 1967), especially pp. 7–12 and 71–74.

II. SOME VIEWS IN THE LITERATURE

One prominent interpretation is that the alienated (the term is usually very broadly defined) are quiescent under ordinary circumstances, but subject to mobilization into mass movements (or at least support of extremist, demagogic and/or authoritarian political leaders) when the material or psychological circumstances are proper and the right leader presents himself.[11] Empirical studies done in the United States on support for extremist leaders and groups do not present a very clear picture. Thus, while Nelson Polsby found that Senator McCarthy gathered the bulk of his support in areas "where the Republican Party was stron-

[11] See, for the best known example, William Kornhauser, *The Politics of Mass Society* (Glencoe: Free Press, 1959). For Kornhauser, alienation is described rather indiscriminately in several forms. He employs operational measures of political inefficacy, social distrust and low ego strength in his secondary analyses and alludes to expressions of hostility and resentment against established institutions in the text. (See, among others, pp. 109–112, 148 and 166.) Economic dislocation, social shock caused by events like defeat in war and/or social confusion accompanied by demogogic attack on the existing political system can arouse the public susceptible to mass appeals. The mass man (who suffers from "a lack of proximate attachments") lacks internalized standards and is, therefore, politically volatile. When there are no clear signals available his anxiety is mixed with political apathy, but spasmodic "flights into activity" are also characteristic. Precise predictions about the behavioral and psychological results of alienation are not made by the theory except at the extremes of a continuum which runs from no signals (resulting in apathy) to appeals in time of acute societal distress (yielding active mass movements).

gest"[12] and that his reputed influence on elections outside of Wisconsin had no basis in fact, Michael Rogin's study of George Wallace's 1964 Democratic presidential primary campaign in Wisconsin demonstrated that Republican crossover voters (particularly in middle- and upper-class districts) gave him substantial support.[13] The implication of the first study is that McCarthy's impact on the behavior of voters was negligible, but the second indicates that many Republicans may compose a natural reservoir of support for classic American demagogues.[14] Some substantiation of the latter point is found in the Wolfinger, *et al.*, study of Christian Anti-communist Crusaders where the most salient characteristic of the respondents studied was that they were Republicans.[15]

A more general principle may do something to reconcile any surface contradictions in these studies: In clearly structured general elections, standard reference symbols like party and group govern the direction of voting behavior; but in situations where these symbols provide no clear guide (primaries, nonpartisan elections, decisions to join movements of various types), other factors can assume great importance. In the Rogin (Wallace) case, many Republicans were people prone to "express their general resentment against outside interference in their lives—from government as well as Negroes"[16] and gave this feeling free reign (along with certain working-class Democrats) in a primary where an issue appropriate to this sentiment was raised, while in the Polsby (McCarthy) case voting was more clearly on party lines and motivations based in resentment were more obscure.

Support for this interpretation can be found in the literature on the impact of personality on politics ably summarized by Greenstein.[17] Drawing on the writings of Lane, Shils, Christie and Jahoda, Levinson and others in the field, Greenstein stresses that an unambiguous environment reduces the opportunities for personal variations in behavior. Where the situation is clearly structured—for example, an attractive reference group takes a clear and definite stand, or strong sanctions are attached to certain behavior alternatives—the scope for personality to affect behavior is severely limited. However, where salient reference groups have conflicting points of view, or "current situations . . . are in conflict with previous experience," etc., the individual's clues are not clear and personality factors can play a key role.[18]

Writers employing psychological measures of alienation present evidence which appears to confirm this general principle. For example, detailed investigations of referendum voting consistently emphasize the negativism of the politically alienated "lead [ing] to an attitude on a given issue which represents a protest against

[12] Nelson W. Polsby, "Toward an Explanation of McCarthyism" in Nelson W. Polsby, Robert A. Dentler and Paul A. Smith (eds.), *Politics and Social Life* (Boston: Houghton Mifflin Company, 1963), p. 816.

[13] Michael Rogin, "Wallace and the Middle Class: The White Backlash in Wisconsin," *Public Opinion Quarterly*, 30 (1966), 98–109. On page 100 he says that "In Wisconsin, a state where Republicans can vote in the Democratic primary, Wallace's vote closely resembled the normal Republican vote." This analysis involves ecological correlations and thus all of the risks which go with it but Polsby's basic technique is similar.

[14] *Ibid.*, p. 106. Rogin, it should be noted, points to the fact that there were some differences in the support attracted by Wallace and McCarthy. Wallace was stronger in working-class areas, while McCarthy did much better in rural areas. But he emphasized that "each was strong among conservative Republicans" (p. 105). This is a deduction supported by some reasonable evidence, but Rogin discounts the unpopularity of Governor Reynolds who stood in for Johnson in the primary and the possibility that Republicans crossed over to vote for Wallace in order to embarrass the Democrats. These data, of course, cannot be used to make a definitive conclusion in this area.

[15] Raymond E. Wolfinger, Barbara Kaye Wolfinger, Kenneth Prewitt, and Sheilah Rosenhack, "America's Radical Right: Politics and Ideology," pp. 262–293, in David E. Apter (ed.), *Ideology and Discontent* (New York: The Free Press of Glencoe, 1964). See especially, p. 288. The subjects in this study felt quite efficacious politically (and, therefore not alienated in the powerlessness sense), were fundamentalist Goldwater supporters and "old-fashioned individualists."

[16] Rogin, *op. cit.*, p. 106.

[17] Fred I. Greenstein, "The Impact of Personality on Politics: An Attempt to Clear Away Underbrush," this REVIEW, 61 (1967), 629–642. Personality is not defined by Greenstein because psychologists themselves have not come close to an agreed-upon meaning of the term and, more importantly, because he is really talking about "psychological factors" (p. 630) which is a very broad category indeed. This broad focus is forced upon him by the content of the literature being reviewed as much as by any personal preference.

[18] Robert E. Lane, *Political Life* (Glencoe: Free Press, 1959), p. 99.

the existing structure in the community."[19] The alienated, therefore, tend to express their resentment in negative votes on bond issues and the like. This could, of course, also explain the behavior of certain Republicans in the Wallace primary.

A more detailed look at these studies is in order because of the nature of the measures employed and the different arenas (foci) considered. In some cases combined (composite) measures of political distrust (cynicism) and powerlessness are used.[20] The focus of the measurement items in these cases is clearly the local government and the dependent variable is vote in a local referendum. Composites are useful devices for either obtaining a more homogenous and precise measurement of a variable or for obtaining a better device for predicting another variable.[21] However, forming them on an *ad hoc* basis (i.e., without either of the above purposes clearly in mind) may disguise the relationship of some of the components with outside variables or otherwise distort the results.[22]

My argument is not that it is illegitimate to base an operational definition of political alienation on an assertion that alienation "really" means, for example, a state in which an individual is powerless and distrusts those who govern, but that using an *ad hoc* composite to measure political alienation is self-defeating and leaves unanswered a series of nagging questions. Just how are the indicators related? Which part of the composite index is causing the relationships described or, better, how does each indicator relate to the various dependent variables? Perhaps they operate in the same way, maybe only one plays a significant role, or perhaps there are interesting interactions between the indicators which are significant theoretically and empirically.[23] The fundamental reason for defining alienation as a combination of distrust and powerlessness is the expectation that the joint occurrence of the two attitudes produces a unique behavioral tendency[24] and this should be established rather than, as in the case of these authors, assumed.

Other researchers in this area have used powerlessness measures alone in their work or have made some incomplete tests on the independent effects of powerlessness and distrust. Horton and Thompson, for example, emphasize that "referendums may serve as institutional outlets for protest . . . on the part of the powerless and ordinarily apathetic members of the community."[25] They operationally define political alienation as a feeling of powerlessness and an identification of an appropriate power center as a controlling agent.[26] They not only find the expected relationship between the composite index of powerlessness and negative voting, but they present some evidence that the feeling of powerlessness by itself has an independent effect on voting behavior in this instance.[27] They do have a direct measure of the distrust of local government, but it is unfortunately not discussed in terms of political behavior.

William Gamson's work on attitudes toward water fluoridation proposals is also germane here. He cites Coleman's suggestion that this issue gives citizens an opportunity to vent their

[19] Wayne E. Thompson and John E. Horton, "Political Alienation as a Force in Political Action," *Social Forces,* 38 (1960), p. 195.

[20] *Ibid.;* and Edward L. McDill and Jeanne Claire Ridley, "Status, Anomia, Political Alienation and Political Participation," *American Journal of Sociology,* 68 (1962), 205–214. No test of unidimensionality is reported.

[21] Edwin E. Ghiselli, *The Theory of Psychological Measurement* (New York: McGraw-Hill, 1964), p. 184. When the analyst wishes to make his measurement of a variable more homogenous and precise he chooses elements which have relatively high intercorrelations. One does not greatly increase the correlation between a composite variable and an outside variable if the components added to form the composite are highly correlated. However, when a composite is developed for purposes of prediction, the components will probably be correlated at a low level with each other and the addition of each element will greatly increase the level of correlation with the outside variable. The goal in this case, then, is a composite formed of relatively independent components each highly correlated with the outside variable we wish to predict.

[22] See Richard F. Curtis and Elton P. Jackson, "Multiple Indicators in Survey Research," *American Journal of Sociology,* 68 (1962), 195–204.

[23] This argument is made in detail in Aberbach, *op. cit.,* Ch. 2. I am currently preparing an article which reviews the literature on alienation in these terms.

[24] Clarence N. Stone makes a simular point in "Local Referendums: An Alternative to the Alienated-Voter Model," *Public Opinion Quarterly,* 29 (1962), 213–222.

[25] John E. Horton and Wayne E. Thompson, "Powerlessness and Political Negativism: A Study of Defeated Local Referendums," *American Journal of Sociology,* 67 (1962), p. 485.

[26] There is an implied, but not explicit, element of distrust in the second element of the definition.

[27] Horton and Thompson, *op. cit.,* p. 490, Table 1.

frustrations against local administrators who introduce the idea.[28] Gamson attempted to gauge ideological differences between respondents in his Cambridge, Massachusetts sample, but found that "overt differences in ideology concerning individual rights and government intervention have little to do with the average voter's position on fluoridation in this Cambridge precinct."[29] However, alienation measures or, better, some alienation measures were useful as predictors. Indicators of powerlessness and meaninglessness did successfully discriminate opponents of fluoridation from proponents,[30] but measures of distrust did not. Unfortunately, the measures of powerlessness were political and those of distrust were not, but we can see that distrust is not necessarily related to issue negativism.

Clearly, on these local issues, a perceived sense of powerlessness does lead to negative voting or to opposition to certain controversial programs. However, it is not certain just what effect distrust of local government exerts and how it interacts with feelings of powerlessness.

It is also still not clear that attitudes of alienation (of any form) play a role in voting in all types of referendums. One author has suggested that alienation is important in determining direction of vote only in referendums where "vague emotion-laden symbols are employed"[31] and that the alienated may turn out in large numbers in other types of referendums and vote like other citizens. There is, obviously, still a great need for research on the whole question of turnout, alienation and vote in local referendums and, for that matter, in local partisan elections.

If research and knowledge on the relationship between alienation and vote are somewhat confused and definitely incomplete on local elections, they are virtually non-existent on national elections. Templeton found alienation, in this case measured by Srole's Anomia scale,[32] unrelated to respondents' party identification or voting behavior in 1956 and 1960.[33] However, following Levin and Eden,[34] he finds some inferential support for the notion that the "quality" of the vote cast by an alienated voter may be different, i.e., that he tends to vote negatively and is most inspired when issues are perceived in terms of good and evil. Since, under most circumstances, "both major parties present themselves as 'responsible' national voices and both are in fact committed to the established political system . . . neither provides the citizen with the opportunity to validate his personal rejection of the political system."[35] As a result of this, says Templeton, the alienated citizen tends to withdraw from participation in the national political process. He manifests this through inconsistent voting (vacillating between the parties), and low political knowledge and interest,[36] although no evidence is presented of unusual nonparticipation in voting *per se*. According to this view, the whole process of national politics as usually practiced in this country serves to insulate us from a direct impact of the alienated feelings of part of the electorate.

Reported data on political efficacy are replete with findings of nonparticipation, low interest, etc., on the part of the inefficacious,[37] but there is no evidence that they are "protest voters" in national elections. Again, this may well be a function of the choices and symbols presented in these contests where party and group identifications provide such strong guides to behavior.

An analysis by Donald E. Stokes of the public's "basic evaluative orientations toward the national government"[38] (political trust)

[28] William Gamson, "The Fluoridation Dialogue," *Public Opinion Quarterly,* 35 (1961), p. 533; citing James Coleman, *Community Conflict* (Glencoe: Free Press, 1957).

[29] Gamson, *op. cit.,* p. 531.

[30] *Ibid.,* p. 535. As in the other studies, these relationships hold with social status controlled.

[31] Stone, *op. cit.,* p. 222. He includes school bonds in this category along with fluoridation and charter elections. More "concrete" issues which Stone hypothesizes draw large turnouts, but do not activate feelings of alienation, include such things as water supply, fire protection, etc. This is the familiar distinction between style and position issues.

[32] Fredric Templeton, "Alienation and Political Participation," *Public Opinion Quarterly,* 30 (1966), p. 252. In light of our discussion of alienation in terms of form and focus it should be noted that Templeton believes "it . . . unlikely that the present findings would be appreciably modified by using one of the other currently available measures of alienation."

[33] *Ibid.,* pp. 254–255.

[34] Murray B. Levin and Murray Eden, "Political Strategy for the Alienated Voter," *Public Opinion Quarterly,* 26 (1962), 47–63, citel in Templeton, *op. cit.,* p. 255.

[35] *Ibid.,* p. 256. Note that Templeton believed the 1964 election might constitute a "partial exception to this generalization."

[36] *Ibid.,* p. 256.

[37] See, for example, Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960), pp. 103–105.

[38] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment" in Harlan Cleveland and Harold D. Lasswell (eds.), *Ethics and Bigness: Scientific, Academic, Religious,*

based on the SRC 1958 election study failed to turn up any very exciting positive information. Political trust was virtually unrelated to direction of party identification (and vote), but associated with rejection of the party system as indicated by failure to identify oneself along the partisan spectrum.[39] There was a relationship with sense of political efficacy (which Stokes sees as lying behind the individual's attitude toward government) and a slight tendency for poorly educated respondents scoring low on political trust to participate less frequently than others. Stokes feels that those who are low in trust have no articulate commitment to a fundamental change in the political order or to extremist movements or leaders, but that their negative orientation "must . . . be seen as latent support that might be tapped by parties or factions or individual political leaders whose stance is hostile to the prevailing order."[40] The basic and unresolved issue is still when and if this latent support can be tapped in elections given the role of the institutions of our political system in dampening the behavioral effects of attitudes of alienation.

A graphic example of this can be seen in investigations using the murky variable, authoritarianism. These yield some evidence that this attitude, which contains a strong element of hostility, has a much weaker impact on behavior in general elections than its effect on candidate preferences might indicate. While Lane, using SRC data, found a small relationship between authoritarianism and both Republican party identification and vote for Eisenhower in 1952,[41] Milton's study of the presidential preferences of University of Tennessee students in 1952 demonstrated that neither Eisenhower nor Stevenson received strong support from authoritarians, although Eisenhower was clearly preferred to Stevenson. With a more varied slate of nominees presented to them, authoritarian students opted heavily for Taft and MacArthur, two candidates whose names were not on the official ballot.[42] Two basic points in the Milton study should be noted: 1) There was a strong correlation "between the authoritarianism attributed to aspirants for candidacy and the F-scale scores of students preferring each man"[43] in this preference test without partisan structure; and 2) The actual nominees were those least attractive to authoritarians, although one candidate (Eisenhower) was preferred.

Given the above, to provide some satisfactory test of the effects of attitudes of alienation on political behavior in national elections we need an appropriate set of stimuli. For science, the 1964 election was a blessing.

III. SETTING AND PROCEDURES

In 1964 the Republicans nominated a candidate widely labelled as an "extremist",[44] a man who offered people "a choice, not an echo," and whose covert appeal to racism (in addition to an overt appeal to the old American values) was at least given credence in the mass media if it was not based in his own intentions. This represented a major opportunity for investigating the relationship between attitudes of alienation and direction of voting behavior in a situation conducive to the joint variation of the two in a national election. The 1964 election study of the Survey Research Center at the University of Michigan provides a vehicle for such an investigation.

Fortunately, this study contained four separate measures of alienation as well as rich information on political attitudes and voting behavior. There were indicators of political trust (a slightly modified version of the Stokes scale) and interpersonal trust (trust in people),[45] po-

Political and Military (New York: Harper and Brothers, 1962), p. 64.

[39] *Ibid.*, p. 69. Stokes is referring to the small number of people who fall into the SRC a-political category and not to those who classify themselves as independents.

[40] *Ibid.*, p. 72.

[41] Robert E. Lane, "Political Personality and Electoral Choice," this REVIEW, 49 (1955). 173–190. See Table IV on p. 181 and the conclusion, p. 190.

[42] O. Milton, "Presidential Choice and Partisan Performance on a Scale of Authoritarianism," *The American Psychologist*, 7 (1952), 597–598. The reference is from Richard Christie, "Authoritarianism Re-examined," pp. 123–197 in Richard Christie and Marie Jahoda (eds.), *Studies in the Scope and Method of "The Authoritarian Personality"* (Glencoe: The Free Press, 1954).

[43] *Ibid.*, p. 146.

[44] This was a part of the rhetoric of the campaign in the primaries as well as the national election, but was also accepted by less directly partisan individuals. See, for example, Robert A. Dahl, "The American Oppositions," pp. 65–68 in Robert A. Dahl (ed.), *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), where he discusses the response of the "Alienated Right" (p. 66) to both Joe McCarthy and Barry Goldwater. Both, he says, wished to "shake off the shackles of two-party pragmatic, compromising politics and offer a radical and uncompromising alternative."

[45] Morris Rosenberg, "Misanthropy and Political Ideology," *American Sociological Review*, 21 (1956), 690–695, developed the basic scale.

TABLE 1: ALIENATION ITEMS

A. Interpersonal Trust (Trust in People)

1. Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?
2. Would you say that most of the time people try to be helpful or that they are mostly just looking out for themselves?
3. Do you think most people would try to take advantage of you if they got a chance or would they try to be fair?

B. Political Trust (Prefaced by the following statement: Now I'd like to talk about some of the different ideas people have about the government in Washington and see how you feel about them. These opinions don't refer to Democrats or Republicans in particular but just to the government in general. For example:)

1. Do you think that quite a few of the people running the government are a little crooked, not very many are, or do you think hardly any of them are crooked at all?
2. Do you think that people in the government waste a lot of money we pay in taxes, waste some of it, or don't waste very much of it?
3. How much of the time do you think you can trust the government in Washington to do what is right—just about always, most of the time, or only some of the time?
4. Do you feel that almost all of the people running the government are smart people who usually know what they are doing, or do you think that quite a few of them don't seem to know what they are doing?
5. Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?

C. Personal Efficacy (Personal Effectiveness)

1. Have you usually felt pretty sure your life would work out the way you want it to, or have there been times when you haven't been very sure about it?
2. Do you feel that you are the kind of person who gets his share of bad luck or do you feel that you have mostly good luck?
3. When you make plans ahead do you usually get to carry out things the way you expected, or do things usually come up to make you change your plans?

D. Political Efficacy (Agree—Disagree Statements)

1. Voting is the only way that people like me can have a say about how the government runs things.
2. People like me don't have any say about what the government does.
3. Sometimes politics and government seem so complicated that a person like me can't really understand what's going on.
4. I don't think public officials care much what people like me think.

TABLE 1 (*continued*)

litical efficacy and personal efficacy (personal effectiveness).[46] In other words, we have measures of two forms (subjects) of alienation and two foci (objects) for each form. As Table 1 indicates, the items in the trust-in-people and personal-efficacy measures refer to the self and the environment in a very general way; they ask about "most people," "luck," and "life" working out, etc. The political trust items, on the other hand, refer to the government in Washington very specifically and the political efficacy questions have a manifest political content.[47] A factor-analytic test shows that these items, as predicted, fall empirically on distinct psychological dimensions, although the factors are positively correlated.[48] Available evidence, therefore,

[46] Angus Campbell, *et al., op. cit.*, pp. 516–517, developed this measure as an "agree-disagree" scale. It has since been modified.

[47] It would be better to have political efficacy questions which focus on the federal government also. See Edgar Litt, "Political Cynicism and Political Futility," *The Journal of Politics*, 25 (1963), 312–323, for an example of the potential importance of this. I am currently conducting a study designed, in part, to check the relevance of focus in measuring efficacy.

[48] Principal component factors with eigen-values (amount of variance explained) greater than one were rotated using both orthogonal and oblique criteria. The four factors extracted explained about 65 percent of the variance of all variables. The clarity of structure was marked, with the items for each measure showing strong loadings on separate factors. An oblique factor analysis using the biquartimin solution recommended by Harmon (see Harry Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960), pp. 261–337) yielded the following correlations between the primary factors:

	Trust in People	Political Trust	Political Efficacy	Personal Efficacy
Trust in People	—	.16	.11	.13
Political Trust		—	.07	.12
Political Efficacy			—	.05
Personal Efficacy				—

See Aberbach, *op. cit.*, pp. 75–101 for a full dis-

TABLE 2. T_c CORRELATIONS BETWEEN ATTITUDES OF ALIENATION AND VOTE, 1964

Attitude	Interpersonal Trust	Political Trust	Personal Efficacy	Political Efficacy
Vote* (1 = Johnson) (0 = Goldwater)	−.01	.39	−.03	−.05

* A positive coefficient indicates that the higher the person's score on the index (high score equals positive attitude or allegiance), the more likely he was to vote for the Democrat.

points to the fact that thinking about alienation in terms of form and focus should be useful empirically as well as conceptually.

I have constructed separate standardized indices for each factor in the form of T-scores (i.e., with a mean of 50 and a standard deviation of 10) and, for purposes of this analysis, consolidated the scales so that each value represents a standard deviation unit. These scales are then related to vote in the 1964 election and, where results indicate the utility of further exploration, are correlated with additional variables under controlled conditions. Sometimes comparisons are made to relationships in other elections. The goal is always to illuminate, in as much depth as a secondary analysis permits, the conditions under which these attitudes of alienation are relevant to behavior.

The analysis is limited to white respondents because of one stark fact: there is simply no meaningful variation in the direction of Negroes' votes in 1964. This comes as no surprise in light of the issues involved, but illustrates the general principle that when a highly salient reference group's interests are threatened in a political contest, its members behave in a uniform manner if a clear behavioral option is open.[49]

cussion. These findings will be reported in detail in an article I am now writing. Similar findings are reported in Arthur G. Neal and Salomon Rettig, "On the Multidimensionality of Alienation," *American Sociological Review,* 32 (1967), 54–64.

IV. FINDINGS

A look at the relationship in Table 2 between the different attitudes of alienation measured here and the direction of vote in 1964 reveals rather clearly that *only* political trust was a viable zero-order predictor of vote in this election.[50] Table 3 illustrates the true strength of

[49] Negro bloc-voting is not uncommon, even in less dire situations. John C. Legget, for example, studied the 1958 gubernatorial election in Michigan using a sample of 375 Detroit blue-collar workers. He found that class consciousness and union militance had a strong influence on white workers' voting behavior (the more militant voted for the reform candidate), but that Negroes voted for the reform candidate regardless of militance or union membership. See John C. Legget, "Working Class Consciousness, Race and Political Choice," *American Journal of Sociology,* 69 (1963), 171–177.

[50] The ordinal correlation used here is Kendall's tau_c (T_c). It ranges between 0 and 1, but reaches unity when all cells are empty except those in the diagonal. Since this rarely happens, T_c seldom approaches unity and is a very conservative (although sensitive) measure of association. See Maurice G. Kendall, *Rank Correlation Methods* (London: C. Griffin, 1955), Chs. 1 and 3; and Hubert

TABLE 3. POLITICAL TRUST AND VOTE, 1964

Political Trust*		0	1	2	3	4	5
Vote	Goldwater	62.7%	52.3%	36.5%	29.9%	20.9%	13.5%
	Johnson	37.3%	47.7%	63.5%	70.1%	79.1%	86.5%
N =		185	149	104	197	215	156

* In T-score terms (i.e., a mean of 50 and standard deviation of 10):
0 = Below 40
1 = 40–45
2 = 46–50
3 = 51–55
4 = 56–60
5 = Above 60

this relationship. The others are substantively trivial. Even interpersonal trust, which is seen by many as the fundamental underpinning of political trust,[51] did not successfully predict the candidate chosen by the electorate on this occasion. Clearly, both the form and focus of alienation are important factors to consider in any study.

An immediate question arises as to whether political trust, as measured here, is really just a proximal partisan attitude of the type used so successfully by the authors of *The American Voter* in predicting vote.[52] This is not the case. Campbell *et al.* found party identification very strongly related to each of the partisan attitudes they described,[53] but in 1964 political trust was only mildly related to party identification ($T_c =$ .15, i.e., Republicans were less trusting than Democrats). The strength of the relationship between political trust and vote ($T_c = .39$) goes well beyond what one might expect from the measure of association given for party identification.

It is useful to compare the relationships between party identification, political trust, and vote in 1958 and 1964. It would probably be better to make a direct comparison of two Presidential elections,[54] but when I did this analysis these were the only election studies available where the relevant data were gathered, and there is reasonable evidence that the 1958 data are not atypical on the relationship of political trust to both party identification and vote. In 1958, political trust was negatively correlated with Democratic party identification ($T_c =$ $-.06$, i.e., the Democrats were slightly more distrustful) and Democratic vote for Congress ($T_c = -.08$). There is, then, some evidence that a mild partisan element is captured by the measure of political trust since the followers of the party *out* of power tend to be less trusting,[55] but the behavioral relevance of this attitude in 1964 went well beyond this. The special significance of the 1964 election in this regard is manifested in the correlation between political trust (measured, you will recall, in 1964) and respondent's memory of his 1960 vote. As one might expect, respondents disproportionately remember voting for Kennedy (61.1 percent), but the correlation between reported Kennedy vote and political trust is only .14 (compared to .39 with Johnson vote). These is *no* relationship between 1958 political trust scores and recalled vote for Eisenhower in 1956. In respect to the relationship of political trust to vote, then, 1958 appears to be a fairly typical year and 1964 a very unusual one.

If we examine the relationship between political trust and vote for the two years in more detail the situation becomes even clearer. Controlling for party identification virtually eliminates even the small relationship between distrust and Democratic vote for Congress in 1958, but the pattern in 1964 is striking. As Table 4 demonstrates, the most strongly identified partisans in

M. Blalock, *Social Statistics* (New York: McGraw Hill, 1960), pp. 321–324.

[51] See, for example, Morris Rosenberg, *op. cit.*, pp. 690–695; Robert E. Lane, *Political Life* (Glencoe: Free Press, 1959), p. 164; and Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), p. 285.

[52] Campbell, *et al., op. cit.*, Chs. 3, 14, 19.

[53] *Ibid.*, pp. 128–133.

[54] Presidential, as compared to Congressional, elections usually stimulate turnout. This was true in 1964 as compared to 1958, although it is not always the case. See Angus Campbell, "Surge and Decline: A Study of Electoral Change," *Public Opinion Quarterly*, 24 (1960), 397–418.

[55] The 1966 SRC election study, now available for analysis, had items comparable to those asked in 1958 and 1964 and results of a quick analysis confirm what is reported throughout the body of this article. On the particular point at issue here, Democrats (with their party in power) were slightly more trusting than Republicans ($T_c =$ .13) and the trusting voted for the Democratic candidates for Congress ($T_c = .16$). Again the other alienation measures were totally irrelevant in predicting the voters' choices.

TABLE 4. T_c CORRELATIONS BETWEEN POLITICAL TRUST AND DEMOCRATIC VOTE BY PARTY IDENTIFICATION AND YEAR

Party Identification:		Strong Democrat	Democrat	Independent Democrat	Independent	Independent Republican	Republican	Strong Republican
Year	1958	−.03	.00	−.08	.03	*.37	.04	−.02
	1964	.09	.25	.20	.49	.41	.34	.12

* This deviant coefficient is a function of the Democratic votes of 6 of the 9 Independent Republicans with the lowest scores on political trust. It is difficult to assess its substantive importance.

TABLE 5. POLITICAL TRUST BY VOTING BEHAVIOR FOR 1964 PRIMARY VOTERS BY PARTY PRIMARY

Political Trust		Voters in the 1964 Primaries: Republican Primary Low (0–1)	Republican Primary Medium (2–3)	Republican Primary High (4–5)	Democratic Primary Low (0–1)	Democratic Primary Medium (2–3)	Democratic Primary High (4–5)
General Election Voting	Goldwater	87%	74%	50%	40%	13%	7%
	Johnson	13	26	50	60	87	93
		100%	100%	100%	100%	100%	100%
		(N = 89)	(N = 51)	(N = 40)	(N = 88)	(N = 83)	(N = 127)

1964 were least affected in their voting behavior by the extent of their political distrust, while independents voted very heavily for Goldwater if they were distrustful. In this case, as with Negroes, a strong attachment to a reference group (here party) inhibited the effects of cynicism on the direction of the vote—only the least trusting Strong Democrats broke ranks to vote for Goldwater and only the most trusting Strong Republicans were able to support Johnson with their ballots.[56] It should also be noted that these relationships were stronger for the Republican identifiers than for the Democrats. This is probably because of the added pressures on these individuals to break party ranks in this election[57] as reflected here by the sensitivity of T_c coefficients to the slopes of relationships. The 1964 campaign, then, served to activate feelings of political hostility in a meaningful way for all but the most strongly identified partisans, and even these people were not totally immune.

[56] It is interesting to note that *Gamma* coefficients are less sensitive to this fact, but give a better indication of the fact that a relationship exists for every group of identifiers. These coefficients are as follows:

Strong Democrat	.49
Democrat	.48
Independent Democrat	.63
Independent	.75
Independent Republican	.66
Republican	.42
Strong Republican	.45

[57] See Philip E. Converse, Aage R. Clausen and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," this REVIEW, 59 (1965), 321–337; and Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," this REVIEW, 60 (1966), 19–29, especially pp. 21–23.

Even primary voters were strongly affected in their 1964 general election behavior by feelings of political trust or distrust. As one might suspect, primary voters are even more strongly identified with one of the two parties than other members of the electorate[58] and party identification provides them with an even stronger guide to electoral behavior than it does other voters. As Table 5 indicates, however, voters in the Republican primaries who were high in political trust split their votes between Goldwater and Johnson in the national election, while forty percent of the Democratic primary voters who were low in trust cast their ballots for Barry Goldwater. This table illustrates very plainly the fact that a larger proportion of the Republican than Democratic primary voters were politically distrustful and shows the influence of party on vote, but it also gives a clear indication of the importance of political trust in determining voter defections in this election,[59] even among the more active members of the electorate.

[58] It should be noted that very few members of the troublesome Wisconsin electorate are represented in the 1964 SRC sample since only Sheboygan was in the sampling frame. A study done in Wisconsin (Austin Ranney and Leon D. Epstein, "The Two Electorates: Voters and Non-Voters in a Wisconsin Primary," *Journal of Politics*, 28 (1966), 598–617), however, demonstrated that even there (p. 613) "primary voters, far more than party identifiers who did not vote in primaries, merited the description 'the party faithful'" since they (primary voters) did not defect as much from their party in general elections as non-primary voters and were more active politically.

[59] Unfortunately, the SRC data do not indicate who the individual voted for in the particular primaries.

TABLE 6a: POLITICAL POWERLESSNESS AND DISTRUST BY VOTING PREFERENCE AND BEHAVIOR, 1964

Alienation:*	Powerless and Distrustful %	Powerful and Distrustful %	Powerless and Trustful %	Powerful and Trustful %
Voted Democratic	45%	43%	60%	67%
Non-voter, Democratic Preference	20	10	26	16
Voted Republican	28	43	11	15
Non-voter, Republican Preference	7	4	3	2
	100% (N=296)	100% (N=341)	100% (N=300)	100% (N=600)
No answer, Refuse or Don't Know on Voting Preference or Behavior	(N=10)	(N=3)	(N=10)	(N=8)

Without carrying this much further, it should be noted that people who engaged in activities beyond voting (attending political meetings, working in a campaign, writing letters, giving money) were more likely than others to support Goldwater in 1964, and the relationship between political trust and vote direction grew progressively stronger for the more active groups. The Republicans who could not support Goldwater tended to withdraw from active participation in the campaign. There is no evidence, however, that Goldwater was able to mobilize substantial numbers of politically distrustful voters who were previously politically inactive.[60]

[60] In Stokes, *op. cit.*, there is very minor evidence of a relationship between past voting participation and political distrust, i.e., the greater the distrust, the less regular a person's voting turnout. There is no evidence of this in the Goldwater election. This not only negated a part of his electoral strategy, but goes against notions about the mobilization of the alienated non-voter in certain electoral situations.

Was there an interaction between political powerlessness and distrust in this election? Did individuals who saw themselves as politically powerless *and* distrustful of the national government flock to the Goldwater banner in unusual numbers? Clearly, as Table 6a shows, the powerless voted less often than the powerful in this election, regardless of their level of political trust. In addition, as both Tables 6a and 6b demonstrate, the distrustful were more likely to vote Republican, no matter how powerful or powerless they felt. In fact, individuals who felt powerless and distrustful were somewhat more likely to vote *against* Goldwater than persons who felt powerful as well as distrustful. This is the exact opposite of what those who stress an interaction between powerlessness and distrust would predict.

If a feeling of powerlessness does not act as a stimulus to voting for an extremist candidate among the distrustful, what does? While I cannot attempt to give a complete answer to this question here, the emphasis in the literature on negativism and protest among the alienated de-

TABLE 6b. POLITICAL POWERLESSNESS AND DISTRUST AMONG VOTERS ONLY

Alienation:*	Powerless and Distrustful %	Powerful and Distrustful %	Powerless and Trustful %	Powerful and Trustful %
Voted Democratic	61%	50%	85%	82%
Voted Republican	39	50	15	18
	100% (N=218)	100% (N=293)	100% (N=213)	100% (N=496)
Percentages are down				

* The powerful and trustful are defined as those above the sample mean (i.e., T>50).

TABLE 7: T_c CORRELATIONS BETWEEN POLITICAL TRUST AND DEMOCRATIC VOTE BY ISSUE ATTITUDE, AND YEAR

Issue	Issue Attitude (Direction)			
	Strongly Favorable		Strongly Opposed	
	1958	1964	1958	1964
1. Federal Aid to Education	.01	.21	−.17	.47
2. Government Enforced School Integration	.01	.27	−.16	.47
3. Government Enforced Equal Job Opportunity	.05	.25	−.25	.36
4. Foreign Aid	*	.30	*	.46
4. Trade with Communist Nations	*	.21	*	.43
4. Internationalism**	−.08	**	.00	**

* Question not asked in 1958.

** Questions not asked in 1964. This general item was the only close approximation: "This country would be better off if we just stayed home and did not concern ourselves with problems in other parts of the world."

serves some attention. The 1964 election heightened the tendency for people holding conservative attitudes to vote Republican,[61] but the more interesting phenomenon for our purposes is the interaction between attitudinal negativism, political distrust and protest voting which occurred in *both* 1958 and 1964 (the two years for which we have data).

Table 7 is a comparison of the T_c coefficients relating political trust and Democratic vote in 1958 and 1964 within opinion categories of representative issues. For purposes of presentation only the correlations for the strongly favorable and strongly opposed categories are given,[62] but the magnitudes of the coefficients are usually ordered across the opinion spectrum. What emerges is a very interesting pattern (with one exception) where those strongly opposing a liberal policy position tend to vote against the party in power (in this case defined as the party holding the Presidency) if they are distrustful politically. Conservatism (or at least negative attitudes) and political distrust interact to produce a behavioral negativism.

In 1958, politically distrustful people with conservative policy attitudes tended to vote Democratic and in 1964 similar individuals went heavily for the Republicans. The situation in 1964 was different, of course, in that the relationship between political distrust and vote held for those with positive (liberal) attitudes too, but the relationships here were much lower in magnitude than for the conservatives. These data suggest a modification of the hypothesis that there is a connection between alienation (at least in the form of political distrust) and protest voting. They suggest that attitudinal negativism on political issues is an important contextual factor which stimulates the relationship.[63] They show in addition, of course, that a candidacy like Goldwater's also makes political distrust behaviorally relevant for individuals with positive policy attitudes.

Certain important questions remain. First, are these issue attitudes important contextual factors in defining the relationship between other forms of alienation (or trust with a different focus) and behavior? The general answer, based on the measures available to me, is no. The only interesting differences are found in the relationship between political efficacy and vote, and here those with negative issue attitudes tended to vote disproportionately for Goldwater if they felt highly efficacious.[64] This is not only the exact opposite of the situation with political trust (since alienation is related to a Johnson vote in the efficacy case), but the magnitudes of the coefficients are exceedingly small (below .10) and it is hard to attach much substantive significance to them.

[61] The correlations between Republican identification and issue attitudes also increased.

[62] SRC asked respondents to rate their agreement with an attitude statement on a five point Likert scale ranging from "Agree Strongly" to "Disagree Strongly" in 1958, and in 1964 asked them their opinions on an issue first and then assessed opinion intensity.

[63] The 1966 election data reveal a similar pattern.

[64] In contrast, the highly efficacious voters with positive issue attitudes tended to vote for Johnson.

A second question concerns the meaning of issue conservatism here. Are the relationships found a reflection of an interaction between deeply held attitudes or are we merely dealing with a cumulation of negative feelings, some of which are shallow in content? The fact that the very vague item on internationalism asked in 1958 was the only one which did not stimulate a relationship between political distrust and negative voting leads one to suspect that there may be something to an interpretation which puts some emphasis on the substance of the particular issues involved, but this is certainly an open question at this point.[65] What is required now is research on the structure and stability of issue attitudes for the trustful and distrustful and an investigation of the cumulative effects of negative issue attitudes as they interact with political distrust in influencing the vote.

V. POLITICAL ALIENATION AND THE POLITICAL SYSTEM: THE MODERATING ROLE OF INSTITUTIONS

In the American political system, basic issues are usually muted in elections and debate. Candidates for office seek the middle ground to gain marginal support, legislative personnel do the same to build winning coalitions, parties are composed of "wings" representing divergent points of view, etc. When this delicate system is upset, the patterns of people's responses to the stimuli presented also change. Motivations which are usually of limited importance in determining the direction of voting behavior can come strongly to the fore. This is especially true for individuals who are not very strongly committed to either of the established parties currently operating.

We have seen in the preceding pages a vivid example of the role of political institutions in moderating the effects of political hostility, and what can happen when this system does not function effectively. Goldwater's appeal was not unrecognized and did not go unheeded by large groups of people who were cynical about the personnel and operations of government. The basic issues and appeals in this election were, in one sense at least, *less* ambiguous than usual—the usual smoke screen laid down by fundamental agreement between traditional party candidates was missing—and voters responded accordingly. Party loyalties were partly set aside by basic orientations to the political system. The fact that Goldwater was so unsuccessful does not eliminate this fact, nor the possibility that a more able candidate could make better capital out of the potential manifested in this situation.[66]

It seems that the importance of a variable like political trust in governing the direction of vote is not determined solely by whether elections are structured contests between standard parties or non-structured (primaries, referendums, etc.), but by what appeals are made and how clearly they are perceived. In some cases party or group is a more relevant variable for organizing whatever perceptions exist and for making choices as to the direction of behavior than in others. The political system of the United States, as it traditionally operates, serves to dampen the relevance of attitudes like political trust, but this is not an inevitable by-product of partisan elections, even in a two-party system.

It is also aparent that attitudes of alienation are not equally relevant in each situation. Political trust was a powerful determinant of vote in 1964, but interpersonal trust, personal efficacy, and political efficacy were virtually irrelevant. The utility of speaking of alienation in terms of focus as well as form is amply demonstrated by the fact that the correlation between vote and scores on trust in people in 1964 was nonexistent ($T_c = -.01$) in spite of the extreme relevance of political trust. Goldwater's partisans, no matter what their party affiliation, were quite selective in the object of their distrust.

Evidence in the literature clearly suggests that politically inefficacious people vote negatively in referendums and there is inferential material to support the notion that the powerless are hostile to local authorities and express their distrust in this way. In 1964 there is absolutely no support for a model which holds that perceived powerlessness leads to political dis-

[65] See Campbell, *et. al., op. cit.*, Chs. 8 and 9, and Philip E. Converse, "The Nature of Belief Systems in Mass Publics," pp. 206–262 in David E. Apter (ed.), *Ideology and Discontent* (New York: Free Press of Glencoe, 1964). Given the research reported by these scholars, an ideological interpretation must be approached with great caution.

[66] See Philip E. Converse *et al., op. cit.*, pp. 321–336. They point out how poor Goldwater's appeal was on most policy issues and how fear of his position on non-racial issues often muted the so-called "white-backlash" which did exist as a powerful potential. A more effective candidate with his views who could avoid labels like "trigger-happy" might have done considerably better. Certainly, these data indicate that an appreciable increase in the number of people holding strongly negative issue opinions would increase the vote totals of a Goldwater-type candidate.

trust which, in turn, leads to voting against the prevailing order (negative voting) or even for the simpler idea that attitudes of powerlessness and distrust interact in affecting behavior in national elections. The evidence is that political distrust alone (or, better, distrust interacting with negative policy attitudes) spurs negative voting in national elections, especially under certain favorable conditions which heighten the relevance of distrust to the voting choice. Feelings of powerlessness influence turnout, but *not* the voters' choices.

Why wasn't perceived political powerlessness a stimulus to negative voting in the 1964 election? One possibility, I think, is the nature of the Goldwater appeal. It was against a major governmental role in the society—opposed to a backdrop of social and economic security provided or supported by government. This kind of appeal might ring true to those who distrust government, but the politically powerless would, if anything, tend to feel that they need this protection. Therefore, the finding that those who felt politically powerless and distrustful were more likely to vote for Johnson than people who felt powerful and distrustful. If a populist-type candidate were to present himself to the electorate this might be reversed. In such a setting, powerlessness and distrust could conceivably interact significantly. Survey data on Wallace voters in the 1968 election will soon be available for testing this hypothesis.

PARTY CHOICE AND SUPPORT FOR DEMAGOGUES: AN EXPERIMENTAL EXAMINATION*

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The relationship of parties to democracy and to political stability has concerned scholars for years.[1] Extant party systems have almost continually been under attack by reformers bent on achieving various objectives, among them greater party responsibility, greater honesty, and greater efficiency of governmental operation.[2] But party systems have not suffered from a lack of defenders: reformers have often been engaged in vigorous debate. Such controversy, apart from the merits of the arguments on either side, focuses attention on a number of interesting empirical propositions about parties in democratic systems. These propositions, unfortunately, have not often been put to rigorous systematic test.

For example, defenders of the American party system in particular point out that an important function of parties is the guiding of social conflict into moderate, non-revolutionary modes of expression.[3] Attempts at reform, goes the argument, should be considered as to whether they might interfere with this beneficial function. Citizens are viewed as having inherent tendencies to perform destabilizing political acts or to vote for extremist political figures. Somehow, we are told, these citizens are restrained from indulging these immoderate predispositions by the party system.[4] In a sense, the argument continues, parties save the great unwashed from themselves; all democrats should be thankful that they do and appreciate them for this service.

What empirical ground is there for believing that parties contribute to behavior patterns productive of stability and moderation in democracies? Though not abundant, there is some evidence that where parties are not highly salient for citizens, voting behavior destabilizing to the political system may take place. Where there is no voter commitment to party *qua* party, an immoderate politics seems more likely. Party may be replaced by or act as a vehicle for 1) personal candidate characteristics or 2) ideological principle. Either contingency may enhance the prospects for electoral success of high-temperature demagogues. An often-cited example of the first is to be found in the American South. In this one-party region, voters frequently are denied the opportunity to cognize meaningful electoral alternatives in terms of party. Victory in the Democratic primary has been tantamount to election. Apparently, at least in some areas, loyalty to faction has not been the functional

* The order of the authors' names should not be interpreted to imply seniority of authorship. This is a joint enterprise. The authors wish to thank the University of Kentucky Research Foundation for financial support of the research upon which this article is based.

[1] Sigmund Neumann, "Toward a Comparative Study of Political Parties," in Sigmund Neumann (ed.), *Modern Political Parties* (Chicago: University of Chicago Press, 1956), pp. 395-421; Maurice Duverger, *Political Parties* (New York: John Wiley Science Editions, 1963), pp. 116–132, 275–280; M. Ostrogorski, *Democracy and the Organization of Political Parties* (New York: Macmillan Co., 1922), Vol. II, pp. 651–662; Frank J. Sorauf, *Political Parties in the American System* (Boston: Little, Brown Co., 1966), pp. 8–12.

[2] Report of the Committee on Political Parties of the American Political Science Association, *Toward a More Responsible Two-Party System* (New York: Rinehart, 1950); E. E. Schattschneider, *Party Government* (New York: Rinehart, 1942), pp. 206–210; James MacGregor Burns, *The Deadlock of Democracy* (Englewood Cliffs, N.J.: Prentice Hall, 1963), pp. 325–332; However also see Austin Ranney, *The Doctrine of Responsible Party Government* (Urbana: University of Illinois Press, 1954).

[3] Clinton Rossiter, *Parties and Politics in America* (Ithaca, N.Y.: Cornell University Press, 1960), p. 39; V. O. Key, *Politics, Parties and Pressure Groups*, 5th. ed. (New York: Crowell, 1964), pp. 200–205; Alan Nevins, "The Strength of Our Political System," *New York Times Magazine* (July 18, 1948), pp. 5ff.

[4] Edward C. Banfield, "In Defense of the American Party System," in Robert A. Goldwin (ed.), *Political Parties, U.S.A.* (Chicago: Rand-McNally, 1964), pp. 21–39; Aaron B. Wildavsky, "On the Superiority of National Conventions," *Review of Politics*, 24 (July, 1962), 307–319.

equivalent of party loyalty. Factions engender only transient allegiances resulting in a very volatile electoral process.[5] There is a great deal of unattached political affect which can be capitalized upon by irresponsible demagogues of the moment.[6] "Perhaps a clue to he picturesque quality of southern political leaders lies in the fact that attention-getting antics function as a substitute for party . . . in the organization of support."[7]

The contention that voter commitment to party mutes ideological fervor is often found in comparisons of the American two-party system and European multi-party systems. In France, we are told, partisan conflict reflects controversy over basic questions of political community and regime.[8] Everything is a matter of principle. Voters do not develop enduring loyalties to party, but rather to positions or "*tendances*." Accordingly, parties themselves are secondary, even transient. "Flash parties," probably committed to some extreme position, threaten to disrupt the polity only to lose all electoral support and recede a short time later.[9]

Evidence is by no means firm, but there is some suggestion that non-partisan elections in the United States provide arenas for successful presentation of strident appeals. Though partisan voting loyalties often continue to intrude upon non-partisan contests,[10] such elections sometimes do provide opportunities for voters to indulge sentiments about groups for which they have negative valences without repudiating their party.[11] Structuring of the election by party is apparently replaced not by issue content, but more likely by "religious, ethnic, or other extraneous associations,"[12] perhaps with overtones of viciousness.

Finally, referenda require voters to structure balloting situations without benefit of party. Once more there is slight evidence that this kind of situation allows demagogic approaches to find support. Under some conditions, referenda produce rather high rates of participation among normally electorally inert segments of the population. Moreover, such persons demonstrate a great propensity to reject referendum proposals.[13] These phenomena are especially likely to occur in response to demagogic appeals. "When distrust of . . . the powerful generally, allegations of conspiracy or of behind the scenes maneuvering for personal gain, unrestrained personal attacks . . . characterize the opposition campaign,"[14] referenda are likely to be defeated.

Thus, though the evidence here presented is fragmentary and varied, it is reasonable to expect that where electoral choices are likely to be perceived in terms of party alternatives, the probability of successful appeals by extreme candidates will be quite low. This literature performs a very important service in bringing evidence to bear on the substantive proposition that viable parties are associated with moderate politics. However, it leaves the really interesting questions largely unanswered in that few convincing, general *explanations* of the phenomenon are offered. To be sure, the task of explanation is a formidable one. It is the modest goal of this paper to sketch out two general interpretations and offer some evidence which bears on their relative merits.

I. THE POLITICAL EXPLANATION

In this approach, parties *per se* act as causal agents in electoral choice, for they function as political educators. This explanation is manifested most clearly in the literature on party identification. The "psychological attachment" to party connoted by that term can be extremely useful for an electorate. Party becomes a beacon in a confusing political world of which each voter cannot have complete knowledge.[15]

[5] V. O. Key, *Southern Politics* (New York: Alfred A. Knopf, 1949), pp. 46–52.

[6] In the sense that there are no relatively stable blocs of voters to whom they must defer for support.

[7] Key, *Southern Politics*, p. 46.

[8] Alfred Grosser, "France: Nothing but Opposition," in Robert A. Dahl (ed.), *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966), pp. 289–292.

[9] Philip E. Converse and Georges Dupeux, "Politicization of the Electorate in France and the United States," *Public Opinion Quarterly*, 26 (Spring, 1962), 1–24.

[10] Robert H. Salisbury and Gordon Black, "Class and Party in Partisan and Non-Partisan Elections: The Case of Des Moines," this REVIEW, 57 (September, 1963), 589.

[11] J. David Greenstone, *A Report on the Politics of Detroit* (Cambridge, Mass.: Joint Center for Urban Studies, 1961), pp. II-7, II-8.

[12] Charles R. Adrian, "Some General Characteristics of Nonpartisan Elections," this REVIEW, 46 (September, 1952), 773.

[13] John E. Horton and Wayne E. Thompson, "Powerlessness and Political Negativism: A Study of Defeated Local Referendums," *American Journal of Sociology*, 67 (March, 1962), 485–493.

[14] Clarence N. Stone, "Local Referendums: An Alternative to the Alienated Voter Model," *Public Opinion Quarterly*, 29 (Summer, 1965), 216.

[15] Angus Campbell, *et. al.*, *The American Voter* (New York: John Wiley, 1960), pp. 120–167.

"... [T]he parties themselves serve as the significant source of political direction for the electorate. . . . As the . . . major source of political education, they create a public image of politics as a competition between parties *per se*. . . . In such a situation, politics tends to lose its ideological character; the public is not stimulated to inform itself regarding specific policies; its political role becomes one of deciding who shall manage the government, not what shall the government do. For the bulk of the electorate, this decision is determined by long-established party loyalties. . . ."[16] The prominence of this cognitive device in electoral decisions among American voters is well established. Moreover, its role in maintaining the non-ideological, stable character of American politics is suggested by cross-national research which demonstrates the absence of widespread party identification in more turbulent political systems.[17]

Using different kinds of data and insight, other writers describe a highly positive and specific role for parties in the determination of voting behavior. Parties, once they are established, allegedly encourage, supervise, persuade, cajole, educate, electioneer, and organize electorates so that they respond to party symbols.[18] This response, we are told, entails a large number of "desirable" consequences, not the least of which is the moderation of political conflict. The rallying of popular support by parties is seen as "the functional equivalent of revolution."[19] This fortunate "sublimation" of the electorates' dangerous impulses of course means less extreme practice, less violence, and fewer demagogues.

To some extent, the political approach begs the question of why in some democracies, extant parties fail in their educative task. The French voter does not perceive his voting alternatives in terms of party to the extent that the American does. And the French are allegedly more prone to favor demagogues. But parties in France, like those in the U.S., have enjoyed a long existence. What makes for party viability, for successful education, for ability to present meaningful symbols to the electorate? Additional explanation is necessary. However, the approach does focus our attention on the possibility that parties can have an independent impact once the conditions of their viability are met. This caveat leads to consideration of the second major interpretation.

II. THE CULTURAL EXPLANATION

In this approach to the disassociation of party-defined electoral choice and success of demogogues, parties are not causal agents to all. Indeed, they are epiphenomenal. If they are associated with moderate political behavior, it is not because they condition the behavior, but because their existence and moderate behavior are both dependent upon the same cultural conditions. Once cultural orientations are such as to produce moderation, appropriate party institutions will develop to act as vehicles for political expression.

There are several scholars who stress the cultural antecedents of moderate, stable democracy, and most of them attribute to party an expressive or attendant (rather than causal) role. In an extensive five-nation study is described the "civic culture," certainly the very antithesis of a condition which would lead to the making of electoral choices on the basis of attraction to demagogues. This culture is defined by a configuration of personal feelings and attitudes. Where many people feel an obligation to participate in the electoral process, where there is a good deal of involvement in the (moderate) activities that customarily surround the selection of leaders, where the population is generally not alientated (feels efficacious), there is likely to exist little prospect for the success of extremists or "messianic movements that lead to democratic instability."[20]

But these cultural conditions also entail "patterns of partisanship." There must be some channel through which these personal feelings can "flow" to have an impact on political affairs. Partisanship provides these. The civic culture is most likely to produce "open" partisans, very moderate voters to whom the notion of party seems very central. Such citizens contrast sharply to partisans bred in non-civic cultures —on the one hand, apathetics who are withdrawn, and on the other hand, intense types or parochials for whom "partisanship is not a political phenomenon, but a cultural-religious one."[21]

[16] Angus Campbell and Henry Valen, "Party Identification in Norway and the United States," in Angus Campbell, *et. al.*, (eds.), *Elections and the Political Order* (New York: John Wiley, 1966), p. 267.

[17] Converse and Dupeux, *loc cit.*

[18] Hugh Clokie, "The Modern Party State," *Canadian Journal of Economics and Political Science*, 15 (May, 1949), 142–143; Robert MacIver, *The Web of Government* (New York: Macmillan, 1947), pp. 211–214; E. E. Schattschneider, *The Semi-Sovereign People* (New York: Holt, Rinehart, and Winston, 1960), p 59.

[19] Key, *Parties, Politics and Pressure Groups*, p. 203.

[20] Gabriel Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown, 1965), p. 355.

[21] *Ibid.*, p. 115.

Thus it appears that the civic culture not only inhibits the success of demagogues, but creates the underpinnings for competition between viable parties, while other cultures conduce to "messianic" leaders and also lead to situations where party is either unsupported or relegated to vehicle status for other passionately felt concerns.

Lipset's famous study suggests that various "extremist" political movements are engendered by social factors such as isolation, lack of wealth, and illiteracy. To be sure, parties are not ignored. Indeed, where "parties of representation" secure most of the vote, there is likely to exist a stable democratic order. But this is primarily because both the parties and the stability are dependent upon a series of cross-cutting, non-reinforcing social cleavages (which are most likely to occur in integrated, wealthy, literate societies).[22] These "cross-pressures" produce such effects by generating a culture of "cosmopolitanism," one characterized by "tolerance," and by "reduced emotion and aggressiveness." Electoral support for party is merely the reflection of such cultural conditions.

The list of advocates of the cultural explanation could be lengthened considerably. A recent "theoretical" explication suggests that vulnerability of democratic government, which may involve citizen susceptibility to the appeals of demagogues, is due to anomie, which is in turn a consequence of "incongruity" between various authority structures in society. Thus, heavily Catholic countries are less likely to remain democracies because the authoritarian structure of religious life is incongruent with democratic political rule.[23] The "strain" thus created renders the masses more susceptible to extreme programs. Parties are not a cause of democracy, for both political parties and democracy have common antecedents. In fact, meaningful partisan elections are "practically synonomous" with democracy.[24]

III. SOME EMPIRICAL TESTS

Hypotheses. Available evidence dramatically favors neither the political nor the cultural explanation of these phenomena. If party symbols to which the electorate has been educated to respond are not present, voting behavior indeed seems to have more demagogic support potential than if such symbols are available as choice criteria. On the other hand, the amount of support for the proposition that both demagogic support potential and the presence of effective, symbol-providing parties depend upon cultural conditions is also impressive. Thus, the contrast between these two explanations may appear overdrawn. Probably not even the most fervent apologist for parties would deny that certain social conditions are conducive to stable, non-demagogue-troubled political systems, while others are inimical. Similarly, even extreme social determinists would admit that parties can become established and that measurable effects can flow independently from them.

However, this should not obscure the necessity of additional research on the role of party in demagogic choice by voters. Though extant evidence suggests some role, we certainly do not know *how great* that role is. Indeed, due to the fact that uncontrolled variables often change when party choice criteria change—for example, comparisons between partisan and non-partisan voting entails comparisons of voting for different offices or in different locations—we cannot be absolutely sure that there is a role at all.

Our assessment of the available literature leads us to posit that cultural variables do indeed have a pronounced effect on attractiveness of demagogues to voters, but that over and above this, party choice criteria have a measurable impact—an impact which declines, however, as the values of the cultural variables approach the extremes. That is, some voters may be in conditions which conduce so strongly to support for demagogues that the educative outputs of parties and their provision of symbols in elections have no effect whatever. Demagogues may be chosen whether there are party alternatives or not. By contrast, others may be so rich in "civic culture" that demagogic candidates could never be tolerated and the educative effects of party may then be redundant. Though parties might be present in the electoral situation, their independent effect on demagogic support potential would be small indeed. In intermediate cultural conditions, however, demagogues may not be unthinkable, but parties, should they be present, may provide meaningful symbols to which electorates respond in preference to demagogic appeals.

These propositions are clearly of great importance, for they deal with the conditions under which party can be an effective instrument in the determination of the kind of rule which electorates will choose, i.e., in the determination of the stability of democracy. It is to a test of these propositions that we now turn.

Method. Systematic research of the above no-

[22] Seymour Martin Lipset, *Political Man* (Garden City, N.Y.: Doubleday, 1960), pp. 74–77.

[23] Harry Eckstein, *A Theory of Stable Democracy* (Princeton: Center of International Studies, 1961), pp. 21–28, 35–38.

[24] *Ibid.*, p. 3.

tions requires a design in which voter choice of demagogues is always possible, in which there is variation in the presence and absence of meaningful party symbols, and in which there is cultural variation. The extreme difficulty of realizing these desiderata in a real world situation, not to mention the problems of control of variables, necessitated a quasi-experimental approach. Data were gathered from 847 residents of the Lexington, Kentucky metropolitan area. A randomized cluster sample of all housing units in the area was drawn, and this served as a basis for the selection of respondents. Several standard instruments and modes of soliciting data were employed to provide information on social, personal, and political variables.

Clearly, there is no cultural variation among the subjects in that they are not drawn from various distinct cultural units; they are indeed all from the same local community. However, this problem is obviated if we consider as the definition of culture, "that of psychological orientation toward social objects."[25] We need not assume cultural homogeneity within a given area; in fact, it may be dangerous to do so. The culture of an area is the particular distribution of patterns of these orientations. Within our sample, there is considerable variation in relevant orientations and we can therefore deal quite nicely with culture. The three psycho-cultural variables of concern to us, allegedly of great importance for the character of political support, are alienation, anomie, and sense of citizen duty.[26]

The other relevant variables, electoral selection of demagogic candidates, and opportunity to structure voting situations in terms of partisan alternatives, are somewhat more difficult to approach. The lack of general agreement as to what constitutes a demagogue makes operationalization of the concept somewhat arbitrary. Our assessment of the literature plus the necessity of experimental manipulation led us to approach the task in the following way. A pool of "extreme" programmatic statements, such as might be uttered in a campaign, was assembled by the authors. The items referred to four issue areas: the Viet Nam war, internal communist subversion, federal centralization of government prerogatives, and civil rights or racial conflict. Most of the statements were adaptions of remarks made by figures who have at one time or another, justly or unjustly, been called demagogues. The statements were then submitted to the members of the political science department of the University of Kentucky, who were asked in light of *A Dictionary of the Social Sciences*' definition, to rank the statements on a five-point scale for their degree of demagogic content. Those statements in each of the four issue areas which received the highest cumulative scores were accepted for use in the instrument. Respondents were subsequently presented with these statements which were attributed to a candidate for Congress. Their choice of this candidate over others is regarded as evidence of attraction to demagoguery. Admittedly, this scheme does not tap all of what seems to be involved in the notion of demagoguery. Here we have only the content of an appeal. Certainly demagoguery does not flourish only because of the words used. The way they are said, the intensity and frequency of the presentation, the appearance of the presenter, and possibly the nature of available sanctions, may all play a role in demagoguery. However, few observers of the phenomenon exclude content,[27] and thus it would be a denial of considerable consensus to do so. We are measuring at least one aspect of the phenomenon, and perhaps the one most amenable to systematic observation. Thus equipped with hypothetical candidates who make demagogic utterances, we established a basic experimental design as follows: (1) All respondents were forced to choose one of the four issue areas as most salient; (2) The direction of each respondent's policy preference within the selected issue area was ascertained; (3) With the appropriate policy preference reflected in the content of some of the alternatives offered, each respondent was asked in which of four hypothetical elections he would most likely participate; and (4) He was asked how he would vote in that election. Hence, each respondent was given printed information about eight candidates—opposed pairs in four hypothetical elec-

[25] Almond and Verba, *op. cit.*, p. 13.

[26] Alienation is measured by the Survey Research Center's Political Efficacy Scale, Angus Campbell, *et al., The Voter Decides* (Evanston, Ill.: Row, Peterson, 1954). pp. 187–194; anomie is measured by a version of Srole's instrument, see Edward L. McDill and Jean Clare Ridley, "Status, Anomia, Political Alienation and Political Participation," *American Journal of Sociology*, 68 (September, 1962), 205–213; The measure of sense of citizen duty is also taken from the Survey Research Center's arsenal, Campbell, *et. al., The Voter Decides*, pp. 194–199.

[27] Allen Bullock, *Hitler: A Study in Tyranny* (New York: Bantam Books, 1961), especially pp. 43–46; Ernst Kris and Nathan Leites, "Democratic and Totalitarian Propaganda," in Leonard Fein (ed.), *American Democracy: Essays on Image and Realities* (New York: Holt, Rinehart and Winston, 1964), pp. 55–60; and Lipset, *op. cit.*, Chapter 5.

tions. The elections were characterized in the following way: *Election I,* candidates of differing personal characteristics (one "attractive" and the other "unattractive"); *Electron II,* candidates opposed in that one was "demagogic" in what he said while his opponent's only known characteristic was that he disagreed with the demagogue; *Election III,* candidates who took opposed but moderate stances on issues; and *Election IV,* candidates of different parties. Information about these candidates was rigidly controlled. Respondents knew nothing about the candidates in Election I except their personal characteristics; nothing about the candidates in Election III except their issue stances, and so on. There was no information about the candidates to which our subjects could respond except the designated features. Names were not even attached for fear of activating ethnic loyalties. In Elections II and III candidates addressed themselves to a policy area (one of four) previously ascertained to be of most concern to the respondent. Moreover, in Election II the demagogic candidate's remarks were always in the *direction* that the respondent's preferences had been determined to lie.[28] To reduce the possibility that the instrument itself would affect the choice, the information about each of the eight candidates occupied about the same number of printed lines. Interviewers read the information

[28] For example, a respondent who indicated that racial conflict was the most salient of the four issues, and who further indicated that his personal position was pro-Negro, was presented with a hypothetical demagogic candidate in election II to whom the following statement was attributed: "Every white man in the south is an uncivilized dog. His record of lynchings proves it. There is no white man's justice; justice comes only when the black man takes it by force." A person choosing the same issue but indicating an anti-Negro stand was presented with a hypothetical demagogue who said, "We have a right to keep niggers from moving in next door to us and we have a right to use force to do it. We must preserve western Christian civilization. Would you want your daughter to marry a nigger?" The statement attributed to a demagogue was not always the direct counterpart of that attributed to the demagogue on the opposite side of the same issue, nor was it necessarily directly parallel to the "less extreme" statement attributed to an issue-oriented candidate in election III. This was due to the fact that our operation for generating demagogic statements sometimes produced items with less specificity of reference than those used for issue-oriented candidates. On the ground that our experts seemed to believe it was the nature of demagoguery to avoid issue-specificity, we did not interpose our own judgement. As an example of the format of the electoral choice instrument, we present the entire configuration of options offered to respondents who choose Vietnam as the most salient issue, and who indicated a pro-involvement posture. The complete instrument can be obtained from the authors.

ELECTION #1

Candidate "A" is in his early thirties and has a reputation for energy and drive. He is married, has three children, and is active in his church. He is a lawyer. He recently received an award for distinguished service to his community.

Candidate "B" is a middle aged bachelor who is employed with a business firm which recently moved to Lexington from Pennsylvania. It is rumored that he is secretly engaged to a woman twenty years younger than himself.

ELECTION #2

Candidate "C" says, "The only language the communists understand is war. Your children will be safe only when our armies have killed every last communist. The Lord is on our side in Viet Nam. To fight on to total victory is the wish of the blessed Saviour."

Candidate "D" says, "One thing about this campaign is clear. My opponent and I are in very great disagreement on almost every point. The voters must choose between two programs. Mine is the most beneficial for all citizens."

ELECTION #3

Candidate "E" says, "Because of the necessity of containing communism, we must remain in Viet Nam. It is simply smart world politics to stop the efforts of the expansionist powers. If they succeed, they will go after more."

Candidate "F" says, "We must weigh the cost of staying in Viet Nam against what we gain. I believe that the costs are too high and the gain too small. I propose that we cut our costs by withdrawing from this conflict."

ELECTION #4

Candidate "G" is a *Democrat.* He has stated that he respects the principles of his party and believes that Democrats can be important influences in achieving excellent government for all citizens.

Candidate "H" is a *Republican.* He has stated that he is impressed with the prospects for his party in the years ahead, and has pledged to make a good image for Republicans in all that he does.

about each candidate aloud with the respondent to insure that all alternative choices had been cognized.

Manipulation of the opportunity to structure electoral alternatives in terms of party was accomplished by denying a random half of the sample access to the hypothetical partisan election. This half, hereafter called the experimental group was required to choose among three elections and six candidates, selecting on the bases of candidate personal characteristics, issue position, or demogogic features. The remaining half, the control group, of course received partisan stimuli, which, given its probable life-long exposure to the "educative" efforts of Democrats and Republicans, should have been meaningful.[29]

Although this design controls many potentially contaminating variables and allows considerable manipulation, it has certain liabilities. Obviously, the respondents are not actually politically supporting demagogus by their hypothetical voting choices nor are they actually operating in party or non-party contexts. They are participating in an experimental situation. This, however, is not an overpowering defect. A great deal has been leaned about human behavior through psychological experimentation for example. The truly remarkable thing is how little experimentation is used in political science.[30] The advantage of being able to abstract concepts for research—especially such basic ones as are discussed here—outweigh the consideration that we are not directly observing overt participation in the political process.[31]

The viability of our procedures has been established elsewhere.[32] Suffice it to say here that response patterns carry no hints that subjects treated their hypothetical choices capriciously. Many of the emergent data are thoroughly consistent with what has been found in studies of actual electoral behavior.

[29] This technique is similar to the "split ballot" discussed in Hadley Cantrill, *Gauging Public Opinion* (Princeton: Princeton University Press, 1944), pp. 23ff. Split ballots, by presenting different wordings or alternate forms of items to separate but matched subsamples, reveal response differences which are artifacts of instrumentation. In this study, such response differences, due to alternate instrumentation, are used as a measure of the dependent variable. Sound experimental research, of course, requires that there be no relevant contaminating differences between experimental and control groups. We sought to achieve this by random assignment of respondents. That we were successful is demonstrated in the table below. Tests for differences were performed on several variables. In no case was there anything approaching a significant difference.

COMPARISON OF EXPERIMENTAL AND CONTROL GROUPS

Variable	Test
alienation	$D = .058$
anomie	$D = .035$
sense of citizen duty	$D = .080$
party identification	$D = .084$
education	$D = .074$
income	$D = .054$
sex	$Z = .035$
race	$Z = .008$

Where data are in ordinally arranged categories, the Smirnov two-sample test is employed. Dichotomous data require the difference of proportions test. In all cases, $p > .50$. Obviously, these variables could produce no observed differences in behavior.

[30] Many scholars including philosophers of science urge experimentation on the study of politics despite the obvious difficulties of implementation in field situations. The advantages of manipulation and control of variables and of relatively easy replication outweigh these considerations. H. J. Eysenck, *The Psychology of Politics* (New York: Frederick A. Praeger, 1955), p. 252; Abraham Kaplan, *The Conduct of Inquiry* (San Francisco: Chandler Publishing Co., 1964), pp. 145–147; Fred N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt, Rinehart and Winston, 1964), pp. 359, 375–391; John R. P. French, Jr., "Experiments in Field Settings," in Leon Festinger and Daniel Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Holt, Rinehart, and Winston, 1953), pp. 98–135; Samuel A. Stouffer, "Some Observations on Study Design," *American Journal of Sociology,* 55 (January, 1950), 355–361.

[31] Some scholars have successfully employed experimental method in the study of politics. One work employing a design especially similar to ours is Leon J. Kamin, "Ethnic and Party Affiliations of Candidates as Determinants of Voting," *Canadian Journal of Psychology,* 12 (March, 1958), 205–213. Other experimental studies of political behavior include the works of Theodore Becker, most of which builds upon his *Political Behavioralism and Modern Jurisprudence* (Chicago: Rand-McNally, 1964); see also J. A. LaPonce, "An Experimental Method to Measure the Tendency to Equibalance in a Political System," This REVIEW, 60 (December, 1966), 982–993.

[32] See the authors' forthcoming "Alienation and Support for Demagogues," *Polity,* 1 (June, 1969).

Findings: The Political Explanation. Do party symbols in fact independently contribute to the unattractiveness of demagogic candidates? This question can be approached by manipulating party symbols in the electoral situation and by comparing those persons for whom party is very salient with those for whom it is not.

We accomplish the first by comparing our experimental and control groups with respect to propensity to choose demagogic candidates. The denial of partisan electoral alternatives to our experimental group appears dramatically to increase their attraction to demagogues (Table I). Indeed, these results are very consistent with those in the literature on non-partisan elections and referenda. Our confidence in the implications of this literature is increased, however, because of the tighter control of possibly contaminating variables in this quasi-experimental context.

Second, do those persons who do not identify with a political party, who generally reject partisan criteria for electoral choice, show a greater propensity to choose extremist candidates? Like the respondents of the experimental group, we would expect independents to operate in a condition—albeit self-imposed—which is devoid of party choice criteria. If so, they should indeed be more attracted to demagogues than those who identify with party. However, Table II reveals that this is not the case. In fact, the independents are less attracted than either Democrats or Republicans. Insofar as these data are concerned, this political explanation fails. The cultural attributes of the Republicans (e.g., as determined by sense of citizen duty or alienation), partially dependent upon social variables such as wealth and education, may help to explain the fact that their DDC is lower than that of the Democrats. However, a similar cultural explanation cannot be mustered to account for the low DCC of the independents. Although the independents of our sample are neither as politically inert nor as politically alientated as the sterotypic independent one envisages from contemporary voting studies, neither are they rich in civic culture.[33] Indeed, we have no data to explain the anomaly of the independents.[34]

The political explanation fares a little better when we consider the effect of denial of party alternatives together with party identification. Such denial of course increases the propensity to choose demagogues of both party identifiers and independents (Table III). However, the effect is least visible among independents and most pro-

TABLE I. EFFECT OF PARTISAN ALTERNATIVES ON CHOICE OF DEMAGOGUES

	Experimental Group (N = 400)	Control Group (N = 427)
Demagogic choice coefficient*	.53	.33

* Demagogic Choice Coefficient (DCC) is simply *fo/fe* calculated from the distribution of electoral choices within the two groups. That is, since there were six discrete options available to members of the experimental group, random distribution would place one-sixth of the 386 subjects responding or 64.3 in the demagogic category. Only 34 subjects were observed so to choose, however. Hence the DCC for the experimental group is 34/64.3 or .53. The control group had eight electoral options owing to the addition of two partisan choices. Thus, *fe* of demagogic choice is one-eighth of the 418 subjects responding or 52.2. However, *fo* = 17; hence DCC = 17/52.2 = .33. The use of such calculations rather than raw percentages is necessary because of the disparity in the number of choices available. The raw percentages of respondents choosing demagogic candidates, though their absolute values are of relatively little meaning given their heavy dependence both upon the number and kind of other electoral choices available, are for the experimental group, 9%; for the control group, 4%.

TABLE II. EFFECT OF PARTY IDENTIFICATION ON CHOICE OF DEMAGOGUES

	Democrats (N = 428)	Independents (N = 198)	Republicans (N = 197)
Demagogic Choice Coefficient	.49	.36	.41

[33] Though there are problems of rapidly diminishing cell size, attempts at controlling the DCC-party identification relationship for cultural attributes generally reduces slightly the magnitude of Republican-Democratic differences; the independents, however, retain the lowest DCC scores. That the DCC's of the party identification groups are not mere reflections of distributions of cultural or social attributes is evident from the following data. The party identification groups are remarkably homogenous. Republicans are of slightly higher social status and approximate the civic culture more closely than Democrats or independents. However, these differences attain statistical significance on only two of six variables.

[34] We suspect that there is a political explanation

TABLE III. PARTISAN ALTERNATIVES, PARTY IDENTIFICATION, AND CHOICE OF DEMAGOGUES

	Experimental Group			Control Group		
	Democrat (N=216)	Independent (N=90)	Republican (N=78)	Democrat (N=202)	Independent (N=106)	Republican (N=114)
Demagogic Choice Coefficient	.61	.40	.47	.32	.30	.36

nounced upon Democrats. This is in line with what we would expect; since party is less salient to independents, the non-availability of partisan alternatives should have relatively little impact on the criteria they employ in electoral choice. The dramatic increase in the propensity of experimental group Democrats to choose demagogues and the relatively small parallel increase among Republicans is undoubtedly due to the traditional Democratic dominance of the area. Republicans have been conditioned to rely less on party label for political guidance. They may rely on other electrical criteria, and hence denial of partisan alternatives is not a highly disorienting experience. Democrats, on the other hand, have been steeped in party symbols and hence should rely upon them more. Sudden removal of this structuring variable would understandably disorient them. This disorientation appears to result in increased choice of demagogic electoral alternatives.

It is clear that the existence of party symbols decreases the likelihood of voters choosing demogogic candidates. The political explanation of the simultaneous presence of democratic stability and viable parties is supported by these data. Despite the fact that our expectations about party identification were not met, the existence of partisan alternatives undeniably affected the behavior of our subjects. Party seems in and of itself a causal agent in decreasing attraction to demagogues and thus promoting stability.

The Cultural Explanation. As noted above, much literature suggests that political stability, and hence the unattractiveness of demagogues, is dependent upon cultural antecedents. Though —somewhat at variance with this literature— our data suggest an independent effect of parties, they do not preclude the possibility of cultural variables having a simultaneous impact. Various works suggest that a cluster of psychological orientations toward political objects is at the root of stable political order. This cluster is composed of a large number of variables— among them, certainly, alienation, anomie, and

for the independents' low DCC, but of a slightly different kind. Evidence collected by Malcolm Jewell suggests that some southern independents may be in a transitional stage between Democratic party identification and Republican party identification. Such persons are dealing with their dissatisfaction with the Democrats decidedly within the partisan context. Consequently, demagogues may not be attractive to them.

Mean Values on Cultural and Social Variables of Party Identification Groups

	Democrats (N=428)	Independents (N=198)	Republicans (N=197)	Kruskal-Wallis Test, Value of H
Alienation	3.04	3.04	2.88	2.68, $p>.05$
Anomie	2.55	2.46	2.22	8.90, $p<.05$
Sense of Citizen Duty	4.01	3.97	4.16	5.76, $p>.05$
Campaign Activity Index	1.84	1.77	1.95	3.77, $p>.05$
Education*	5.02	5.38	5.47	10.66, $p<.01$
Income*	2.98	3.09	3.37	5.44, $p>.05$

* The means for these variables are calculated from the ordinal numbers assigned to categories into which respondents classified themselves.

feelings of obligation to participate in politics.

Thus, we would expect persons who exhibit low levels of alienation and anomie and an acute sense of citizen duty to eschew demagogic electoral alternatives.[35] We investigated this proposition by examining the relationship of these three cultural variables to choice of hypothetical demogogic candidates. Table IV reveals that our expectations are met. In fact, with the distribution of scores on the independent variables trichotomized, there is a monotonic progression of the Demagogic Choice Coefficient in the anticipated direction for all three variables.

[35] In additions to suggestions in Almond and Verba, *op. cit.*, clear evidence confirming this expectation is found in the authors' "Alienation and Support for Demagogues," *op. cit.*

TABLE IV. EFFECT OF CULTURAL VARIABLES ON DEMAGOGIC CHOICE COEFFICIENTS

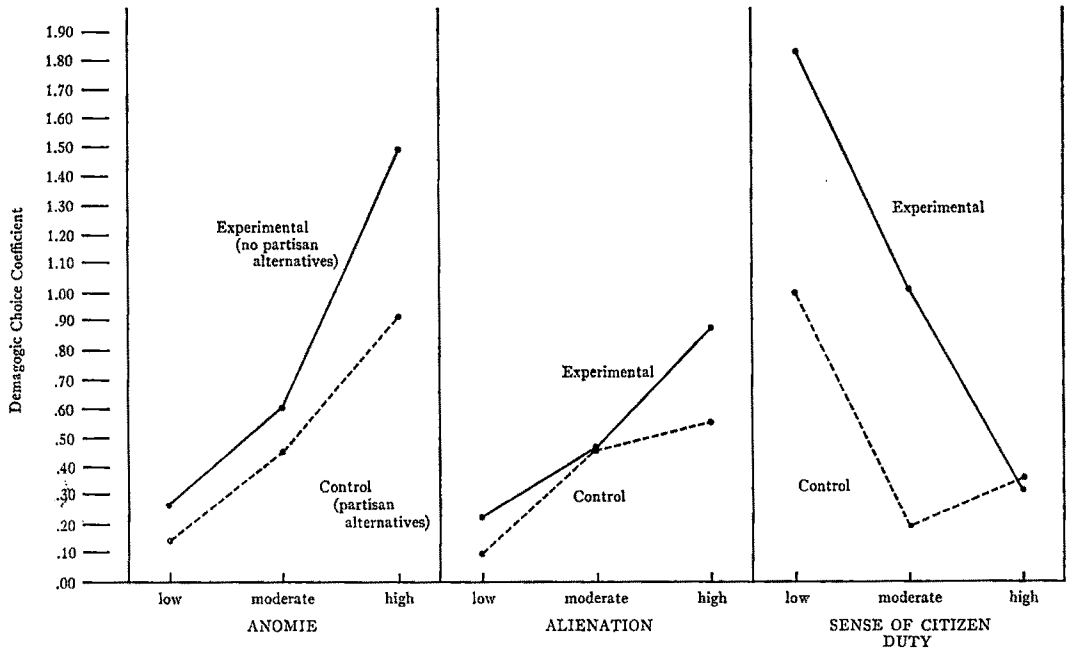
	Low	Moderate	High
Anomie	.21	.52	1.21
Alienation	.16	.43	.70
Sense of Citizen Duty	1.53	.61	.28

Hence, attraction to demagoguery, in addition to being dependent upon party, is partially explained by cultural variables. Since we know by virtue of our experimental controls that the effects attributed to party cannot be due to the uncontrolled presence of these cultural variables, it appears that both the political and cultural

TABLE V. EFFECT OF CULTURAL VARIABLES AND PARTISAN ALTERNATIVES ON DEMAGOGIC CHOICE COEFFICIENTS

	Experimental Group			Control Group		
	Low	Moderate	High	Low	Moderate	High
Anomie	.26	.59	1.47	.14	.42	.89
Alienation	.22	.43	.83	.10	.43	.51
Sense of Citizen Duty	1.89	1.00	.27	.97	.15	.29

FIG. 1. The Interaction of Cultural Variables and Partisan Alternatives in Their Effects Upon Demagogic Choice.



explanations have elements of validity. Neither explanation can be discarded in its entirety. It thus remains to be asked just how these two sets of variables interact to produce their effects upon attraction to demagogues. It is clear that the party choice variable operates among persons of all cultural orientations (Table 5).

We posited that the availability of partisan electoral alternatives would have its smallest effect when values of the cultural variables approached extremes. Anomic, alienated persons who are weak in feelings of civic obligation may be beneath a cultural threshold where partisan symbols can be meaningful. On the other hand, persons with opposite cultural complexions may find the educative effects of party superfluous. The existence of partisan symbols should operate most effectively on those persons who are between these cultural extremes. An examination of Table 5 reveals that this is not the case. The graphing of these data (Figure 1) clearly shows an unanticipated interactive pattern. Instead of finding that removal of party produced the greatest effects at moderate cultural levels, we note that the largest impact is at one extreme. While persons rich in civic culture appear to be so disinclined to choose demagogues that party is largely redundant, it is definitely not the case that citizens of opposite orientations are beneath a level where partisan symbols are meaningful. Instead the relationship is linear: the more cultural variables conduce toward choice of demagogues, the greater is the sensitivity of this choice to the presence of party symbols.

IV. CONCLUSION

There is some evidence that where voters are able to respond to party symbols, very moderate political behavior takes place. This phenomenon has been attributed to 1) the educative effects of parties *per se* (the political explanation) and 2) to the holding of particular "psychological orientations toward social objects" by citizens (the cultural explanation). Advocates of the first seem to feel that parties inhibit voters from indulging immoderate predispositions, while defenders of the second would argue that moderation (and viable parties) are dependent upon the *absence* of any such predispositions. This paper, by demonstrating that voters' propensities to choose demagogic political candidates is dependent both upon availability of party symbols and upon cultural variables, lends support to both arguments. In addition, it attempts to specify the cultural conditions under which party symbols can be particularly effective variables.

The number of efforts directly and systematically to investigate the roots of political stability and instability is small. Despite the inaccessibility of behavioral manifestations of these conditions (like the attractiveness of demagogues), attempts should be made to research them. Though the findings of this paper do not confirm all of our expectations and though the relationships shown are not all startlingly dramatic, they do demonstrate the feasibility of quantitative inquiry into these kinds of classic political concerns.

DETERMINANTS OF INNOVATION IN ORGANIZATIONS*

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The present study is an attempt to identify the determinants of innovation in public agencies, i.e., the degree to which they adopt and emphasize programs that depart from traditional concerns. Innovation is suggested to be the function of an interaction among the motivation to innovate, the strength of obstacles against innovation, and the availability of resources for overcoming such obstacles.

The significance of the research can be viewed in terms of Hyneman's observation nearly twenty years ago that bureaucratic agencies ". . . may fail to take the initiative and supply the leadership that is required of them in view of their relation to particular sectors of public affairs. . . ."[1] His concern was the responsiveness of the public sector not only to expressed wants but to public wants that may go unexpressed, or be only weakly expressed, and whose utility is much more easily recognized by the informed bureaucratic official than by the ordinary citizen.[2]

While the results and conclusions to be reported appear to be largely valid for organizations in general, the empirical focus will be local departments of public health which, as a class, have had a rather dramatic succession of opportunities to respond to new public problems over the past twenty-five years. A brief introductory paragraph will orient the reader to the applied setting.

By the early 1940's—the end of the "traditional" period in public health programming from the viewpoint of this study—the American Public Health Association had defined the task of the local health department essentially in terms of a basic set of six services: vital statistics, basic sanitation, communicable disease control, laboratory services, maternal and child health services, and health education.[3] Since the middle 1940's, infectious diseases (which these services were designed to combat) have declined radically as a health problem. In the last two decades, the professional leadership has increasingly called upon local health departments to turn their attention to the control of chronic diseases, the prevention of accidents, the provision of mental health services, the control of the quality of water and air, to dental health needs and to a host of additional non-traditional concerns. Many local public health organizations have responded to these newer concerns as advancing technology has provided ways of meeting them. However, it appears that while some departments have indeed reacted innovatively, a great many others continue to pursue the traditional objectives, if not exclusively, then certainly with primary emphasis.

The study group for the present research included all full-time local health departments in Illinois, Michigan, New York, Ohio, and Ontario, serving a jurisdiction no greater than 600,000 in population, whose chief executive—the local health officer—had occupied his current position during the entire period of 1960–1964. This group comprised 94 agencies. Since one health officer refused to participate in the study, ninety-three elements were provided for analysis. The data were collected primarily by interviews with the local health officers during the summer of 1965.

I. THE CONCEPT OF INNOVATION

Because the term innovation has been employed so widely and ambiguously, it is essential

*I wish to express my gratitude for valuable advice and comments from Robert Friedman, Irwin Rosenstock, Philip Converse, Ferrel Heady, M. Kent Jennings, Robert Northrop, and John Romani. Many of the ideas were sharpened and elaborated in discussions during the preparation of "Innovation in State and Local Bureaucracies," by R. S. Friedman, L. B. Mohr and R. M. Northrop, a paper presented at the annual meeting of the American Political Science Association, New York, 1966. The research reported here was supported by the Public Health Service, Research Grant No. CH 00044 from the Division of Community Health Services.

[1] Charles S. Hyneman, *Bureaucracy in a Democracy* (New York: Harper and Brothers, 1950), p. 26.

[2] For a much more recent treatment of bureaucratic innovation and public responsiveness, see Herbert A. Simon, "The Changing Theory and Changing Practice of Public Administration," in Ithiel de Sola Pool (ed.), *Contemporary Political Science: Toward Empirical Theory* (New York: McGraw-Hill Book Co., 1967), pp. 86–120. "The 'power' to innovate," says Simon (p. 106), ". . . is probably the principal power of the bureaucracy in the realm of policy and value."

[3] Haven Emerson, *Local Health Units for the Nation: A Report* (New York: The Commonwealth Fund, 1945), p. 2.

to specify at the outset how the concept will be used and how it is related to other usages. Innovation will be defined here as *the successful introduction into an applied situation of means or ends that are new to that situation.* Alternative and generally more inclusive definitions have been offered in the past.[4] For example, there has been a frequent tendency to combine the idea of adoption or adaptiveness with the idea of invention; occasionally, the term "innovative" has been assigned to mean exclusively what is more generally called "inventive" or "creative."

It seems important at the present stage of research and theory to separate the idea of invention from the idea of innovation. Invention implies bringing something new into being; innovation implies bringing something new into use.[5] In the organizational world this distinction is particularly important, for we are interested at times in whether an organization can create something new for its own use or for exploitation by others, and at other times in whether an organization can successfully adopt goals or processes or policies that are new in the sense of being departures from its own tradition.

There appears to be a good deal of agreement now on some of the factors that most enhance inventiveness or creativity in organizations, including the availability of individuals capable of producing new ideas[6] and the development of an organizational pattern that maximizes flexibility and opens lines of communication.[7] Unfortunately, less unanimity can be reported on the correlates of organizational innovation. There has been much more empirical research in this area, but it consists of scattered projects representing different disciplines, motivated by different considerations, and employing a strikingly heterogeneous selection of independent variables.

If any one group of variables may be said to stand out among all others as empirically determined correlates of innovation, it is the group of interrelated factors indicating size, wealth, or the availability of resources. Mansfield,[8] Mytinger,[9] Hage and Aiken,[10] Eisenstadt,[11] and Rogers[12] all conclude that organizational size and wealth are among the strongest predictors of innovation in the sense of readiness to adopt new patterns of behavior. Other organizational characteristics have also been identified as predictors of innovation in this sense, including informality, complexity, and decentralization in organizational structure,[13] breadth of organizational goals, and absence of dominance by a single professional ideology.[14]

In addition to the organization itself, the environment of the organization appears to be extremely important in two ways. An organization may be more likely to innovate when its environment is rapidly changing than when it is steady. In this sense, "environment" includes such factors as market conditions, technological changes, clientele needs and demands, and the labor market. Burns and Stalker count this variable heavily as determining whether or not the firms they studied adopted new, more organic management techniques.[15] In addition, innovation should also be more likely when the *social* environment to which an organization (or an individual) belongs has norms that favor change than when its norms do not favor change. In this regard, Rogers reports research in which fairly strong correlations were found between innovation and the norms of the relevant com-

[4] See Victor A. Thompson, "Bureaucracy and Innovation," *Administrative Science Quarterly,* 10 (June, 1965), p. 2; Everett M. Rogers, *Diffusion of Innovation* (New York: Free Press of Glencoe, 1962), p. 308; James Q. Wilson, "Innovation in Organization: Notes Toward A Theory," in James D. Thompson (ed.), *Approaches to Organizational Design* (Pittsburgh: Univ. of Pittsburgh Press, 1966), p. 196; Homer G. Barnett, *Innovation* (New York: McGraw-Hill Book Co., 1953), p. 7.

[5] The same general distinction has been made by others. *Cf.,* Rogers, *op. cit.,* pp. 195–196, and Simon, *op. cit.,* p 107.

[6] See Gary A. Steiner (ed.), *The Creative Organization* (Chicago: The University of Chicago Press, 1965), pp. 16–18.

[7] Harold Guetzkow, "The Creative Person in Organizations," in Steiner, *op. cit.,* pp. 35–45; Tom Burns and G. M. Stalker, *The Management of Innovation* (London: Tavistock Publications, 1961), pp. 85–86, 89, 121–122; Thompson, *op. cit.,* p. 12.

[8] Edwin Mansfield, "The Speed of Response of Firms to New Techniques," *Quarterly Journal of Economics,* (May, 1963), 293–304.

[9] Robert E. Mytinger, *Innovations in Public Health* (unpublished doctoral dissertation, Univ. of California at Los Angles, 1965), p. 212.

[10] Jerald Hage and Michael Aiken, "Program Change and Organizational Properties: A Comparative Analysis," *American Journal of Sociology,* 72 (March, 1967), pp. 516–517.

[11] Samuel N. Eisenstadt, *The Political Systems of Empires* (New York: The Free Press of Glencoe, 1963), pp. 27, 33–112.

[12] Rogers, *op. cit.,* pp. 40, 285–292.

[13] Hage and Aiken, *op. cit.,* pp. 503–519.

[14] Mayer N. Zald and Patricia Denton, "From Evangelism to General Service: The Transformation of the YMCA," *Administrative Science Quarterly,* VIII, No. 2 (September, 1963), p. 234.

[15] Burns and Stalker, *op. cit.,* p. 96.

munity, placed on a dimension from "traditional" to "modern."[16]

Each of the foregoing correlates of innovation must be viewed as an attribute of a collectivity —an organization. To complete the list, numerous variables measured at the individual level have also been found to relate to the type of innovative change of interest to us here. It would appear from a review of the literature that chief among these are the attitudes of an individual toward change, or any ideology he may have that would influence a specific type of innovation. Such factors were found to be important by Blau, Fliegel, Rogers and Eisenstadt.[17] In addition, the "cosmopoliteness"[18] of an individual is reported to be a significant correlate of innovation by Mytinger and by Rogers.[19] Blau found both the competence of an individual and his material and status interests to be associated with innovation.[20] A positive professional orientation was found by Rogers to be associated with innovation, as was opinion leadership status within a relevant communications network.[21]

It seems fairly clear, then, that the determinants of invention and the determinants of innovation in organizations are not identical. Inventiveness seems to be affected most by individual creativity and by the degree of hierarchical informality in organizational structure. Innovation, on the other hand, has been linked to size, wealth, environment, ideology, motivation, competence, professionalism, non-professionalism, decentralization, opinion leadership, and still other variables. Because of this difference, it appears highly desirable to distinguish innovation from invention in research.

[16] Rogers, *op. cit.*, pp. 285–292.

[17] Peter M. Blau, *The Dynamics of Bureaucracy* (2d ed. rev.; Chicago: Univ. of Chicago Press, 1963), p. 246; Rogers, *loc. cit.;* Frederick C. Fliegel, "A Multiple Correlation Analysis of Factors Associated with Adoption of Farm Practices," *Rural Sociology,* 21 (September-December, 1956), pp. 288–289, 291; Everett M. Rogers, "A Conceptual Variable Analysis of Technological Change," *Rural Sociology,* 23 (June, 1958), pp. 139–140, 143–145; Eisenstadt, *loc. cit.*

[18] Robert K. Merton, *Social Theory and Social Structure* (Glencoe: The Free Press, 1949 and 1957), pp. 387–420; Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly,* II (December, 1957), pp. 281–306, and (March, 1958), pp. 444–480.

[19] Mytinger, *op. cit.*, p. 195; Rogers, *Diffusion of Innovation, op. cit.*, pp. 285–292.

[20] Blau, *op. cit.*, p. 246.

[21] Rogers, *loc. cit.*

If the preceding discussion is considered in light of the definition of innovation offered above, it is clear that we have been elaborating upon the word "introduction" and upon the sense of the phrase "new to that situation." We have, in other words, emphasized that innovation is meant to exclude creativity *per se* and to include the notion of adopting something non-traditional whether it was invented within or outside of the organization concerned.

The term "successful," like "introduction," has important implications for the measurement of innovation. The successful introduction of some new method or goal implies its acceptance by the individual or group that constitutes the human element in a pertinent applied situation. If a health department tentatively introduces a new program, action of the department to increase emphasis on the program over time continues to be innovative behavior until the change has been completely accepted by organizational personnel, the public or other relevant groups. Such action is no longer innovative, of course, when the idea has become part of the organization's tradition.

The same point has been made in connection with innovation by individuals, but it is especially significant in organizational behavior, where we have long recognized and been concerned with the phenomenon of resistance to change.[22] It is important to note that both organizations and individuals may adopt a practice and then discontinue it for one reason or another or maintain it only on a token level. Rogers reports some studies in which discontinuance rates were found to be quite high.[23] It may help us to gain insight into innovation, therefore, if we do not restrict ourselves to its usual operational definition, the simple adoption of new practices, but include also a definition that allows for increased emphasis upon non-traditional programs recently introduced.[24] For this

[22] See Harold J. Leavitt, "Applied Organizational Change in Industry: Structural, Technical, and Human Approaches," *Handbook of Organizations,* ed. James B. March (Chicago: Rand McNally & Company, 1965), pp. 1144–1170, for a valuable summary and critique.

[23] Rogers, *Diffusion of Innovation, op, cit.*, pp. 88–93.

[24] See the introductory paragraphs, above, for examples of traditional and non-traditional programs. The primary sources for determining the precise composition of the two lists were "An Official Declaration of Attitude of the American Public Health Association on Desirable Standard Minimum Functions and Suitable Organization of

reason and others to be noted subsequently, two basic working measures of innovation will be employed in the analysis. One is the total number of non-traditional services adopted by the department. The other is the total number of personnel units (measured in man-years or the equivalent in dollars) that were added in all non-traditional program areas during the five-year period 1960–1964. This latter view of innovation will be labeled "progressive programming."

The term "new" appears almost invariably and as a matter of course in definitions of innovation, but it has an implication that should not be taken for granted. Innovation is difficult because it involves doing something new. The introduction of innovative practices into a social setting implies actions that entail a certain amount of uncertainty, risk, or hazard. This, then, suggests one significant factor that may help to explain innovation, i.e., there are certain obstacles or deterrents to innovation that may be more or less operative in any given case.[25] These are, first, the cost of such things as materials, time, or skills, and second, the power of human forces and fears, individual and social, that may be arrayed against the introduction of new means or ends. The latter category may include the power of tradition and social values, the power of individuals and groups who may be threatened by an innovation, and the power of one's concern for his own safety, security, and self-esteem, as well as the security of people who are important to him. The more staunchly these forces are arrayed against a particular innovation, the more difficult will it be for that innovation to take place, other things being equal. Furthermore, when one thinks of "newness" in these terms, it is not the absolute or objective sense of the word that counts, but rather the relative newness of an idea to a given role or set of roles, for it is in that context that obstacles to innovation will frequently arise. Thus, the innovations we will consider here are not necessarily new to the professional leadership in the field of public health, but they are indeed innovations for the local community, the organization, or the small clique of fellow health officers or local physicians.

Although obstacles may generally be expected to inhibit innovation, previous studies indicate that other factors may stimulate or enhance it. One individual or organization will usually have greater motivation than another to adopt a new idea. In addition, one may have greater resources than another, including not only the money and skills to overcome obstacles of expense, but also resources such as a position of authority, a charismatic effect, the support of prestigious individuals and self-confidence to overcome obstacles presented in terms of human forces.

On the basis of the foregoing considerations, the following three-dimensional hypothesis is specified: *Innovation is directly related to the motivation to innovate, inversely related to the strength of obstacles to innovation, and directly related to the availability of resources for overcoming such obstacles.*

It is thus possible to specify in more general terms why some of the many independent variables covered by previous studies were related to innovation; each indicated either a relative absence of obstacles or a relative presence of motivation or resources. Environmental changes and demands, for example, frequently constitute an important source of motivation, as do material and status interests and certain relevant ideologies. "Traditional" community norms, personal attitudes generally unfavorable to change, worker resistance to change, narrow organizational goals, lack of information, mechanistic decision structures—all of these are examples of obstacles to innovation. Competence and wealth are significant resources for innovation.

Having defined innovation and presented a theoretical framework in which to consider it, let us turn now to the findings of the research and an examination of the specific determinants considered. We shall have a two-fold purpose: a) to explain a substantial proportion of the innovation studied, and b) to arrive at a better understanding of the nature of the explanatory model itself.

II. MOTIVATION

Before constructing the interview schedules, four exploratory interviews were conducted with highly placed public health professionals. These interviews disclosed a consensus on the importance of attitudes of the health officer for the innovativeness of local health departments. Specifically, the health officer's public health ideology and his inclination toward activism were considered to be the best indicators of the likelihood that his organization would introduce and

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Health Activities," *American Journal of Public Health*, 30 (September, 1940), 1099–1106; and Harry S. Mustard, *Government in Public Health* (New York: The Commonwealth Fund, 1945), pp. 128, 140–182.

[25] Cf., Mytinger's discussion of "barriers" to innovation. Robert E. Mytinger, "Barriers to Adoption of New Programs as Perceived by Local Health Officers," *Public Health Reports*, 82 (February, 1967), 108–114.

emphasize non-traditional programs. Ideologically, a "conservatism-liberalism" dimension concerning the proper breadth of local public health programming was considered important. In addition, a health officer was considered more likely to innovate if he was a "go-getter," "aggressive," "ambitious," "interested in accomplishment," and "willing to stick his neck out." These opinions expressed in the exploratory interviews were supported by prior research, which both relates innovation to similar attitudes conducive to change and emphasizes the substantial importance of leadership motivation and behavior. Thus, the "activism" and the "ideology" of the health officer, taken together, were selected to indicate motivation to innovate.

Activism is more precisely defined as the health officer's perception of the extent to which the role of local health officer requires interaction with others, especially outside the health department, to obtain ideas, support, approval, and resources for departmental programs. The attitude was measured by a 23-item Likert-type instrument that was completed by the respondent in the presence of an interviewer. The individual items were designed to measure opinions regarding the desirability of four different kinds of role activity: a) attempts to influence the health power structure in the community, i.e., higher administrative and legislative officials, the local medical society, and influential community leaders, b) attempts to obtain support, such as grant support, beyond local appropriations, c) relative emphasis to be placed upon interagency and public related affairs as opposed to intra-agency activities, and d) attempts to seek out community problems as opposed to waiting for them to be pointed out by others. It should be repeated that "activism" as measured is an attitude or perception rather than a demonstrated behavior.

Public health ideology is more precisely defined as the health officer's opinion regarding the scope of services that should properly be offered by the local public health agency (as distinguished from local private and voluntary enterprise) in non-traditional public health program areas. The great majority of full-time local health officers in the area covered by this research are physicians, most of whom have had years of experience in private practice. These individuals are highly likely to feel conflicting ideological pulls from their dual identification with the medical profession and the profession of public health, the one allocating to the private sphere all medical care except those programs specifically or traditionally relinquished to government by organized medicine, and the other allocating to the public sector all unmet health needs. Not surprisingly, many of the programs considered innovative in public health today, such as screening for chronic diseases, for example, are addressed toward meeting unmet health needs traditionally allocated to private medicine. The public health ideology instrument was therefore designed to determine how far the health officer leaned toward either of these poles by asking his opinion of the proper locus of control for twenty-six kinds of medical and health care. The form and method of administration of this instrument were essentially the same as that of the activism scale.

Turning now to the findings concerning attitudes of leaders, our first concern is with the relationship between activism and ideology themselves. If it could be shown that each measure was correlated with innovation, but not highly intercorrelated, then they could profitably be combined. Their joint explanatory power would be greater than either attitude alone. Our day-to-day experience indicates that the liberal is not necessarily energetic in pursuing his views, nor is the conservative necessarily a passive reactor to events. There was good reason to predict that activism and ideology would not be strongly related and that indeed proved to be true with this group of local health officers. The correlation between the two variables is $r = .27$, a statistically significant relationship in a group this size ($p < .005$), but certainly not a strong one.[26] For further analyses, the single variable "activism-ideology" was constructed, which represents the simple sum of the two attitude scores for each health officer.[27]

The finding of central interest in this section,

[26] A significance level does not have the usual meaning here, since the group studied is a population rather than a probability sample. However, the test does provide some additional information about the strength of the reported relationship. It tells us the probability that a relationship this strong would appear if the group were divided into categories at random rather than according to actual scores on the independent variable. In light of these considerations, I have elected not to sprinkle significance levels throughout the report but to provide some bench marks that may be used as a guide by the interested reader: For $N = 93$, using a one-tailed test and the .05 level, the correlation $r = .18$ is significant; the comparable coefficients for other sample sizes in which we will be interested are $r = .21$ (for $N = 69$), $r = .24$ (for $N = 49$), and $r = .30$ for ($N = 33$).

[27] Since the standard deviation of the ideology scores is greater than that of the activism scores (10.9 to 7.8), ideology contributes slightly more than activism to the summed index.

the correlation between the activism-ideology of the health officer and progressive programming, is $r = .36$. However, to interpret that finding it is necessary to rule out the effects of community size, since progressive programming is an expression of the absolute amount of resources added in non-traditional program areas during 1960–1964, and large communities will naturally be served by greater numbers of personnel than small ones. When the control is introduced, the resulting partial correlation becomes $r = .40$,[28] indicating that the size of communities makes essentially no difference for the relationship between activism-ideology and progressive programming in these agencies.[29]

The relationships just reported do not hold uniformly for the entire group of health officers studied. In Ontario, apparently owing to cultural differences in attitude toward the role of government and the station of private medicine, the relationship between ideology and innovation was slightly negative.[30] The data do not provide a direct explanation of this finding, but it became quite evident during the interviews that many Ontario health officers conduct a number of innovative programs in earnest while at the same time believing that this type of activity is, in general, more properly the responsibility of private medicine. From responses to open-ended questions, it appears that Ontario health officers generally feel even more strongly than those in the four American states that personal health matters are the province of private practice, but they also feel it quite proper for the government to step in when this responsibility is not being fully met. The orientation is a peculiar but understandable mixture of British socialism on one hand and small town American capitalism on the other. Because of such differences, ideology as measured here was not a good indicator in Ontario of the motivation to innovate, nor was it in any other way a good predictor of innovation in the Ontario departments. With this understood, the findings reported below will at times be given both for the group as a whole and also for the four American states considered separately ($N = 69$). In the American states alone, for example, the correlation between progressive programming and the activism-ideology of the health officer is $r = .40$, and when community size is controlled the partial correlation becomes $r = .44$.

It is desirable to show how activism-ideology is related to the total number of non-traditional programs adopted as well as to progressive programming; only then may we really compare these results with the results of other studies, most of which have defined innovation as the simple adoption of new ideas. However, the total group of organizations cannot be used in this analysis; many programs might have been adopted before the incumbency of the current health officer, making his attitudes largely irrelevant.[31] We must use a subgroup of organizations in which all adoption of non-traditional services can reasonably be attributed to the health officer who was interviewed. Specifically, the subgroup includes all health officers who had held the post in that organization for at least 15 years, i.e., since 1950, plus each health officer whose annual rate of adoption during 1960–1964, when multiplied by his total years of tenure, yields a product that is equal to or greater than the total number of innovative programs adopted by the department.[32] This group contains 33 U. S. organizations and, including Ontario, 49 departments in all. The subgroup is small but quite well representative of the entire study population on key indicators.[33] We find that in this subgroup the correlations between activism-ideology and adoption are essen-

[28] Separately, the correlation between activism and innovation, controlling for community size, is partial $r = .32$; for ideology and innovation, controlling for size, partial $r = .29$.

[29] It will be of interest to note here how much innovation actually took place. Progressive programming ranged in these agencies from zero to 25.5 man-years, with a mean of 3.5 and a standard deviation of 4.9. Adoption, which will be considered in a moment, ranged from 2 programs to 27, with a mean of 11.7 and a standard deviation of 6.1.

[30] $r = -.17$, a non-significant correlation.

[31] Nor would it have been feasible to consider only post-1960 adoptions: such a procedure would have penalized the early adopters, many of whose innovations were made prior to 1960.

[32] For example, one health officer had adopted 1 non-traditional program per year during 1960–1964 and had been in the job for 10 years. One might, therefore, reasonably estimate that 10 of the department's non-traditional programs are associated with his incumbency. Since this department had a total of 14 non-traditional programs it was not included in the subgroup. If its total had been 10 or less, it would have been included. If the health officer had been there for 15 years, the department would have been included regardless of total number of adoptions, for almost none of these programs had been introduced in local health departments before 1950.

[33] The means for the whole group and for the subgroup, respectively, are: number of programs adopted, 11.7 and 11.2; population of jurisdiction, 110,000 and 109,000; 1959 expenditures, $188,000 and $165,000; activism-ideology, 119.9 and 120.1.

tially the same as those reported above, when progressive programming was the dependent variable. In the 33 U. S. departments in the subgroup, for example, the uncontrolled correlation between activism-ideology and the number of programs adopted is $r = .38$. When community size is controlled, the partial correlation becomes $r = .43$. In the entire subgroup of 49 organizations, the corresponding product-moment correlations are $r = .38$ and $r = .40$. (To make comparisons among groups and subgroups convenient for the reader, separate intercorrelation matrices among the major variables in the study are provided in Tables 1 and 2).

Thus, we have good reason to conclude that the motivation of the health officer, as measured, is indeed related to the innovation of the department, whether this be defined as number of programs adopted or as increments in resources devoted to those programs.

On the other hand, it is noteworthy that behavior in this study is only *moderately* predictable from attitudes, and especially from ideology. Furthermore, the zero order correlation between ideology and innovation observed here ($r = .24$, $N = 93$) is not substantially different from that obtained in similar studies. In the research of Fliegel and of Rogers cited above, the comparable relationships were $r = .42$ and $r = .26$, respectively. In the study by Hage and Aiken, the comparable measure was slightly negative ($r = -.15$). Blau and Mytinger concluded that ideology was important for change, but their results were not obtained through systematic measurement of the ideology of respondents and subsequent statistical analysis of all cases.

While it is possible that ideologies are so difficult to measure that their true effects cannot often be captured, two alternative explanations seem more compelling. Both begin with the inference, drawn from the empirical results discussed above, that ideology *in itself* is indeed *not* an important predictor of innovation. The first alternative is that a mere willingness to innovate, as reflected by ideology, is an attitude of secondary importance to the *determination* to innovate. The second alternative is that an attitude such as ideology may lead to predicted behavior only when *other* important conditions are also favorable. These possibilities suggest two research strategies. One is to measure the intensity with which an ideology is held in addition to measuring its mere existence, for in that way one may well capture the difference between those who are merely willing to behave consistent with an ideology and those strongly impelled to do so. Secondly, there should be an attempt to measure other conditions that may determine whether or not an ideology will be expressed in behavior in a given situation and to

TABLE 1. CORRELATIONS AMONG THE PRIMARY VARIABLES FOR ALL HEALTH DEPARTMENTS STUDIED*

	B	1	2	3	4	5
A. Adoption	*.70* .70	*NA* NA	*.40* .46	*.67* .65	*.64* .63	*NA* NA
B. Progressive Programming		*.40* .36	*.33* .41	*.60* .60	*.60* .61	*.49* .33
1. Health officer activism-ideology			*−.04* .01	*.10* .12	*.08* .10	*.24* .15
2. % of the population in white collar occupations				*.41* .42	*.41* .45	*.28* .11
3. Health department expenditures, 1959					*.88* .88	*.35* .26
4. Population of the health jurisdiction, 1959						*.38* .26
5. Extent of public health training of key employees						

* Correlations in italics apply to the U.S. health departments studied, only.

NA: Not applicable, since some adoptions may have predated the incumbency of the health officer and key employees.

TABLE 2. CORRELATIONS AMONG THE PRIMARY VARIABLES FOR HEALTH DEPARTMENTS IN WHICH ADOPTION IS ASSOCIATED WITH THE HEALTH OFFICER INTERVIEWED*

	1	2	3	4
A. Adoption	*.38* .38	*.30* .36	*.70* .63	*.64* .63
1. Health officer activism-ideology		*−.21* −.14	*.10* .12	*.08* .10
2. % of the population in white collar occupations			*.27* .30	*.23* .30
3. Health department expenditures, 1959				*.83* .83
4. Population of the health jurisdiction, 1959				

* Correlations in italics apply to the U.S. health departments studied, only.

analyze the relationship between attitudes and behavior in the light of such conditions. In the present study, for example, it is quite possible that health officer attitudes resulted in innovative behavior only when other conditions, such as the balance between obstacles and resources, were also favorable. This possibility will be explored after other relevant findings have been presented.

III. OBSTACLES

Two sources of obstacles were thought to be important in explaining differences in innovation among local health departments: the extent to which a) the community and b) the organization itself are prepared to accept or resist such departures from established practice.

Community obstacles to innovation. Since no direct measure of community readiness to innovate was available, an attempt was made to study it indirectly by ascertaining social class levels, as measured by education (per cent of the population 25 years old and older who have completed high school) and occupation (per cent of the labor force in white collar occupations). The rationale for using these data as measures of readiness is derived from other research. It has been shown that the social class of an individual is correlated with his beliefs and behavior regarding the efficacy of preventive health actions;[34] by extension to the aggregate level, public acceptance of the efficacy of preventive health measures may render a community favorable to action by its local health department. Another type of evidence indicates that the higher social classes tend to vote in favor of measures (such as bond issues) perceived as benefitting the general public, even to the extent of voting against their own apparent self-interest. Banfield and Wilson refer to this attitude as "public-regardingness."[35]

Our finding is that education level, as measured, is only weakly related to innovation (the correlation with progressive programming is $r = .23$; with adoption, $r = .24$). When controlled for community size, the partial correlation of education level with progressive programming is only $r = .03$, and with adoption, $r = .04$.[36] Occupation level, on the other hand, was strongly enough related to innovation to retain at least some independent explanatory power. The extent of white collar employment is re-

[34] See Don P. Haefner, S. Stephen Kegeles, John P. Kirscht, and Irwin M. Rosenstock, "Preventive Actions Concerning Dental Disease, Tuberculosis, and Cancer," *Public Health Reports*, 82 (May, 1967), 451–459.

[35] James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," this REVIEW 58 (December, 1964), 876–887.

[36] The percent who completed two or more years of college may well have been a better measure for our purpose. Unfortunately, such information is not available from the normal central sources, such as the *County and City Data Book* published by the U.S. Bureau of the Census. It should definitely not be concluded on the basis of these correlations that aggregate education level has little or nothing to do with resistance to change in communities.

lated to the number of programs adopted at the level $r = .46$ and to progressive programming at the level $r = .41$. When community size is controlled the relationships drop to $r = .25$ and $r = .19$, respectively, so that occupation level does not provide a great deal of additional explanatory power once the size of the community is considered. We will keep in mind, however, that if extent of innovation seems to be determined by community size the reason may lie partly in a relationship between size and social class characteristics. At some level of probability, for example, the larger the health jurisdiction, the greater the per cent white collar, and therefore the greater the readiness to accept innovations in public programs.

Organizational obstacles to innovation. Within the organization, the readiness to accept innovations was measured by the extent of public health training of key lower echelon employees—their capacity to handle the supervisory aspects of the new programs.[37] Some of these supervisors hold advanced degrees in public health, some have had undergraduate programs with substantial public health content, and some have participated in special courses administered by state and federal health agencies and by nearby universities. Some, on the other hand, have had little or no specific public health training; their supervisory capacity is based largely on years of work experience with traditional programs.

A correlation of $r = .33$ was obtained between the public health training of supervisors and progressive programming in the department. When controlled for community size, the partial correlation becomes $r = .22$. It is of interest to note that the lack of training of supervisors is an obstacle primarily when the *health officer* is motivated to innovate. Dividing the organizations into two approximately equal groups according to the activism-ideology of the health officer, we find that when motivation to innovate (as measured) is low, the correlation between progressive programming and the training of supervisors is only $r = .15$, but when motivation is high, this relationship is just under $r = .40$. Thus, when the health officer appears ready to innovate, lack of training at the supervisory echelon can apparently constitute one further obstacle to organizational innovation.

The interview data, supported by impressions obtained during the field work, suggest that continuing education of staff nurses and sanitarians through short courses dealing with the more novel program areas is effective in winning their cooperation and enthusiasm. When asked whether the department operated a specific program, many health officers complained that the unwillingness of lower echelon employees had proven to be an insurmountable obstruction to desired innovation. This seemed to apply particularly to nurses quite advanced in age, a fairly common personnel characteristic of local health departments. On the other hand, new programs such as mental health nursing were frequently established or substantially expanded in some departments after most or all of the employees to be involved had taken the opportunity to receive special training for these new duties. There was no evidence of an employee's refusing or even resisting such training. Thus, if they are not already doing so, it is suggested that leaders who would like to put more emphasis on non-traditional services should devote serious attention to the matter of employee training.

IV. RESOURCES, SIZE AND INNOVATION

It was expected on the basis of prior research that a great deal of the variance in program innovation among local health departments might be explained by the amount of money available to the health department for innovation. The primary operational definition of resources, therefore, was simply the level of expenditures of the health department in 1959 (the last year prior to the period over which progressive programming was measured). Clearly, expenditures do not precisely indicate resources available for innovation, since funds might possibly be inextricably bound to ongoing, traditional activities. It was tentatively assumed, however, that greater absolute income would generally imply greater discretion in the commitment of resources, not only in local health departments but also in organizations in general. For example, the health department with ten nurses might send one nurse at a time to a training course in mental health, but the health department with only one or two nurses might have great difficulty in doing so.

The finding is that there is indeed a strong relationship between expenditures and innovation in these organizations—stronger than that between innovation and either the activism-ideology of the health officer or obstacles presented

[37] Extent of public health training was measured on a ten-point scale. The data were obtained through a self-administered questionnaire left with the supervisors at the time of the health-officer interview. It would have been far better to have obtained this information on all professional employees in the department, but turnover is such that very few in each department had been there from 1960 through 1964.

by community and organizational factors. The correlation between expenditures and progressive programming is $r = .60$, and that between expenditures and the number of non-traditional programs adopted is $r = .65$. We must ask, however, whether "expenditures" is really an indicator of resources or, perhaps, an indicator of the motivation to innovate. Is there a strong relationship because larger *communities* need or demand more programs, or because the greater resources accompanying *organizational* size *enable* more extensive innovation in some departments?

One excellent way to attack this question is to carry out a three-variable causal analysis using partial correlation and regression.

Unfortunately, extreme multicollinearity would seem to render current methodology for the evaluation of causal models unreliable in this case.[38] Despite this problem, and especially considering that there is no attempt here to make inferences to a sampled universe, the analysis was undertaken for the insights and cues to further analysis it might provide.

The partial correlation between expenditures and adoption, controlled for community size, is $r = .27$. In terms of causal interpretation, this result is ambiguous. The relationship is depicted by model *c* in Figure 1, but one wonders

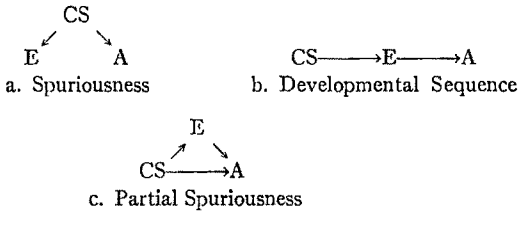


FIG. 1. Possible causal relationships among health department expenditures (E) the adoption of innovative programs (A) and community size (CS).

whether it actually looks *more* like model *a* or model *b*, both of which are contained in model *c*. Since essentially the same coefficient is expected in both cases, the partial correlation does not enable us to ascertain whether this is *primarily* a relation of spuriousness (expenditures and adoption are related mainly by the coincidence that community size caused them both to vary) or a developmental sequence, community size caused expenditures to vary and expenditures, in turn, caused adoption to vary). However, the partial slope of adoption on expenditures, controlling for community size, does suggest a particular causal interpretation. If the true causal relationship were developmental (model *b*), a control for community size would leave the partial regression coefficient essentially unchanged—the slope should not be affected by controlling for a prior cause—whereas if complete spuriousness were involved (model *a*), the partial regression coefficient would be reduced to zero.[39] Analysis shows that in this case, the slope is in fact reduced, but not substantially, when controlled for community size. The zero-order slope of adoption on expenditures is $b = .017$, and when controlled for size the partial slope becomes $b = .011$. The inference to be drawn is that a partially spurious relationship exists, but one that includes a substantial direct effect of expenditures on innovation. Community size causes organizations to be large (while at the same time motivating them to be innovative in some small degree) and the size of the organization then makes possible the variety of programming that constitutes the dependent variable.

Let us now examine some additional factors in order to place size in even sharper perspective. In preceding analyses, it was observed that many variables associated with innovation were also associated with community size, suggesting that the effects of size might not be so pronounced if these other specific variables were not involved. These include the activism of the health officer, the training of key employees, and, most importantly, the expenditure level of the health department. Although the correlation between community size and the number of innovative programs adopted is $r = .63$, when it is controlled for health department expenditures, the relationship is reduced to partial $r = .15$. When additional controls are introduced for health officer activism and per cent white collar, the relationship is reduced further to partial $r = .09$. This "partialling out" of the effects of community size implies that, for these data, how community size acted upon innovation in the health department may be almost completely understood in terms of the other variables just listed.

Thus, we can tentatively conclude that community size was important for innovation in this study because it connoted a summary of factors that included motivation, obstacles, and resources in a highly conducive combination. Nei-

[38] The correlation between community size and health department expenditures is $r = .88$; between expenditures and adoptions, $r = .65$; between size and adoption, $r = .63$.

[39] See Hubert M. Blalock, Jr., *Causal Inference in Nonexperimental Research* (Chapel Hill: University of North Carolina Press, 1961), pp. 83–91.

ther community size, farm size, size of health department, nor size of firm should be accepted hastily as an accurate predictor of innovation. Size itself is not related to innovativeness by logical necessity; it becomes significant only when it implies or indicates the conceptual variables that are important in themselves.

Motivation, obstacles, and especially resources account substantially for differences in the number of innovative programs operated by agencies of different size. It is now desirable to ascertain whether larger health departments put greater relative emphasis on innovative services than smaller ones, or if they merely adopted many more programs. A new dependent variable was constructed to investigate this question—the proportion of total increases in resources devoted to initiating and expanding non-traditional services during the period 1960–1964, or, put another way, the ratio of progressive programming to increases in all programs, new and existing, traditional and non-traditional. For brevity, let us call this variable "proportional innovation." The correlation between health department expenditures and innovation so defined is $r = .04$. This result is surprising, but unequivocal; health department size and, presumably, the resources available as a consequence of size, had on the average almost no impact upon proportional innovation during this period of time.

It would be incorrect to infer that the resources bound up with organizational size (or even only with local health department size) are invariably unrelated to proportional innovation. It may be, for example, that had this same study been carried out ten years earlier, the correlation between health department expenditures and proportional innovation would have proved substantially higher. It is possible that at that time very few small departments had adopted any non-traditional programs, whereas the large departments no doubt already had. In that case, the correlation between expenditures and proportional innovation would almost certainly have been greater ten years ago than .04, although the total amount of innovation in all health departments at that time may have been quite small. The fact remains, however, that size —and therefore the resources implied by size— was not associated with greater proportional innovation in these agencies. Until contrary empirical evidence and sound theoretical justification are presented, one must at least remain skeptical of a causal relationship between organizational size and the proportion of increased resources that will be allocated to innovation.

Proportional innovation might perhaps be considered a faulty operational measure in that large departments, because of a substantially greater growth rate, may actually be more innovative than small departments when they devote approximately the same proportion of their growth to non-traditional services. Growth rates are not greater with increased size in these organizations, however, nor has such been found to be true of other kinds of organizations. "It is now generally agreed that there is little or no relation between size of firm and expected percentage rate of growth. Mansfield (pp. 1034–35) finds slightly higher growth rates for small firms *that survive* than for large firms, but the differences are slight, and do not affect the general argument."[40] The correlation between health department size and growth during 1960–1964 as a percentage of size is $r = -.16$. Thus, large and small organizations are increasing at roughly the same rate.

Although it is clear that size on the average had no appreciable impact on proportional innovation, it is equally clear that large departments did tend to adopt substantially more non-traditional programs than small ones and it is germane to ask which resources may have led to this effect. To find answers to this question, an analysis was made of the median size of the health departments adopting each non-traditional program. Each program was assigned a numerical score—the median size of the agencies that adopted it—and the highest and lowest one-third of a list of programs ranked according to this criterion were singled out for special attention. The complete analysis is too lengthy to be presented here in detail, but the major conclusions can be stated briefly. First, the need or demand for a greater variety of programs in large communities occasioned only a few of the observed differences in number of programs adopted. Services such as air pollution control, the coordination of a number of community health agencies, and occupational or industrial health were, as might be expected, more likely to be found in large urban health jurisdictions. The remaining differences suggested by the two lists may be accounted for most reasonably by differences in resources between small and large departments. The effective resources appear to be as follows:

a. Small departments reach an early limit on variety of programming because of a limitation on the number of different activities into

[40] Herbert A. Simon, "Comment: Firm Size and Rate of Growth," *Journal of Political Economy*, 72 (February, 1964), p. 81. See also the articles by Mansfield and by Simon and Bonini cited there.

which one person's time may be fragmented. Large departments are not so limited simply because their greater number of personnel gives them the flexibility of assigning at least one full-time or half-time employee to each of a great many services.

b. Small departments have difficulty attracting specialized personnel such as nutritionists, research scientists, social workers, and medical specialists. This limitation tends to restrict their non-traditional programs to those for which traditional kinds of personnel may be employed. Large departments attract specialized personnel much more easily.

c. Small departments have little slack in funds obtained from local sources, thereby depending heavily on single-purpose grants for innovation. Large departments, on the other hand, are able to devote a substantial percentage of their local funds to non-traditional programs.

d. Small departments innovate largely in program areas for which grant funds are readily available from the state health department. Large departments are more likely to receive funds and personnel directly from the federal government, as well.

These, then, appear to be the important specific resources enabling large local health departments to adopt many more non-traditional programs than smaller ones. Note that in another setting, such as an industrial organization, the effects of size upon innovation would no doubt be due to an entirely different set of resources, and in some general cases, size may have no implications whatever for the resources available for innovation.

Because they lack the resources needed for diversification, small health departments are unable to support a great variety of non-traditional services. However, by intra-organizational transfers, state grants and other means, they were able to find the resources to place as many personnel in non-traditional programs as larger agencies, in proportion to size, over the five-year period, 1960–1964. Moreover, when they did acquire additional resources during this period, they put as great a proportion of the total into non-traditional programs as did their larger counterparts. These three facts lead one to suspect that, on the average, the smaller agencies devoted their increased resources to relatively fewer non-traditional programs but on a relatively greater scale. A case-by-case analysis confirmed this suspicion. On the other hand, large health departments are able to support a great variety of services and, clearly, do so. They are exercising an option, however. Why should the large departments choose to adopt a large number of programs when they could easily use the same funds to launch fewer programs on a larger scale or to expand existing innovative enterprises?

This question may be answered at least in part by the concept of "slack" innovation introduced by Cyert and March.[41] Slack innovation in these organizations would be innovation motivated largely by a desire for prestige and professional status on the part of the health officer and other health department staff members. In general, if organizational capability remains after the most pressing problems are attended to, some or all of such capability tends to be allocated to status-motivated innovation. Note that this outcome would probably be manifested here by a greater number of different non-traditional activities, as opposed to increased emphasis upon existing activities, which does not ordinarily confer further prestige. Thus, since variety brings the bonus of professional approval, and since large departments have the resources to support greater variety, they have a strong tendency to adopt many more innovative programs than their smaller counterparts. In the general case, when an organization possesses the resources necessary to operate more innovative programs and can thereby obtain professional approval for its members, we would expect to find that it does indeed operate more. One important contributory condition is the general absence of systematic, dependable, retrospective evaluation of decisions regarding the allocation of resources.

Thus, this discussion points to large organizational size as a *facilitator* of innovation more than as a motivator of innovation. The analysis is consistent with the hypothesis concerning slack innovation offered by Cyert and March and it also helps explain some apparent discrepancies in the relationship between size and innovation in industrial firms. For small firms, technological advances may often be examples of what Cyert and March call "problem" innovation, since the firm may need these new techniques to bring about indispensible improvements in its market position. For this reason, relatively unsuccessful firms have been found to be leaders in the adoption of some specific, extremely important technological improvements.[42] However, if one studies a broad range of innova-

[41] Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, New Jersey: Prentice-Hall, 1963), pp. 278–279.

[42] Walter Adams and Joel B. Dirlam, "Big Steel, Invention, and Innovation," *Quarterly Journal of Economics*, 80 (May, 1966), 167–189.

tions, the larger and more successful firms—the firms more able to undertake slack innovation—should be found to lead the field in total adoptions. Such was the finding of Mansfield in his research,[43] and that finding has been replicated here with respect to local health organizations.

It may by no means be concluded from this discussion that either large or small departments do a more effective public health job—because of greater breadth of innovative programming, on one hand, or greater depth, on the other. One can think of numerous reasons why one style or the other might lead to a more effective total program, or why style in this sense would be totally irrelevant. In the last analysis, the question can only be resolved by systematic evaluation of total program effectiveness.[44]

Nor should we conclude that innovation is necessarily frivolous when motivated by concern for professional status rather than organizational production or service goals. A more realistic view is simply that such innovation is often non-rational: how adequately a formal organization will serve the needs of its public, especially unexpressed needs, appears to depend largely upon the values assumed by one or more professional groups in their own struggles for social prestige and survival. It is problematic whether value choices that are good or bad for a professional group lead to action choices that are good or bad for an organization and its public. No doubt, status innovation is sometimes at the root of profound progress while at other times it is merely a waste of resources.

V. A MULTIPLICATIVE MODEL OF INNOVATION

Support has been provided for the inference that innovation is a function of motivation, obstacles, and resources. It remains to determine how these variables act in combination. In such a case, the political scientist and the sociologist frequently turn to the linear additive multiple correlation procedure, a mathematical model that suits the exigencies of survey data quite well. If the analyst were to use a multiplicative model, for example, he might well find himself in a series of awkward positions as he confronted negative values, the desirability of making changes of scale (such as standardizing), and the absence of true ratio scales, as in attitude measurement. The tools for depicting non-additive relationships mathematically are not as accessible, nor do they appear to be as versatile as additive tools.

Multiple correlations provide quite "good" results, in terms of proportion of variance explained, when applied to the data of this study. (For example, the multiple correlation of progressive programming with expenditures, activism-ideology, per cent white collar and supervisor training in the four American states is $R = .73$.) However, it is not entirely reasonable to apply such a model. It is more reasonable to expect an interactive rather than an additive result.

One relationship of interest is that between the activism-ideology of the local health officer and the adoption of non-traditional programs by the department. In the group of forty-nine health departments in which the health officer interviewed was highly likely to have presided over the initiation of all the agency's non-traditional programs, it was found that these two variables were related at the level $r = .38$, both in the study population as a whole and in the U. S. health departments considered separately. It may well be, however, that this relationship is contingent to some extent upon the balance of obstacles and resources prevailing in different cases. When the obstacles are relatively great and the resources small, for example, it would be predicted that even the highly liberal and active health officer might not find it possible to innovate, at least not in substantially greater measure than his counterpart with relatively low motivation. If the balance were tipped heavily in the other direction, however, health officer motivation would be expected to make a great deal of difference. The highly motivated health officer would probably be extremely innovative, while his counterpart with relatively little motivation might not introduce any changes at all in spite of the ease with which he could apparently do so.

In order to test this prediction the group of forty-nine health departments was divided into "high" and "low" subgroups according to the level of resources available to them, and the regression of innovation on health officer attitudes was examined within each subgroup. A relatively low regression coefficient would be expected in the group characterized by few resources, but a relatively high slope in the group with resources more favorable to innovation, where the attitudes of the health officer would be likely to have substantial effect.

In the total group of forty-nine organizations, the regression coefficient of innovation (the adoption of non-traditional programs) on the

[43] Mansfield, *op. cit.* See also his, "Technical Change and the Rate of Imitation," *Econometrica*, 29 (1961), 741–766.

[44] For an excellent methodology for evaluation of public programs, see O. L. Deniston, I. M. Rosenstock and V. A. Getting, "Evaluation of Program Effectiveness," *Public Health Reports*, 83 (April, 1968), 323–335.

activism-ideology of the health officer is $b = .15$. In the twenty health departments whose expenditures in 1959 were below $100,000, the comparable slope was found to be $b = .06$, whereas in the twenty-nine health departments with expenditures above $100,000, it is $b = .28$. When resources are high, in other words, a unit increase in health officer motivation, as measured, has about 4½ times the effect upon innovation as it does when resources are low. (In the U. S. health departments considered separately, the comparable coefficients are $b = .16$, for the group as a whole, and b = .07 and $b = .22$ for the subgroups characterized by low and high resources, respectively.) This difference in slopes suggests that there is, in fact, an interaction in the relationship such that the two independent variables, motivation and resources, may well provide an accurate *multiplicative* prediction of innovation.

Since we are dealing here with three interval —or assumed interval—scale variables, it is possible to go somewhat beyond the recognition of a difference in slopes in testing the fit of these data to a multiplicative mathematical model. The model would state that

$$I = k \cdot M^{b_1} \cdot R^{b_2}, \qquad (1)$$

where I = Innovation, M = Motivation, R = Resources, and k is a constant of proportionality. However, we may establish a prediction equation using the transformation

$$\log I = \log k + b_1 (\log M) + b_2 (\log R) \qquad (2)$$

and use standard least-squares procedures for the purpose of estimation.[45] It would then be of interest to calculate the goodness-of-fit of this logarithmic expression of the relationship, and to compare it with the fit of an ordinary linear additive model to the same data, employing the same variables. The ordinary multiple correlation of the adoption of non-traditional services with both health officer attitudes and health department expenditures, using only this subgroup of forty-nine health departments, is $R = .70$. In terms of proportion of variation explained, $R^2 = .49$. The proportion of variation in these same innovation scores explained by the antilog of the right hand side of equation (2), above, is $r^2 = .53$. Thus, the multiplicative model provides at least as good a fit to the data as the additive model. When only the health departments in the four American states are considered, the goodness-of-fit of the additive model is $R^2 = .59$, whereas the multiplicative model again yields a slightly higher goodness-of-fit measure, $r^2 = .66$ (as a correlation, $r = .81$). When obstacles are considered as well,[46] the goodness-of-fit measures for the multiplicative model in the whole group and in the U. S. only are $r^2 = .63$ and $r^2 = .72$, respectively, whereas this time for the additive model they are exactly the same, $R^2 = .63$ and $R^2 = .72$.

Thus, the empirical observations are consistent with the hypothesis that innovation is a multiplicative function of the motivation to innovate and the resources available for innovation. We were not able to obtain a highly satisfying discrimination between the two models; it is true that the multiplicative model does not provide a clearly better fit than the additive model, or *vice versa*. However, for two reasons noted by Blalock, fairly good additive approximations of relationships that are actually more complex are to be expected: "In general, additive models seem to approximate reality reasonably well, given a limited range of variation and sizeable 'random' fluctuation that make accurate specifications difficult."[47] As behavioral science progresses, however, we will hopefully capture a greater range of values on each variable employed, so that the nature of effects will be more clearly delineated, and we will also both improve the validity of specific measurements and consider or control more of the factors relevant to each analysis, so that random and nonrandom fluctuation will be reduced. As we approach attainment of these objectives, it is logical that

[45] Blalock, *Causal Inferences in Nonexperimental Research, op. cit.*, pp. 91–92. For other suggested treatments of multiplicative relationships, see Hubert M. Blalock, Jr., "Theory Building and the Statistical Concept of Interaction," *American Sociological Review*, 30 (June, 1965), 374–380; James S. Coleman, *Introduction to Mathematical Sociology* (New York: The Free Press of Glencoe, 1964), pp. 224–235; Bruce M. Russett, Hayward R. Alker, Jr., Karl W. Deutsch, and Harold D. Lasswell, *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), pp. 322-340; Hayward R. Alker, Jr., *Mathematics and Politics* (New York: The Macmillan Company, 1965), pp. 108–111; Hayward R. Alker, Jr., "The Long Road to International Relations Theory: Problems of Statistical Nonadditivity," *World Politics*, 18, (July, 1966), 639–644.

[46] The resources term in the equation becomes, in this case, an obstacles-resources term. It was calculated by adding together the expenditures and percent white collar scores, after first weighting each with the appropriate partial slope from the regression of adoption on activism-ideology, expenditures, and percent white collar.

[47] Blalock, "Theory Building and the Statistical Concept of Interaction," *op. cit.*, p. 375.

the mathematical models we employ be dictated as much as possible by theoretical considerations and as little as possible by disciplinary tradition. When two fairly simple forms of explanation are equally effective in organizing empirical events, the one that is theoretically more cogent is to be preferred.

It has been suggested more than once that the kind of theoretical case confronted in the present research involves a multiplicative relationship. Atkinson has proposed a general theory to account for "motivated" behavior, or achievement-oriented behavior, in which any such behavior is a multiplicative function of motivational variables, on one hand, and "expectancy" (the subjective probability of success), on the other.[48] Note that this latter psychological variable may easily be translated into our terms as an objective probability of success—the balance between resources and obstacles. The present results, therefore, may be seen as a specific instance of the operation of Atkinson's hypothesis. In yet another study, Palmore and Hammond have come to an identical analytical conclusion regarding closely similar independent and dependent variables.[49] After analyzing records of welfare recipients in New Haven, these investigators concluded that deviant behavior (juvenile delinquency) is a multiplicative function of what may again be viewed as indicators of motivation and resources. The motivational variables are summarized by the concept "blockage of legitimate opportunity" and are indicated operationally by race, sex, and success in school. The resource-obstacle variables are summarized by the concept "availability of illegitimate opportunity," and are indicated by measures of both family deviance and neighborhood deviance. Blalock, in fact, has attempted to draw a direct analogy between the variables discussed by Palmore and Hammond and those discussed by Atkinson.[50]

Taking Atkinson's proposed theory of "motivated" behavior as a hypothesis, we note that Palmore and Hammond have used the same kind of model and what appear to be very similar conceptual predictors to explain *deviant* behavior. The present research has again used a multiplicative model and similar independent variables to explain *innovative* behavior. Together, these two studies provide substantial empirical support for Atkinson's more general psychological formulation. Moreover, the concepts of innovation and social deviance are closely allied. Merton has specifically designated the deviant behavior of the gangster and the juvenile delinquent as "innovation."[51] In his well-known paradigm, innovation is distinguished from other modes of adaptation to cultural norms—conformity, ritualism, retreatism, and rebellion—by constituting a behavioral pattern in which cultural goals are accepted but institutionalized means for achieving them are rejected. This formulation has been clarified by Harary and others, so that the meaning of "the rejection of institutionalized means" in the case of innovation is expanded to include the substitution of other means for attaining the culturally approved goal.[52] The delinquent is an innovator, according to Merton, in that he has accepted the cultural goal of individual material success but rejected socially accepted means, and adopted non-accepted means, of attaining that objective.

Thus, it has been suggested that several related forms of behavior depend multiplicatively on the motivation to venture into a new pattern of action and the net resources available for accomplishing that purpose. The hypothesis has grown out of the analysis of empirical observations as well as from more abstract inference. Further research is now needed to elaborate upon this theoretical proposition, which would seem to have exceptionally wide application in the social sciences.

[48] John W. Atkinson, "Motivational Determinants of Risk-Taking Behavior," *Psychological Review,* 64 (1957), 359-372. Atkinson actually uses two aspects of total inclination to act—basic psychological drive, such as the need for achievement, and incentive value attached to the object of the behavior in question. These are multiplied together and it is their product that is multiplied by the expectancy factor to obtain a prediction of behavior. The present study does not distinguish the basic drive from the incentive component of overall motivation; both are probably involved to a certain extent in motivation as measured here. Atkinson's article has most recently been reprinted (and also updated) in John W. Atkinson and Norman T. Feather (eds.), *A Theory of Achievement Motivation* (New York: John Wiley, 1966), Chapter I. See also Chapter II, "Notes Concerning the Generality of the Theory of Achievement Motivation," by Atkinson and Chapter 20, "Review and Appraisal," by Atkinson and Feather.

[49] Erdman B. Palmore and Phillip E. Hammond, "Interacting Factors in Juvenile Delinquency," *American Sociological Review,* 29 (December, 1964), 848-854.

[50] Blalock, "Theory Building and the Statistical Concept of Interaction," *op. cit.,* p. 379.

[51] Merton, *op cit.,* pp. 131-194. See especially, p. 140.

[52] Frank Harary, "Merton Revisited: A New Classification for Deviant Behavior," *American Sociological Review,* 31 (October, 1966), 693-697.

VI. CONCLUSIONS

This research began with a concern for the responsiveness of public agencies to changes in the general problems they were designed to meet. We did not study the whole of this question but rather attacked one essential facet, the ability of such organizations to adopt and emphasize programs that depart from traditional behavior. On the other hand, our focus goes somewhat beyond the problem of responsiveness in that it is relevant to questions of organizational innovation in general, whether justified by environmental changes or prompted by any of a host of other possible stimuli.

It was initially proposed that extent of innovation is a negative function of obstacles and a positive function of the motivation to innovate and the availability of relevant resources. The data collected from 93 local public health organizations supported this proposition, although, as operationalized, the relationships were weak in some cases. The variable emerging as by far the most powerful predictor of innovation was "size," but we concluded that this relationship cannot be considered theoretically complete. Rather, size should be expected to predict innovativeness only insofar as it implies the presence of motivation, obstacles and resources. In the present case, organizational size proved to be an excellent indicator of the relevant resources available to local health departments for the adoption of a large number of non-traditional programs.

The analysis of the data yielded several additional conclusions that would seem to merit further test and elaboration:

a. In attempting to derive accurate predictions of behavior from attitudes or beliefs, such as ideologies, the belief itself should be qualified by the intensity with which it is held and one may well expect the belief-behavior relationship to be qualified by one or more contingent conditions.
b. With regard to many kinds of innovation in public organizations, the relationships among community size, organizational size and extent of innovation form a developmental sequence. Larger communities generally require larger service organizations, and it is the resources of the larger organization that lead to innovation. Thus, large organizations in small communities may be expected to adopt many innovations; small organizations in large communities will adopt few.
c. A great deal of innovation in organizations, especially large or successful ones, is "slack" innovation. After solution of immediate problems, the quest for prestige rather than the quest for organizational effectiveness or corporate profit motivates the adoption of most new programs and technologies.
d. Once the diffusion of innovations has progressed far enough to include late adopters, organizational size and wealth will have no bearing upon the relative emphasis accorded to innovative as opposed to traditional activities.
e. Lastly, and perhaps most importantly for theoretical development, innovation is viewed as a multiplicative function of the motivation to innovate and the balance between the obstacles and resources bearing upon innovation.

INDICATORS OF CROSS-NATIONAL AND INTERNATIONAL PATTERNS[1]

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Substantive foci in the study of international relations have altered in time with changes in the international system and the coming of academic age of new generations of scholars.[2] Prior to World War I, the central substantive concepts were international law and diplomacy. Historiography was the major method, and, given the nature of the historical approach during this period, few theoretical generalizations emerged.

World War I revolutionized the study of international relations. The horrible consequences of this first modern war and the idealistic fervor of the war years were instrumental in overlaying the traditional concepts of international law and diplomacy with two new foci: current events and international organizations. Feeling that the citizen should be made aware of the international world—educated for world citizenship—and that he should be given the guidance that the diplomatic history and international law specialist did not provide, many international relations scholars began to accent contemporary affairs. This new interest, however, had no methodological underpinning except fidelity to the "facts," and involved few attempts to delineate recurring patterns of events. The "guidance" given to the student often turned out to be little more than special pleading.

The focus on international organizations also reflected an internationalistic viewpoint. International organizations were conceived of as the structural beginning of world government and as a mechanism for international understanding and peace. This focus articulated itself in descriptive studies of the structure and rules of international organizations, past and present, and blueprints for their alteration.[3]

Although there were indications of another conceptual shift beforehand,[4] World War II marks the domination of the study of international relations by two concepts: politics and power. This change is evident in the role given international organizations. During the interwar years, they were a central concept of international relations and much that happened was related to them. Now, international organizations are conceived of as only one and often a minor element of the milieu in which power politics and conflict take place.

With World War II, there was not only a sharp shift in conceptual foci but in conceptual scheme as well. The previous scheme involved, to borrow the Sprouts' distinction,[5] *concrete structures*. The concepts of central concern referred to readily describable institutions and officially recognized behavior with status in international law. Thus, terms such as nation-state, colonialism, the League of Nations, warfare, and treaties were woven into a framework for describing the international (or most often, European) system. The shift in conceptual scheme was from *concrete structures* to *analytic components*. The emphasis changed from describing past and present official organs and behavior to underlying forces, conditions, and patterned regularity in behavior. If any work can divide the old from this new trend, it is Quincy Wright's *A*

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[2] Kenneth W. Thompson, "The Study of International Politics: A Survey of Trends and Developments." *Review of Politics*, 14 (October 1952), 433–467. See also Dwight Waldo, *Political Science in the United States of America* (UNESCO, 1956).

[3] This is not to belittle a normative or policy approach to international relations. Although current work in empirical and quantitative international relations may appear more removed from moral commitments by its abstractness, the values of the researcher are involved no less than in traditional policy-oriented scholarship. If any difference between empirical and quantitative international relations on the one hand and that which has gone before on the other is to be drawn, it is not in a difference in moral commitments. Rather, the difference lies in making assumptions, logic, and data explicit so that others may test any conclusions drawn *regardless of their values*.

[4] See, for example, Frederick Schuman, *International Politics* (New York: McGraw-Hill, 1933), and H. D. Lasswell, *World Politics and Personal Insecurity* (New York: McGraw-Hill, 1935).

[5] Harold and Margaret Sprout, *Foundations of International Politics* (New York: D. Van Nostrand, 1962).

Study of War,[6] which accents politics, trends, themes, systematic methodologies, and the contribution of the social sciences to international relations.

The new conceptual scheme with its emphasis on analytic components is not divorced from the older scheme, as any current text on international relations would attest. Concepts central to the field before each of the World Wars still title the chapters of our books. Rather than conflicting, the two schemes are complementary. The heavy analytic emphasis of the new conceptual view of international relations has served to tie together the older concepts in a theoretical and explanatory nexus. This is the chief significance of Morgenthau's now classic *Politics Among Nations*.[7]

As it grew within the field, however, the new conceptual scheme has appeared increasingly out of focus. Being analytic, accenting forces, and highlighting patterns, the scheme begs a precision that it lacks. If power and nationalism are forces, how are they defined? How do we know when and with what magnitudes they are operating? If regularities in international behavior exist, what are they? How are such regularities determined? Such questions were crucial to the new scheme, but have not been answered by its developers.

The elaboration of an analytic conceptual scheme was an acknowledged move towards a science of international relations. In being theoretically explicit and talking in terms of causal relationships, the proponents of the post-World War II outlook were self consciously "scientific." But they were scientific largely in form and not substance. Knowledge could not be accumulated because there were few systematically organized findings. Generalizations could not be disproved because they lacked empirical specificity. And theories, such as the balance of power, could not be tested because they lacked logical or mathematical rigor.

Even as the analytic conceptual scheme was consolidating its hold on the study of international relations, two trends became evident within a decade after World War II. One trend involved a number of international relations scholars, who, persuaded of the salience of the social sciences and dissatisfied with the imprecision of "power" and "politics," began to fill out the newer analytic scheme with borrowed concepts of "conflict," "community," "perception," "communication," and "hostility." Symptomatic of this trend were the works of Deutsch, Wright, and Pool.[8]

A second trend comprised the invasion of the field by scientists and scholars trained in other disciplines and methodologies. Concerned about the consequences of another World War, dissatisfied with the dominant conceptual scheme, and unmindful or disrespectful of the graduate school varieties and prevailing methodology, social psychologists, economists, physicists, mathematicians, and sociologists began to write about the international system. Research by Klineberg, Cantril, Richardson, Rapapport, Guetzkow, Schelling, to name a few,[9] began to contest prevailing approaches and concepts.

If tied together by any thread, these trends involving the importation of social science concepts and the movement into the field of "outsiders" shared an emphasis on quantitative techniques and methodologies. Within and without the field, the elaboration and focusing of the prevailing analytic scheme has emphasized data, measurement, statistical arrays, and mathematical models. Attempts to add precision to the concepts and explicitness to the themes have been like a river winding from its source, with more or less breadth or depth from one study to the next, but generally tending in the same direction.[10]

[6] Quincy Wright, *A Study of War*, 2 vols. (Chicago: University of Chicago Press, 1942).

[7] Hans J. Morgenthau, *Politics Among Nations* (New York: Knopf, 1948).

[8] Ithiel de Sola Pool, *Symbols of Internationalism* (Stanford: Stanford University Press, 1951); Karl W. Deutsch, *Nationalism and Social Communication* (New York: Wiley, 1953); Quincy Wright, *The Study of International Relations* (New York: Appleton-Century-Crofts, 1955).

[9] O. Klineberg, *Tensions Affecting International Understanding* (New York: Social Science Research Council, 1950); H. Cantril, *Tensions That Cause Wars* (Urbana: University of Illinois Press, 1951); L. F. Richardson *Statistics of Deadly Quarrels* and *Arms and Insecurity* (Pittsburgh: Boxwood Press, 1960); A. Rapoport, *Fights, Games, and Debates* (Ann Arbor: The University of Michigan Press, 1960); H. G. Guetzkow, "A Use of Simulation in the Study of Inter-Nation Relations," *Behavioral Science*, 4 (1959), 183-191; Thomas C. Schelling, *The Strategy of Conflict*. (Cambridge: Harvard University Press, 1960).

[10] The river simile can be used to make another point. Quantitative research in international relations does not presume an antithesis between traditional and quantitative scholarship. On the contrary the two are complementary. As the course of a river is determined by the fertile valleys through which it flows, so is quantitative research channeled by traditional insight and knowledge. Together the land and the river produce an abundant harvest and together the precision and re-

But conceptual sharpness through quantification has tilted the analytic scheme out of focus along another dimension. Concepts such as communication can be defined by mail flows, to be sure, but why not also or alternatively by telephone calls, telegrams, exports of periodicals and books, diplomatic notes, official conferences, or any summation or ratios formed from these activities? Indeed, once the door is opened to quantification, the quantifier finds himself in a dilemma similar to that of the verbalizer. Power and international politics can be and are differently defined according to the background and imagination of the scholar. With an infinity of variables and masses of data at his disposal, the quantifier can "operationalize" national power as the defense budget, men under arms, total energy resources, or national income, and politics as bloc membership, U. N. voting, military aid, or state visits as his predilection and interests dictate.

In spite of the eclecticism about variables that has infused quantification in international relations, there is an underlying rationality about operationalizing central concepts. For one thing, variables are selected as *indicators* of *underlying* processes or forces not directly measureable themselves. Thus, GNP per capita may be selected to measure economic development, mail to measure communications, and threats to measure conflict. Moreover, variables are selected to be as statistically uncorrelated as possible. Otherwise, the variables are redundant: one of them could do as well as any of the others. Finally, they are selected to have high validity. They should indeed be indicators of that they purport to measure.

Although few would deny the soundness of these requirements, they have led to a multitude of variables, even in the "operationalization" of the same concepts, for several reasons. One is that there has been no large scale mapping of the correlations and patterns of variation among potential indicators. When selecting indicators for an empirical study, quantifiers have had to use their judgement as to what potential measures were highly correlated. Are exports, exports per capita, or ratio of exports to GNP highly correlated indicators of transactions such that any one would be sufficient, or do they measure independent aspects of transactions between nations?

A second reason is that there have been no studies concerned with the validity of possible indicators. How do we know that GNP per capita is the best indicator of economic development? Perhaps we should agree with David McClelland[11] in selecting electricity consumption per capita as the central measure.

A final reason is that there have been few empirical attempts to simultaneously treat the whole conceptual framework. Concepts have been added to the analytic scheme, some have been operationalized and studied empirically, but always as discrete studies. The empirical interrelatedness of all these concepts, such as power, politics, conflict, transactions, communications, economic development, international organizations, perception, geography, and so on, have not been determined. It is not surprising that the choice of indicators for such concepts has been eclectic, since not only is it unclear as to how a variable relates to that which it is to measure, but the empirical patterns among the processes, structures, and forces being separated for study are themselves unknown.

The purpose of this paper is twofold. One goal is to contribute towards a refocusing of the analytic conceptual scheme employed in the study of international relations. Rather than being based on quantification alone, however, this refocusing will entail fastening major concepts to major patterns of variation in the empirical attributes and interactions of nations. The second and primary goal is to help and encourage a definition of the best indicators of these major concepts.

The remainder of the paper will propose several criteria for selecting a preferred set of indicators and a number of measures that more or less satisfy these criteria. The bulk of the discussion will consist of the evidence for selecting the indicators they measure.[12] This evidence is based on dozens of large scale analyses involving in total several hundred variables defining attributes and behavior of nations. The data cover the years ranging from 1836 to the 1960's. To give maximum visibility and comprehension to this evidence, the salient findings will be presented schematically in a number of tables. Those wishing more precise information about these findings will be referred to the proper source.

liability of quantitative scholarship combined with the rich comprehensiveness of traditional scholarship bear new fruit.

[11] David C. McClelland, *The Achieving Society* (New York: D. Van Nostrand, 1961).

[12] At this point I will have to increasingly rely on a technical vocabulary to communicate the criteria and evidence. If the more traditionally trained scholar has gamely stayed with me so far, I beg his further indulgence. Technical terms I will use to a minimum, but I must employ such terms if any of the accumulated evidence is to be offered.

The primary technique involved in generating this evidence is factor analysis. The technique is appropriate for a large scale mapping of empirical concepts in a domain such as international relations and for delineating the best indicators of those concepts.[13]

I. CRITERIA FOR A SET OF BASIC INDICATORS

What criteria might a set of variables satisfy to be preferred or basic indicators of major attribute and behavioral concepts of nations? Let me offer seven.

1) *Data must be readily available on the indicators for most nations.* An indicator such as the "number of secret agents" would hardly be a feasible choice.

2) *Data should be comparable on a basic indicator.* For some variables, the definitions under which data are aggregated vary from nation to nation. Gross national product and national income, for example, are measures for which the data do not mean the same between, say, Poland and France, and for which exchange rate conversion to common currency adds additional elements of noncomparability.

3) *Each basic indicator should be measured on a ratio or interval scale.* Each nation will then usually have a distinct value on the indicator and the indicator will be usable in all mathematical models, including those employing calculus. If the choice of indicator must be between ordinally scaled variables, then that variable having more disrete values (more ranks) is to be preferred.

4) *Each basic indicator should index a pattern of variation among nations.* A pattern of variation is a cluster of intercorrelated variables and a basic indicator should be central to the pattern.[14] Why this should be so may not be immediately evident. Consider "electricity generated," which is a variable related to *economic development* and *size* patterns.[15] How are we to interpret this variable's correlation with other variables such as trade, threats, tourists, or domestic unrest? The correlation may be due to variance in economic development, in size, or in both. However, were we to use a variable indexing only *one* pattern of nations, then the variance involved in that variable's correlations would not be ambiguous. Moreover, since the variable indexes a pattern of intercorrelated variables, *what correlations or findings hold for the variable hold also for the other variables involved in the pattern.* This enables comparison of results for variables in the same pattern and facilitates convergence on scientific laws and theories.

5) *The set of basic indicators should encompass the major variance among nations.* That each indicator should index a pattern implies that the major patterns reflecting most of the variation among nations will be indexed by the set as a whole. With knowledge of such a set of basic indicators, the student of international relations can be sure of covering the major differences and similarities among nations. These basic indicators would be useful, for example, in relating national systems to foreign conflict, national societies to foreign policy, or international behavior to nation attributes. Most importantly, and apropos the introduction to this paper, the set should serve as a collection of indicators with known correlates and from which specific variables can be selected to operationalize the concepts of a theory or hypothesis.

6) *The set of basic indicators should be divided into two types: basic indicators of the attributes of nations and basic indicators of the relations between nations.* This distinction is clearly the same as that dividing comparative politics from international relations—concern about the characteristics of nations versus concern about their relations with each other. For our purposes, the need for two types of basic measures rests on an analytical difference. Nations are our units of analysis. Basic indicators of their attributes then define the variation among these units. Our major concern is not with the units themselves (as it is with comparative politics or comparative economics, for example), but with the behavior of the units vis-a-vis each other. For this primary concern we require a different set of basic indicators: those of nation behavioral patterns. Perhaps, the analytical difference between basic nation *attribute* indicators and basic nation *behavior* indicators can be seen best in terms of two matrices. The nation attribute matrix is a nation by attribute table, where each cell refers to the value of a nation on an attribute, such as GNP, trade, or migrants. These tables are standard in presenting data on nations, such as in the United Nations *Statistical Yearbook*.

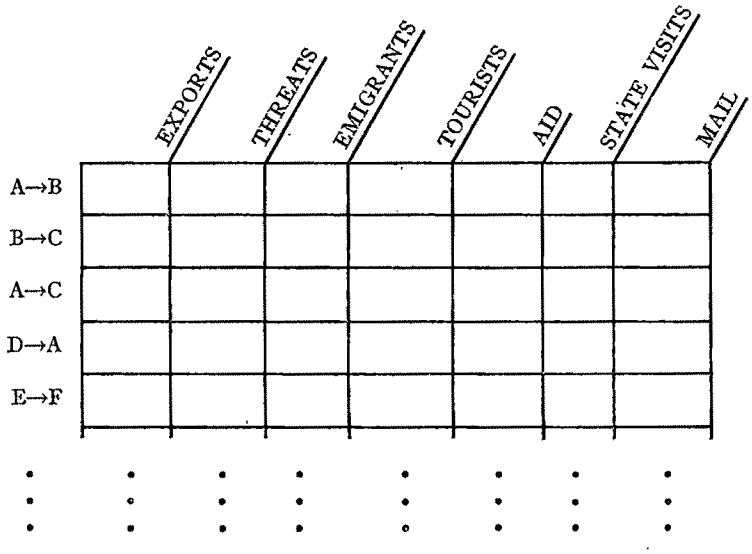
The nation behavior matrix is a *dyadic* nation by behavior table. Each cell of the table refers to the action of a specific nation, such as the U. S., towards a specific other nation, such as

[13] Elsewhere I have published a paper on "Understanding Factor Analysis" *Journal of Conflict Resolution,* 11 (December, 1967), 444–480, which might be helpful auxiliary reading for the remainder of this paper.

[14] In some other papers I have used the term "dimension" rather than "pattern." The latter term is preferred here because it may communicate better the basic idea than does the more mathematical and abstract notion of dimension.

[15] That economic development and size are patterns of nations will be established subsequently.

the USSR, for a behavior defined by the column of the table. Let the letters A, B, C, etc., stand for different nations. Then such a dyadic table would look like the following:



The arrow in A → B means A is the actor and B the object. For the exports variable for example, the data on A → B would be A's exports to B.

7) *That each basic indicator indexes a pattern must be established empirically, systematically, and reliably.*

At this stage in the growth of a quantitative international relations, it would be foolhardy to suggest anything more than a first provisional list of indicators, more or less satisfying these seven criteria. Further studies and the application of these indicators will refine and alter this list. With this tentativeness in mind, I would like to suggest the following major patterns (empirical concepts) of nations and their indicators.

A. *Attribute Indicators*

	Major Patterns
1. energy consumption/population	economic development
2. population	size (power bases)
3. bloc membership[16]	political system
4. killed in foreign violence	foreign conflict behavior
5. killed in domestic violence	domestic conflict behavior
6. Roman Catholics/population	Catholic culture
7. population/area	density

B. *Behavior Indicators*

	Major Patterns
1. tourists A → B	salience
2. emigrants A → B/A's population	emigration and communications
3. weighted UN voting distance A↔B	UN voting
4. students A → B/A's students to all nations	foreign student
5. exports A → B/A's GNP	export
6. IGO A↔B/A's IGO membership	international organizations
7. military violence (factor) score A → B	official conflict behavior
8. embassy or legation A → B/A's total embassies or legations	diplomatic representatiion
9. U.N. voting distance on "Self Determination" issue pattern A↔B	self-determination voting
10. anti-foreign behavior (factor) score A → B	anti-foreign demonstrations

[16] Bloc membership refers to western, neutral, or eastern bloc membership.

The next section will present evidence for selecting the set of attribute indicators and the subsequent section will do the same for the behavior indicators. A final section will discuss some of the implications of the combined set of indicators.

II. ATTRIBUTE MEASURES

As part of the research of the Dimensionality of Nations Project (DON), and as discussed elsewhere,[17] a factor analysis of 236 measures of eighty-two nations for 1955 data delineated fifteen distinct patterns of variation. These fifteen patterns account for seventy-seven percent of the differences and similarities of nations on the data. Many of these patterns are trivial, however, and can be ignored in defining the primary empirical concepts and their basic indicators. The *major* patterns[18] consist of the seven shown in Figure 1, which account for about fifty-six percent of the *total* variance of nations on the 236 variables, and seventy-two percent of the total variance tapped by the original fifteen patterns.

In the analysis variables were organized by *type* of attribute. For example, the agricultural type involved agricultural workers/economically active population, agricultural production/GNP, and agricultural population/population variables. To keep the size of the Figure manageable, the relationships between variables and patterns are shown in terms of attribute types.

The names given to the seven patterns are based on those variables most highly involved in each. They are meant to be descriptive and mnemonic labels only and are not to be interpreted as presumed causes underlying the patterns.

The results given in Figure 1 define the *uncorrelated* patterns of interrelated variables. The patterns shown, however, will depart from reality as the empirical patterns in the data are themselves correlated. Rather than risk presenting artifactual results due to an a priori requirement imposed on the patterns found, an additional analysis relaxing this restriction was done.[19] The best definition of the correlated or uncorrelated data patterns is shown in Figure 2.

This secondary analysis brought forth the same seven patterns, although the relationship of the variables to the patterns was slightly altered. These correlated patterns best define the central variables and are shown in Figure 2, along with the highest "loading" variables for each pattern. A "loading" is a coefficient measuring the amount of variation of a variable involved in a pattern. The higher the loading, the higher the involvement. Negative loadings mean inverse relationship between variable and pattern.[20] The sum of squared loadings shown in the figure and which is calculated for all 236 variables measures the relative size of the patterns. The higher this sum, the more the variation in the variables involved in the pattern.

Those variables that most satisfy the seven criteria for a basic indicator are shown blocked in Figure 2.

How reliable are these seven patterns? Several similar analyses of nation attributes have been done by others, among which are those of Cattell,[21] Berry,[22] and Russett.[23] Table 1 gives

[17] Jack Sawyer, "Dimensions of Nations: Size, Wealth, and Politics," *American Journal of Sociology,* 73 (September 1967), 145–172; R. J. Rummel, "The Dimensionality of Nations Project," in Richard Merritt and Stern Rokkan (eds.), *Comparing Nations* (New Haven: Yale University Press, 1966), pp. 109–129; R. J. Rummel, *Dimensions of Nations,* forthcoming, 1969.

[18] These are orthogonally (varimax) rotated factors. The initial solution was determined using the principal axes technique with unities in the diagonal of the correlation matrix. Correlations were product moment and were calculated on variables with an average of about seventeen percent missing data. All factors with positive eigenvalues were extracted and rotated and all positive eigenvalues exceeded unity. Further details and results are given in Rummel, *Dimensions of Nations.*

[19] This analysis comprised the oblique (biquartimin) rotation of the initial fifteen factor solution. Because of the number of variables involved, the rotation was carried out in stages of ten major cycles each until there was no major alteration in loadings from one stage to the next.

[20] Because of small correlations between the patterns, the loadings can be loosely interpreted in our oblique rotation as the correlation between variable and pattern.

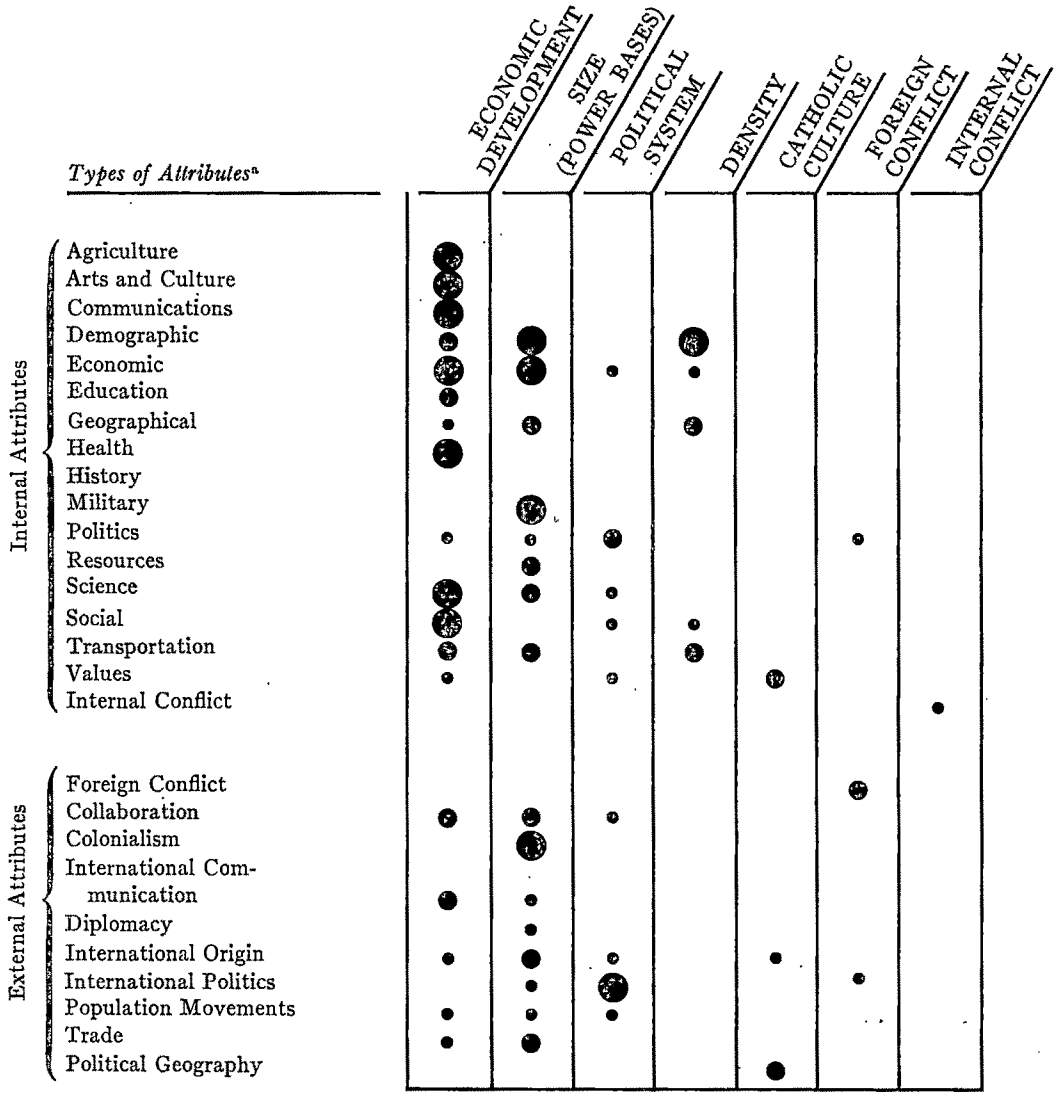
[21] Raymond B. Cattell, "The Dimensions of Culture Patterns of Factorization of National Characters," *Journal of Abnormal and Social Psychology,* 44 (1949), 443–469; Raymond B. Cattell, H. P. Hartman, and H. Breul, "An Attempt at More Refined Definition of the Cultural Dimension of Syntality in Modern Nations," *American Sociological Review,* 17 (1952), 408–421; Raymond B. Cattell and Richard Gorsuch, "The Definition and Measurement of National Morale and Mortality," *The Journal of Social Psychology,* 67 (1965), 77–96.

[22] Brian J. L. Berry, "An Inductive Approach to the Regionalization of Economic Development," in Norton Ginsburg (ed.), *Essays on Geography and Economic Development* (Chicago: University of Chicago Press, 1960).

[23] Bruce M. Russett, *International Regions and*

FIG. 1.

Patterns[b] of Nation Attributes



≥75% ≥50% ≥25% } variation in at least one of the variables categorized under the type of attribute is accounted for by the pattern.

[a] These types categorize 230 variables. A category not shown included six measures of data error.

[b] Orthogonally rotated factors.

FIG. 2.

Correlated Patterns[d] of Nation Attributes

DON Patterns	Sum of Squared Loadings		Highest Loaded Indicators[c]
ECON. DEV.	63.4	1.05	Telephones per cap.
		1.02	Agricultural popula./population
		1.02[a]	Energy consump./popula.
SIZE (POWER)	36.9	1.03	Population×Energy prod.
		1.02	National income
		1.01	Gross national product
		1.00	Population
POLITICS	23.5	1.16	English titles transl./foreign titles transl.
		1.12	English titles transl./Russian and English titles transl.
		.90	Bloc membership
FOR. CONF.	12.2	.89	Threats
		.88	Accusations
		.78	Killed in foreign violence
DOM. CONF.	11.1	.80	Killed in domestic violence
		.75	General strikes
		.74	Riots
CATH. CULT.	10.9	.86	Roman Catholics/popula.
		−.83	Air distance from U.S.
		.79	Medicine NGO/NGO memberships
DENSITY	10.4	.95	Population/area
		.76	Arable land/area
		.67[b]	Road length/area

74.7% of total sum of squared loadings for 15 patterns.

[a] Literacy also has a loading of 1.02.
[b] R. R. length/area, also has a loading of .67.
[c] Suggested basic indicators shown in blocks.
[d] Primary pattern matrix.

the (intraclass)[24] correlation between the DON results and those of each of the studies cited.[25] In all cases of comparison in Table 1, we find the correlation quite high, suggesting that the DON patterns as a whole have also been delineated in other empirical studies.

International Integration (Chicago: Rand McNally, 1967).

[24] The intraclass correlation coefficient measures both profile and magnitude correlation, while the product moment measures only profile correlation. The intraclass is therefore a more stringent measure of reliability than the often employed product moment. See Section 12.3.3 of my *Applied Factor Analysis* (Evanston: Northwestern University Press, 1969) forthcoming.

[25] The technique of comparison employed was Ahmavaara's transformation analysis. See Yrjo Ahmavaara, *On the Unified Factor Theory of Mind* (Helsinki: Annales Akademiae Scientiarum Fennicae, 1957). The DON fifteen factors were first rotated to a least squares fit with those of another study. Then, the intraclass correlation was calculated between all the DON loadings and those of the other study.

TABLE 1. INTRACLASS CORRELATION BETWEEN DON CORRELATED PATTERNS AND OTHER STUDIES

Study			DON 15 Patterns
Russett:	8	patterns	.93
1960c		data	
	54	variables	
	82	nations	
Berry:	5	patterns	.96
1955c		data	
	43	variables	
	95	nations	
Cattel and Gorsuch (1965):	14	patterns	.90
1960c		data	
	51	variables	
	52	nations	
Cattell, *et al.* (1952):	12	patterns	.77
1837-1937		data	
	72	variables	
	40	nations	
Cattell (1949):	12	patterns	.78
1837-1937		data	
	72	variables	
	69	nations	

Are the individual patterns reliable? Figure 3 shows the (product moment)[26] correlation between the seven DON dimensions and those of the other studies. Patterns of the other studies having correlations less than .50 (less than twenty-five percent overlap) with the DON seven are not shown. How naming patterns is an imperfect art can be seen from the different labels applied in the various studies.[27]

The number of studies in which each pattern appears to the number in which it could appear is[28]

$$\text{economic development} = 5/5,$$
$$\text{size} = 5/5,$$
$$\text{politics} = 3/4,$$

As can be seen from Figure 3, the DON patterns are quite similar to those of Russett. In order to extend our view of the reliability of the DON patterns, we might note some additional comparisons that Russett has done[29] with studies of Robinson,[30] Berry,[31] and Gregg and Banks.[32] These are shown in Figure 4. In each case, an image of Russett's patterns is found in the other studies. Since the (intraclass) correlation between DON's results and Russett's is .93, those patterns of other studies similar to Russett's should be also similar to those of DON.

In sum, the comparisons in Table 1 and Figures 3 and 4 suggest that the DON patterns are reliable individually and in total and that variables selected to index these patterns *are indicators of relationships generally found in cross-national data.*

What about data error or bias distorting the empirical patterns? Random error should cause us little concern. Its effect would be to decrease the loadings without obscuring the outlines of the patterns. Moreover, the comparisons with the results of other studies should buttress our confidence in the patterns independence of random error. Systematic error—biased data—is another problem, however, since all the data used in the various studies are from much the same sources, error might bias *all* the studies in the same direction.

Several measures of systematic error were developed[33] and included in the 236 variable factor analysis. The correlation of these error measures with the patterns would then indicate which possible sources of error were related to what patterns. Figure 5 shows the results. The "censorship" and "embassies and legations" error measures were included to index possible bias in the conflict data. Embassies and legations in a nation measures the world interest in that nation and the likelihood of conflict news about that nation being published. As can be seen from Figure 5, the *economic development* and *politics* patterns are the only ones involving measures of systematic error.

[26] Correlations were computed after the DON fifteen factors were rotated to a least squares fit to those of the other study.

[27] I have relabelled, for clarity, Cattell and Gorsuch's *op. cit.* (1965), factors 3 and 14.

[28] If the variables involved in a pattern were not included in the analysis of the other study, then the pattern cannot be compared.

[29] Russett, *op. cit.* I wish to express my appreciation to Russett for communicating these comparisons to me before publication.

[30] John Robinson, "Multidimensional Analysis as a Comparative Framework for Poltical Systems," (Ann Arbor: University of Michigan, 1965, unpublished manuscript).

[31] Berry, *op. cit.*

[32] Phillip M. Gregg and Arthur S. Banks, "Dimensions of Political Systems: Factor Analysis of *A Cross-Polity Survey*," this REVIEW, 59 (1965), 602-614.

[33] R. J. Rummel, "Dimensions of Error in Cross-National Data," in Raoul Naroll and Ronald Cohen (eds.), *Handbook of Method in Cultural Anthropology*, forthcoming, 1969.

FIG. 3. Comparison of DON Correlated Patterns with those of Other Studies.

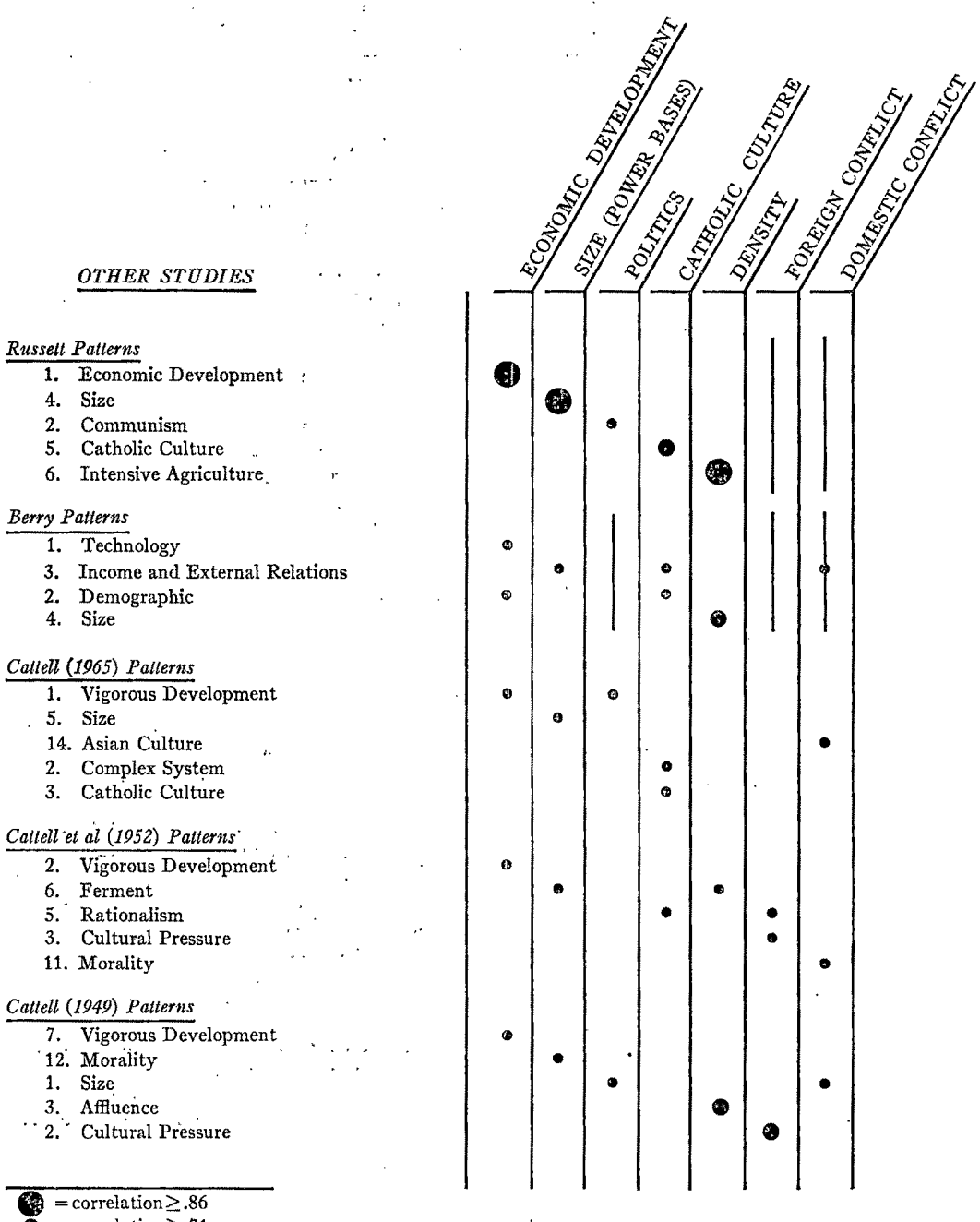
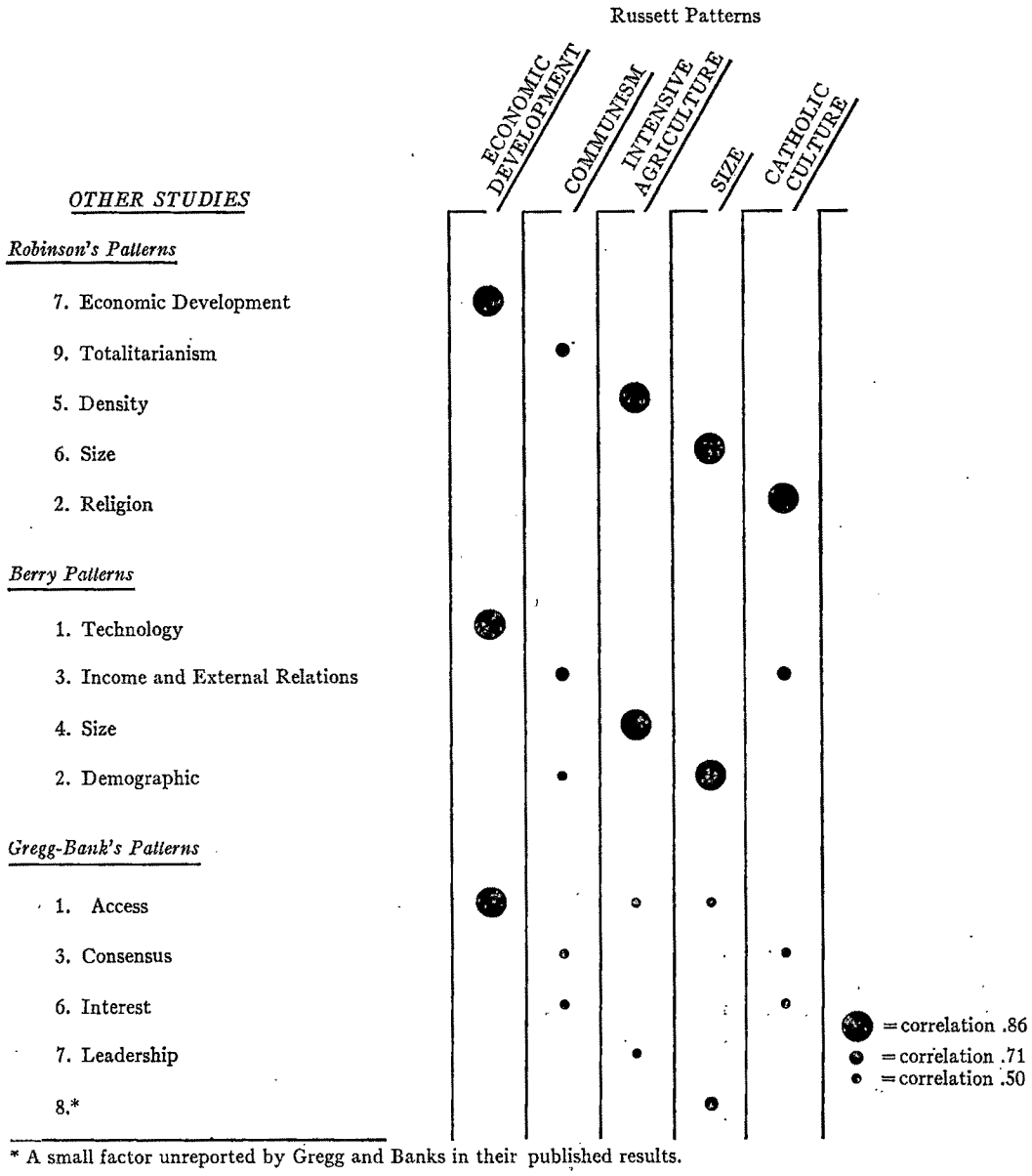


FIG. 4. Comparison of Russett's Patterns with those of Other Studies.

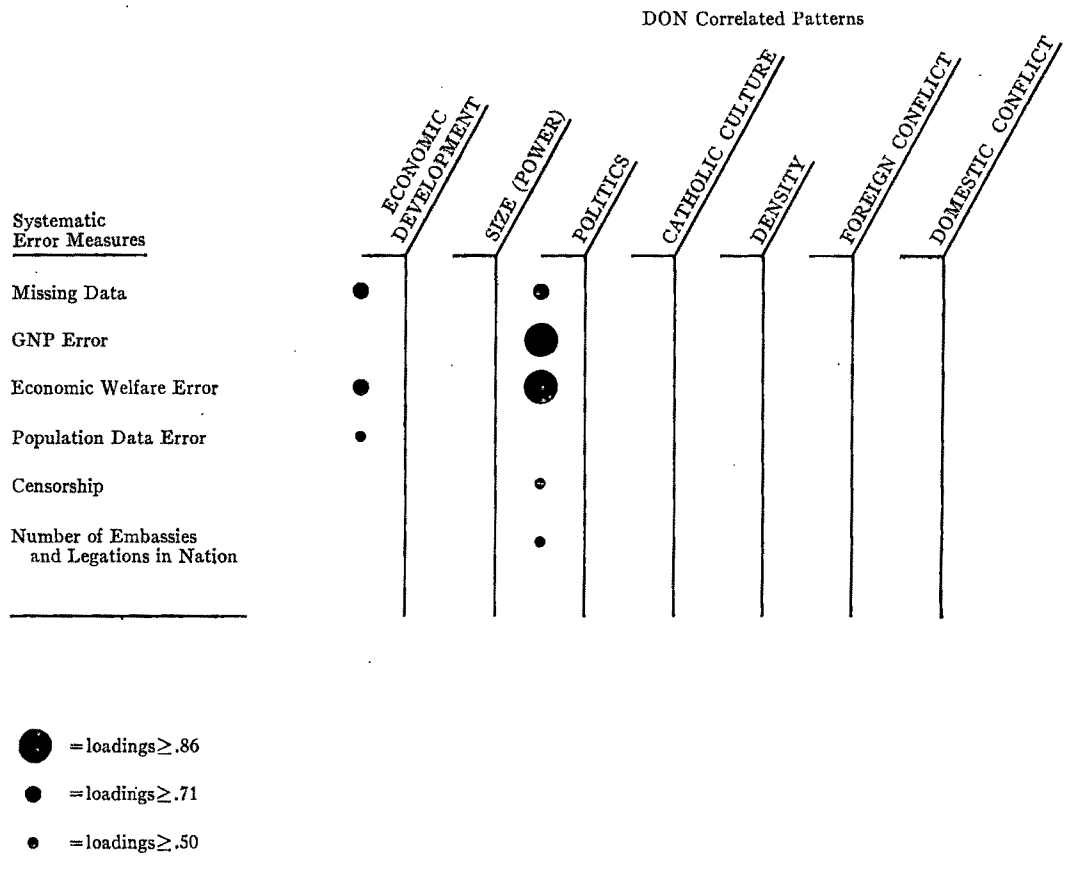


* A small factor unreported by Gregg and Banks in their published results.

A close look at the error measures related to the *economic development pattern*, however, would indicate that the attributes of nations reflected in this pattern are more a cause of this error than the other way around. For one thing, the variance in the variables involved in the pattern, such as the proportion of the population in agriculture, GNP per capita, telephones per capita, vehicles per capita, literacy, newspaper circulation per capita, and the ratio of proteins to calories, far exceeds the variance in possible systematic error. Nations should have nearly the same relative ranking on such measures with or without the error variance. Moreover, it makes good theoretical sense to argue that the amount of missing data and poor quality of economic and population data reflects low economic development—inadequate statistical training and investment in statistical services—rather than the concept of economic development reflecting poor data.

The relationships between the *politics* dimension and the error measures can be rationalized in the same way. Many of the variables defining

FIG. 5. Loadings of Systematic Error Measures.



politics have variance exceeding that of the error. Moreover, for some variables the data source is known to be unaffected by such possible error, as with bloc membership (dependent on military treaties), freedom of group opposition, or voting system measures. Moreover, it also is more plausible to argue that the politics pattern is the source of biasing error, rather than the other way around. Nonetheless, the error measure loadings of Figure 5 in conjunction with the general lack of high relaibility for the politics pattern in the comparisons of Figure 3 and 4 suggest that a politics pattern exists, but that its nature is not well delineated.

One additional finding can be related to the question of error. In Figure 5, the missing data measure is correlated with economic development and politics patterns. Will these patterns emerge, therefore, if we exclude the nations and variables with most missing data? The answer is yes. By eliminating variables and nations with missing data, the average of seventeen percent missing data per variable was reduced to an average of three percent for seventy-one nations and 153 variables. This latter data was then analyzed as was that for 236 variables. The comparison of the results is shown in Figure 6.[34] The (intraclass) correlation between both sets of results is .96 and each of the seven patterns is well defined.

In his factor analysis, Cattell[35] was also plagued with missing data. To check the invariance of the patterns he found, he eliminated nineteen nations and reanalyzed the resulting data on forty nations and seventy-two variables.[36] As in our case, the patterns came out largely the same. Missing data, then, do not have significant influence on our delineation of attribute patterns.

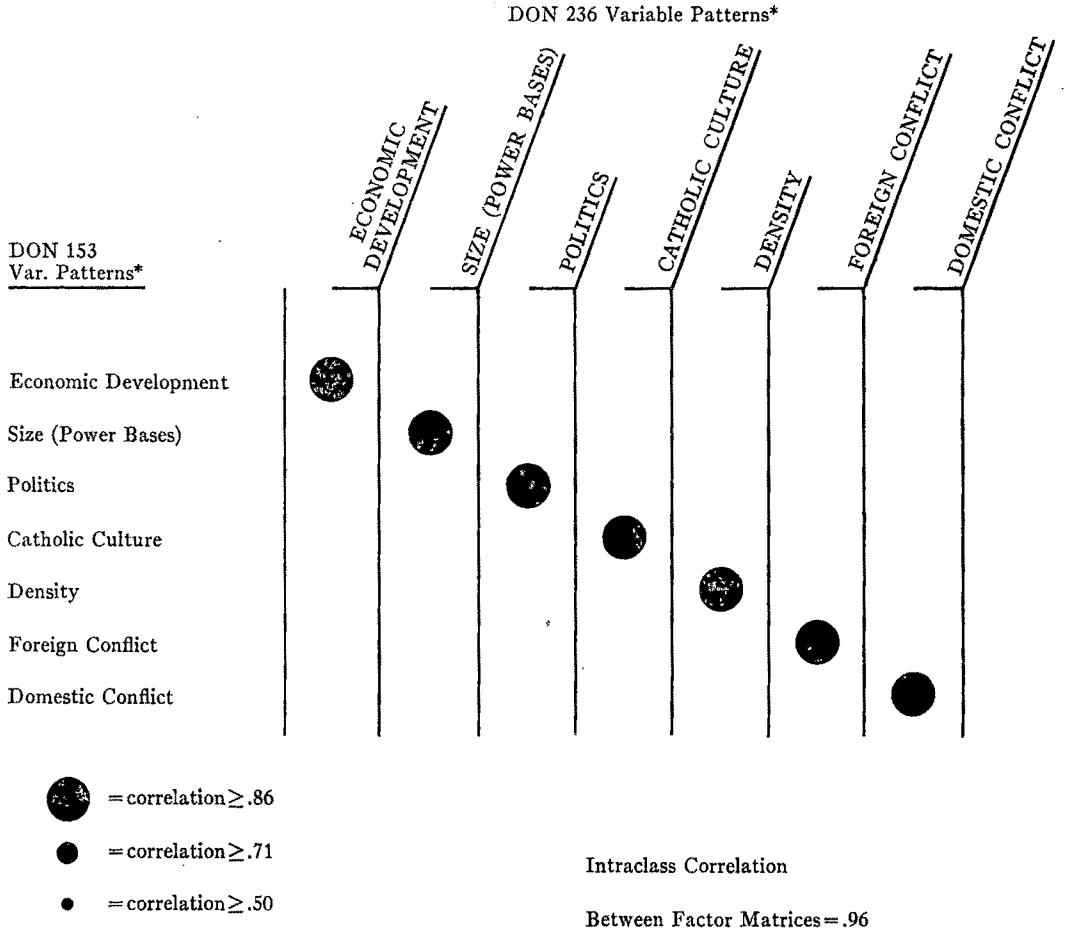
Finally, how correlated are the suggested attribute basic indicators? We certainly do not want highly correlated indicators, otherwise they can hardly reflect distinct patterns. The

[34] The technique of comparison was the same as in the previous comparisons.

[35] Cattell, *op. cit.* (1949).

[36] Cattell, Hartman, and Breul, *op. cit.*

FIG. 6. Comparison of 236 and 153 Variable Patterns.



* Oblique primary pattern matrices

ideal is a set of basic indicators, each virtually uncorrelated with the other. Table 2 presents the coefficient of determination between all basic indicators. The coefficient measures the *proportion* of variation they have in common. The highest coefficient is .12 and most coefficients are virtually zero.[37] *Each basic indicator thus measures a nearly statistically independent component of variance in nation attributes.*

TABLE 2. COEFFICIENT OF DETERMINATION* BETWEEN BASIC INDICATORS

Basic Indicators	1	2	3	4	5	6
1. Energy consump./ population		78	78	74	74	73
2. Population	.03		82	77	77	77
3. Bloc membership	.00	.00		77	77	77
4. Killed in foreign violence	.00	.05	.04		77	72
5. Killed in domestic violence	.00	.09	.03	.05		72
6. Roman Catholics/ population	.06	.02	.12	.03	.03	
7. Population/area	.01	.10	.02	.02	.00	.00

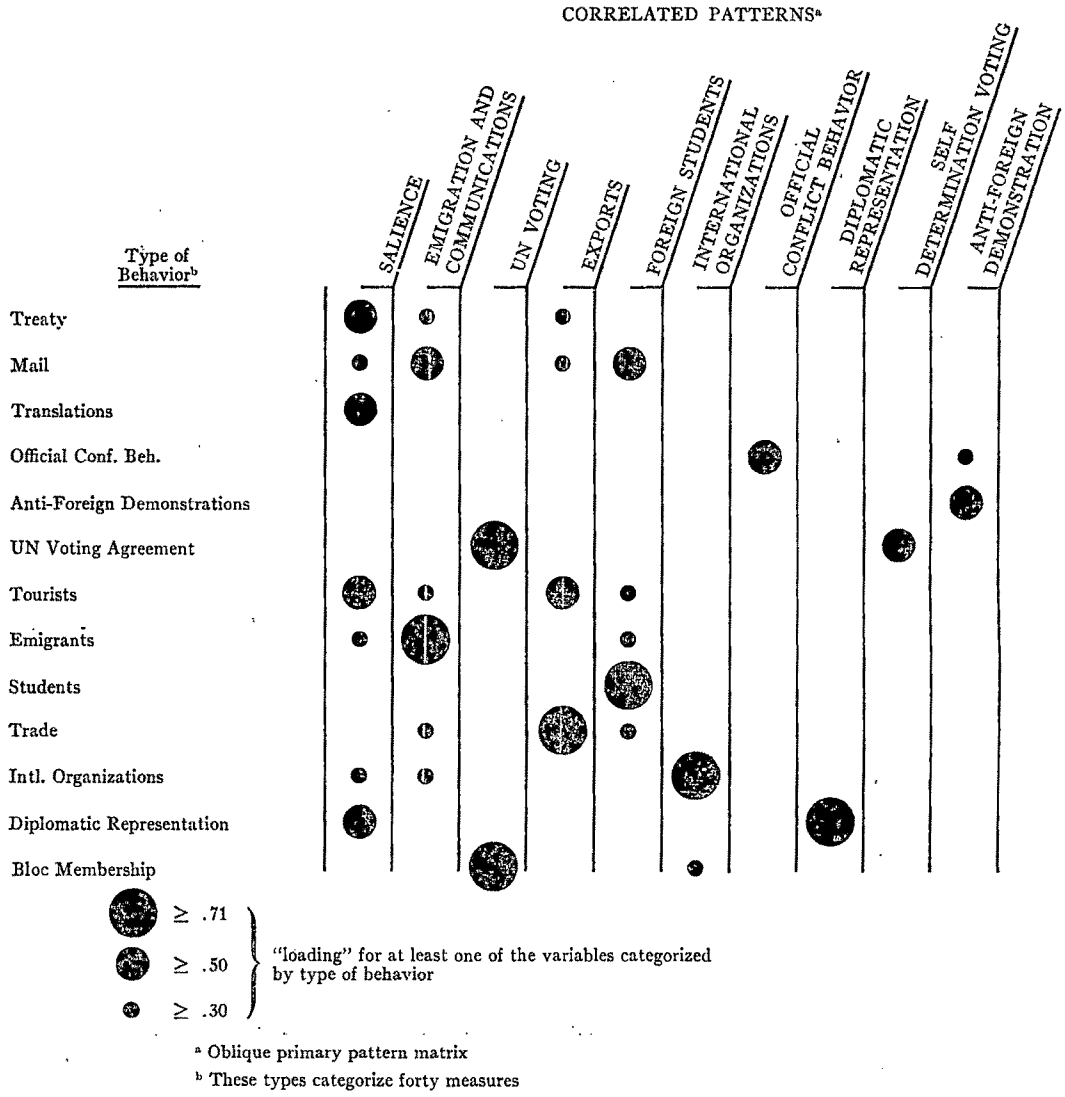
* This is the product moment squared and gives the proportion of variance in common between two basic measures. The upper triangular matrix gives the sample size for each coefficient. Data are for 1955.

III. BEHAVIORAL MEASURES

In order to define patterns and indicators of international behavior, an analysis different from that above is required. Rather than analyzing

[37] For the size pattern, a basic indicator might be population times energy consumption. It has a higher loading than population, but it also has a coefficient of determination of .37 with energy consumption per capita. For this reason population is a preferred basic indicator.

FIG. 7. Patterns of Nation Behavior.



[a] Oblique primary pattern matrix

[b] These types categorize forty measures

data for nations on *attributes*, data for dyads (nation A → B, B → C, etc.) on *behavior* variables are analyzed.

Figure 7 displays the results of analyzing forty behavior variables for 346 nation dyads on 1955 data. These results are a composite of four parallel analyses:

1) Raw data on a random sample of 164 dyads.
2) Transformed data[38] on the above sample.
3) Raw data on a selected sample of 182 dyads. Fourteen nations were selected to represent high, middle, and low scores on the attribute patterns of nations (Figure 1) and major cultural and regional groupings.[39] Then taking all possible dyads (A → B) for these nations produced the selected sample.
4) Transformed data on the above sample.

Each of the four sets of data were similarly

[38] Data were transformed to increase the normality of the distributions and to remove extreme values. This reduces the effect on the results of a few high behavior dyads, such as UK→USA and USA→UK.

[39] Russett, *op. cit.* Raymond B. Cattell, "The Principal Culture Patterns Discoverable in the Syntal Dimensions of Existing Nations," *The Journal of Social Psychology*, 32 (1950), 215–253.

FIG. 8.

Dyadic Analyses Research Design

Data	Sample: Random	Sample: Selected
Raw	164 nation dyads	182 nation dyads
Transformed	164 nation dyads	182 nation dyads

Intraclass Correlation

Between Results

	Raw Selected Sample
Raw Random Sample	.84
Transformed Random Sample	.87
Transformed Selected Sample	.78

analyzed.[40] The *raw selected* sample results were then selected as a base[41] to which were compared each of the patterns from the other samples.[42] Figure 8 displays the four samples, and the (intraclass) correlation between the raw selected correlated *patterns* and the others.

With this four-sample design specified, I can now give meaning to the composite "loadings" in Figure 7. The three sets of patterns found for the transformed selected, transformed random, and raw random samples were fitted to the raw selected sample patterns.[43] Then the "loadings" given in Figure 7 were computed as the geometric mean of the loadings of a variable on the corresponding patterns for all four samples. The "loadings" of Figure 7 therefore represent the most stable findings across the four samples. These composite "loadings" and (intraclass) correlation comparisons of Figure 8 suggest that we can rely on the raw selected sample for selecting a set of behavioral indicators, but with an eye towards the loadings of a variable in the other three samples.

Table 3 gives the correlated patterns of the raw selected sample. The suggested basic indicators have high loadings for all four sample results and the geometric means of these loadings are shown in Table 4.

Four of the selected basic indicators need some clarification. The weighted U.N. voting distance A↔B indicator is the Euclidean

[40] The same factor analysis and rotation procedures were used as for the attribute analysis. See footnotes 18 and 19.

[41] This sample has the most interesting dyads and raw data patterns. Moreover, indicators selected from the analysis of this sample will be simpler to interpret and employ.

[42] The same factor comparison method was used as employed for the comparisons of Figure 3.

[43] The patterns of each of the three samples were rotated to a least squares fit to those of the raw selected sample. This maximized the linear correspondence of the results and washed out irrelevant effects on the oblique rotations which were done independently for each sample.

TABLE 3. CORRELATED PATTERNS OF NATION BEHAVIOR

DON DYADIC PATTERNS (Raw Selected Sample)	Sum of Squared Loadings		Highest Loaded Measures[a]
Salience	5.3	.87	Translation A→B / A's total translations
		.84	Translations A→B
		.79	Tourists A→B
Emigration and Communication	4.9	.98	Emigrants A→B / A's total emigrants
		.84	Emigrants A→B / A's population
		.77	Mail A→B
U.N. Voting	4.2	.92	Weighted U.N. voting distance A↔B
		−.81	Common Bloc membership
		−.79	Common Bloc leadership
Foreign Students	3.8	.95	Students A→B / A's students to all
		.89	Students A→B
		.82	Mail A→B / A's mail to all
Exports	3.7	.93	Exports A→B / A's GNP
		.89	Tourists A→B / A's population
		.83	Exports A→B / A's exports
International Organization	2.9	.86	IGO A↔B / A's IGO[b] membership
		.79	NGO A↔B / A's NGO[c] membership
		.68	IGO A↔B
Official Conflict Behavior	1.9	.87	Military Violence factor score A→B
		.84	Negative communication factor score A→B
Diplomatic Representation	2.0	.95	Embassy or Legation A→B / A's total Embassies or Legations
		.84	Embassy or Legation A→B
Self Determination Voting	1.7	.88	U.N. voting distance on "Self Determination" pattern A↔B
		.70	U.N. voting distance A→B
Anti-Foreign Demonstration	1.3	−.71	Negative sanctions factor score A→B
		.65	Anti-foreign behavior factor score A→B

[a] Suggested basic measures shown in blocks.
[b] IGO: Intergovernmental international organizations.
[c] NGO: Nonintergovernmental international organizations.

distance[44] between nations on the uncorrelated pattern of U.N. voting in the Xth (1955) Sessions of the General Assembly. To determine these distances, Assembly committee and Plenary sessions first were separately analyzed to define the issue patterns of roll call voting. Then, those issues central to the committee and Plenary patterns were combined and reanalyzed. The resulting patterns describe the most general clustering of issues in the U.N. and were used to calculate the weighted U.N. voting distances between nations.[45] "Weighted" means that in

[44] The Euclidean distance, d_{AB}, between two nations, A and B, in the space of p orthogonal factors (uncorrelated patterns) $S_1, S_2, \ldots, S_l, \ldots, S_p$ is

$$d_{AB} = \left[\sum_{l=1}^{p} (S_{Bl} - S_{Al})^2\right]^{1/2}$$

where S_{Al} and S_{Bl} are the factor scores of nations A and B on the lth factor.

[45] The issue patterns were internationalism, admission of new members, self determination, se-

computing the distances each pattern was weighted by the variance in roll call votes for which it accounted.

The U.N. voting distance on a "self determination" pattern measure was derived similarly to the above. "Self determination" was one of the patterns of U.N. voting to emerge from the patterns of U.N. voting to emerge from the U.N. analysis. It involved issues concerning the competence of the U.N. to decide whether a Non-Self-Governing Territory has attained a full measure of self-government (A/3087), a proposed article to the International Covenant on Human Rights on the right of nations to self-determination (A/C.3/L.478/point 2), a proposal to set up a working committee to consider the previous issue (A/C.3/L.477/Rev. 1), and a proposal that would in effect postpone a decision on the previous issue (A/C.3/L.479/Rev. 1). Euclidean distances between nations on this pattern then made up the variable used in the dyadic behavior analysis.

The third and fourth indicators needing clarification are the military violence (factor) scores and anti-foreign demonstration (factor) scores. A previous analysis[46] of all dyads (340) having foreign conflict on any one of sixteen variables[47] determined four raw data patterns of conflict behavior: military violence, negative sanctions, negative communications, and anti-foreign behavior. Scores for dyads on these patterns were included in the dyadic behavior analysis. As noted, the military violence and anti-foreign demonstration scores turned out to be basic indicators. The best single indicator of the military violence scores is the number of discrete military actions or clashes A → B. For the anti-foreign behavior scores, the number of anti-foreign demonstrations is a good indicator.

Returning to the ten behavioral patterns of Figure 7 and Table 3, we might ask: How reliable are the behavioral patterns? Unfortunately, no previous analyses of dyads on such behavioral variables have been published. The four-sample analyses reported here give some idea of the stability of the patterns for different samples and data, but we need a number of independent dyadic analyses by different researchers before we can have the degree of confidence in the behavioral patterns that we can have in the seven attribute patterns of the previous section.

What about data error or bias distorting the behavioral patterns? Similar to the systematic error analysis of the seven attribute patterns (Figure 5), six measures of possible sources of bias were included in the raw selected sample analysis. The error measures are

1) Exports A → B minus imports B ← A. This measures the reliability of trade data, and possibly of other behavioral data as well.

2) Exports A → B imports B ← A/trade average (A → B) + (B ← A). This is a standardization of the above error measure by the amount of trade.

3) Embassies and legations in A and B. This is a measure of the world interest in a pair of nations, and thus the possibility that data, such as that on conflict, might be omitted from the sources.

4) Energy Consumption per capita for A and B. Energy Consumption per capita is the suggested basic indicator of economic development and we have found (Figure 5) that economic development is correlated with some possible sources of systematic error. Therefore, this error measure may tap the contribution to biasing error made by low economic development in either or both nations in a dyad.

mantic (determined largely by one roll call as to whether the word "man" should be replaced by "human being" in the covenant of human rights), feminine rights, petitioning (Ethiopia), and self-government (Togo).

[46] R. J. Rummel, "Some Attribute and Behavioral Patterns of Nations," *Journal of Peace Research*, 2 (1967), 196–206.

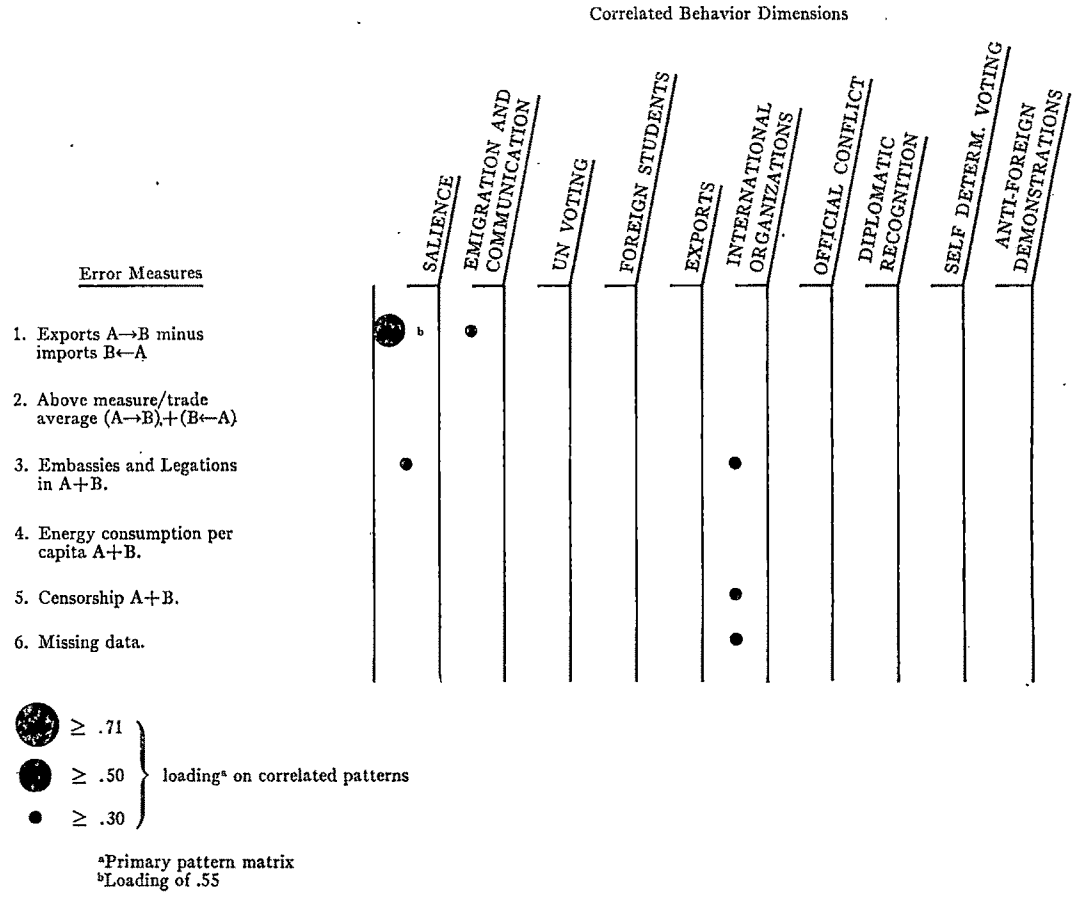
[47] They were violent acts, planned violent acts, incidences of violence, discrete military acts or clashes, days of violence, negative acts, diplomatic rebuffs, negative communications, written negative communications, oral negative communications, written or oral negative communications, unclassified negative communications, accusations, representations or protests, warnings, and anti-foreign demonstrations.

TABLE 4. GEOMETRIC MEANS OF BASIC INDICATORS ON FOUR SAMPLES

Basic	Geometric Mean of Loadings on Four Samples[a]
Tourists A→B	.65
Emigrants A→B/A's population	.72
Weighted U.N. voting distance A←→B	.84
Students A→B/A's students to all	.74
Exports A→B/A's GNP	.76
IGO A←→B/A's IGO membership	.79
Military Violence factor score A→B	.62
Embassy or Legation A→B/A's total Embassies and Legations	.74
U.N. voting distance on "Self Determination" pattern A←→B	.70
Anti-foreign behavior factor score A→B	.55

[a] Samples are raw selected, raw random, transformed selected, and transformed random. The geometric mean is computed for a basic indicator only on the four matching patterns for the four samples.

FIG. 9. Loadings of Systematic Error Measures.



5) Censorship A and B.
6) Missing data.

Figure 9 gives the relationship of the error measures to the ten behavioral patterns. The loadings are generally low, with the highest being .55. These overall results imply that the behavioral dimensions are largely statistically independent of systematic error *as tapped* by the error measures.

As with the attribute measures, we can ask: How correlated are the basic indicators of nation behavior? Table 5 shows the coefficient of determination between the basic measures. The highest coefficient is .10 and most coefficients are zero. This set of basic measures thus forms a nearly uncorrelated selection of indicators spanning the diverse behavior of nations and indexing the basic patterns involved.

IV. THE BASIC INDICATORS

The value of this set of major attribute and behavior concepts—patterns—and their basic indicators may not be fully appreciated, at first. Since these major concepts have been empirically delineated by factor analysis, the patterns they define span and embody variation on diverse national attributes and behavior.[48] A set of indicators of such patterns imply that *attributes or behavioral variables not in the set of basic indicators can be considered a (linear) combination of those in the set.* Therefore, with the set of basic indicators one should be able to predict or generate the data of nations on most of their other attributes or behavior.

Consider the total trade of a nation, for example. This is a nation's attribute of much interest in empirical and theoretical research. The variation of nations on this attribute is largely reflected, however, by their variation on the basic indicators, specifically energy consumption per capita and population. That is, the value of nation A on its total trade is[49]

trade A = .57 (A's energy consumption per capita) + .75 (A's population).

[48] In the terminology of linear algebra, the patterns (as factor dimensions) can be considered as bases of nation attribute and behavioral spaces.

[49] For all following examples, the data are assumed to be standardized.

TABLE 5. COEFFICIENT OF DETERMINATION BETWEEN BASIC INDICATORS*

Basic Indicators	1	2	3	4	5	6	7	8	9	10
1. Tourists A→B		111	90	108	123	123	123	123	90	123
2. Emigrants A→B/A's popul.	.02		100	111	131	131	131	131	100	131
3. Weighted U.N. voting dist.	.01	.01		100	132	132	132	132	132	132
4. Students A→B/A's stud.	.08	.08	.00		139	139	139	139	100	139
5. Exports A→B/A's GNP	.08	.07	.01	.01		182	182	182	132	182
6. IGO A←→B/A's IGO mem.	.02	.02	.02	.00	.00		182	182	132	182
7. Military violence A→B	.00	.00	.06	.00	.00	.00		182	132	182
8. Emb. or Leg. A–B/A's total	.00	.00	.00	.00	.01	.00	.00		132	182
9. U.N. voting on "Self Determ."	.00	.00	.10	.00	.00	.00	.00	.00		132
10. Anti-foreign dem. A→B	.00	.00	.03	.00	.00	.00	.02	.00	.00	

* See the footnote to Table 2. Data are for the raw selected sample.

This equation accounts for about ninety percent of the variation of nations in their trade.[50]

Another interesting attribute is the number of intergovernmental organizations (IGO) of which a nation is a member. The variation of nations on this attribute is more complex than that for trade. For nation A,

A's IGO membership = .57 (A's energy consumption per capita) + .51 (A's bloc membership) + .33 (A's population) — .33 (A's Roman Catholics/population).

This combination of four basic variables should account for about eighty percent of the variation in IGO memberships among nations.

Measurements of communication attributes play a large role in some current theoretical work. Most communication attributes, however, can be expressed as a linear combination of energy consumption per capita. For example, for nation A,

A's newspaper circulation per cap. = .88 (A's energy cons. per cap.),
A's newsprint consumption per cap. = .89 (A's energy cons. per cap.),
A's literacy rate = .89 (A's energy cons. per cap.),
A's telephones per cap. = .95 (A's energy cons. per cap.),
A's domestic mail per cap. = .86 (A's energy cons. per cap.),
A's radio receivers per cap. = .91 (A's energy cons. per cap.).

The smallest variation energy consumption per capita accounts for among these communication variables in about seventy-seven percent (newspaper circulation). The highest variance accounted for is approximately ninety percent (telephones per capita). Since only one basic indicator is involved, communication attributes are proportional to energy consumption per capita and this *basic variable may be used alone as an indicator of the internal communications of nations.*

I have so far exemplified the meaning of the attribute basic indicators. Similar examples can be given for the behavior indicators. Most behavior between two nations should be a (linear) combination of the basic behavior indicators.

Consider treaties, for example. The number of treaties between two nations, A and B, should be,

treaties A ↔ B = .61 (tourists A → B) + .55 (emigrants A → B/A's population),

and accounting for about sixty-seven percent of the variance in treaties.

Mail and exports between nations have become important measures in theories of international integration. Variance in both kinds of behavior are represented by the behavior basic measures. To wit,

Mail A → B = .52 (tourists A → B) + .78 (emigrants A → B / A's total emigrants),

Exports A → B = .62 (tourists A → B) + .36 (exports A → B / A's GNP)

[50] These and subsequent linear combinations (regressions) are derived from the orthogonal factor results of the 236 variable attribute and 40 variable (raw selected sample) behavioral analyses (Rummel, *Dimensions of Nations, op. cit.*). The values for the constants in the equations are the loadings of the dependent variable on the factor patterns indexed by the basic indicators.

$+ .40$ (students $A \rightarrow B$ / A's total students).

The variance accounted for is around eighty-eight percent for mail and about sixty-seven percent for exports.

One final example might be given. The private interaction between two nations, as opposed to public or official behavior, can be partly measured by their joint memberships in nongovernmental international organizations (NGO). The variation in co-memberships among pairs of nations is tapped by the basic behavior indicators,

NGO comemberships $A \leftrightarrow B$
$= .54$ (tourists $A \rightarrow B$)
$+ .34$ (emigrants $A \rightarrow B$ / A's emigrants)
$+ .53$ (intergovernmental organization comemberships $A \leftrightarrow B$ / A's total memberships),

for around sixty-nine percent of the variance in NGO co-memberships.

In summary, we can say that if Y is an attribute of nations, then for any nation i

$$Y_i = \alpha_1 X_{i1} + \alpha_2 X_{i2} + \ldots + \alpha_7 X_{i7},$$

where X is one of the seven basic attribute indicators. Similarly for internation behavior, if W is a behavior of nation i to nation j, then

$$W_{ij} = \alpha_1 Z_{ij1} + \alpha_2 Z_{ij2} + \ldots + \alpha_{10} Z_{ij10},$$

where Z is one of the basic behavior indicators.

I have focused so far on the ability of the basic indicators to account for the variance of nations on their attributes and behavior. There is another implication to the set of basic indicators at least as important. Since each basic indicator indexes a pattern of variation for nations, each basic measure indexes a functional unity—a cluster of phenomena that vary together.

Functional unities may represent causal influences within or between nations. Covariation of phenomena is a necessary but not sufficient requirement for their being a common cause. Therefore, we can consider the basic indicators as not only indexing major empirical concepts, but also we can view them as possible indicators of primary causal influences. This means that the basic indicators can be used to investigate the nature of such influences and represent them in causal analyses.

V. CONCLUSION

I stated in the beginning that by fastening major concepts to major patterns of variation in the empirical attributes and interactions of nations, this paper was meant to help refocus the analytic conceptual scheme employed in the study of international relations. A second and primary goal was to help define the best indicators of these concepts. These indicators have been described in the previous pages and there is no need to list them again. What has been accomplished in refocusing a conceptual scheme of international relations may be more subtle and require some elaboration here.

First, I have divided international relations onto two analytically distinct kinds of data: those about nation attributes and those about behavior between nations. Although hardly a new idea, what gives the distinction power is defining these two kinds of data by two matrices: one that is nation by attributes, and another is dyad ($A \rightarrow B$) by behavior, respectively. These matrices are then conceived of as attribute and behavior *spaces;* each space comprises, respectively, all the interrelationships among the attributes and behavior of nations.[51] I have then

[51] The reason for dividing international relations into these two spaces is to mathematically resolve the problem of linking the characteristics of nations to their international behavior, or to use more fashionable language, to mathematically couple national subsystems to the international system. The representation of national attributes as a mathematical (Euclidean) space enables us to define nations as points in this space. Then we can define a distance vector between any two nation-points on each coordinate, say economic development, of space. Such distance vectors for all the coordinants of the space will measure the differences and similarities of nations across all their attributes.

Now, in the space of behavior we can locate pairs of nations—dyads—as points according to the behavior of the actor nation to the object nation. If we draw a line from the origin of this space to each dyad and add an arrowhead at the point where the line makes contact with the dyad, we then have a vector representation of each dyad's location in behavior space. The basic linkage between attribute space (nation attributes) and behavior space (nation behavior) is made through the distance vector. The magnitude and direction of a dyad vector in behavior space is a mathematical function of the distance vectors between the two nations in attribute space. That is

$$\vec{\omega}_{A \rightarrow B} = \sum_{i=1}^{p} \alpha_i \vec{d}_{A-Bj,i},$$

where $\vec{\omega}_{A \rightarrow B}$ = the vector describing the location of the dyad, $A \rightarrow B$, in behavior space,

employed mathematics to locate the largest independent patterns of relationships in each space. Each pattern thus defined is an independent empirical concept maximally discriminating between attributes or between behavior. Economic development, size, and political orientation are three such major attribute patterns that have been found. Three such major patterns of behavior are U.N. voting, emigration and communications, and official conflict behavior.

Thus, empirical concepts, such as size and economic development, have been locked into a framework involving two data matrices, mathematical procedures for their analysis, and a theoretically fruitful mathematical representation of international relations. The refocusing which was my goal has thus involved picturing international relations through a wide angle mathematical lens that filtered out all but the distinct clusters of interrelated phenomena.

$\rightarrow$ α_i = parameters for a dyad, $A \rightarrow B$,
$d_{A-B,i}$ = distance vector between nation A and B in attribute space on coordinant (dimension) i.

Thus, international behavior is made a function of the similarities and differences between nations. The mathematical notions are fully integrated into one model, are operationalizable and the linkage is testable. The purpose of this paper, however, was not to present this linkage but rather a set of indicators (which locate each nation on the coordinants of these spaces). Describing this theory in detail would take us far afield from our original purpose. However, those wishing to read more on this theory, which I call a social field theory, are referred to my "A Field Theory of Social Action with Application to Conflict Within Nations," *General Systems Yearbook,* 10 (1965), 183–211; "A Social Field Theory of Foreign Conflict Behavior," *Peace Research Society: Papers, IV,* Cracow Conference, 1965, pp. 131–150; *Dimensions of Nations, op. cit.,* Chapter 16.

FOREIGN POLICY AS AN ISSUE AREA: A ROLL CALL ANALYSIS*

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I. INTRODUCTION

James N. Rosenau has elaborated the issue area typology developed by Theodore Lowi in an ambitious attempt to categorize motivations, role structures, and interaction sequences which distinguish "domestic" from "foreign" issues. Lowi classified domestic issues into three areas: distribution of resources, regulation of resources, and redistribution of resources, and he equated interaction sequences in these areas with the coalition (distributive), pluralist (regulatory), and elitist (redistributive) models of the political process. The theoretical underpinning of Lowi's typology was the assumption that stable expectations develop about appropriate patterns of political competition and conflict for issues in each of these three "arenas."[1]

The internal logic of Lowi's argument is as follows: (1) people's expectations concerning benefits to be derived from relating to others determine their choices of relationships; (2) governmental policies (outputs) determine expectations about questions of politics; (3) ergo, the type of policy at stake determines the patterning of any political relationship; a distinctive type of political relationship should characterize every major type of policy.[2] In these arenas of power, "each arena tends to develop its own characteristic political structure, political process, elites, and group relations."[3] Distributive policies are easily disaggregable into small units and can be apportioned among participants in relative isolation from one another. Regulatory policies, while specific in their impact, are not so infinitely disaggregable: "regulatory policies are distinguishable from distributive in that in the short run the regulatory decision involves a direct choice as to who will be indulged and who deprived."[4] Redistributive policies are similar to regulatory policies insofar as individual decisions are interrelated and apply to large categories of participants, but the impact of redistributive policies on these aggregates is much greater.[5]

Rosenau uses the Lowi formulations as a foundation for the development of hypotheses about expectations and interaction sequences characteristic of "domestic" as opposed to "foreign" issues.[6] The merit of Rosenau's ambition is that he attempts to partial out, theoretically, a distinction between these types of issue areas which is more than an exercise in analytical arbitrariness. His notion is that the common usage "foreign-domestic" dichotomy, if it is to be made fruitful for the development of hypotheses about the process of foreign policy-making, is best conceived as superimposed on Lowi's continuum of distributive-regulatory-redistributive types of policies. Or, it might be said that Rosenau conceives of the "domestic-foreign" continuum and the "distributive-regulatory-redistributive" continuum as intersecting planes, each plane subsuming characteristic behavior patterns dependent on expectations about: (1) the amount of available resources for whose allocation participants are competing; (2) the degree to which allocation of resources to one group or some groups necessarily entails deprivation of other groups.

On this basis, Rosenau suggests that the *way in which issues are processed* is determined by the extent to which processing of the issue involves perceived allocation of finite, limited societal resources. That is, if we take the subset of issues which, on the basis of subject-heading alone, we might intuitively classify as "foreign" (those which, following Samuel P. Huntington's distinction, involve to some extent allocations of values and resources in the international system

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[1] Theodore Lowi, "American Business, Public Policy, Case Studies, and Political Theory," *World Politics*, 16 (July, 1964), 677–715.

[2] *Ibid.*, p. 688.

[3] *Ibid.*, p. 690.

[4] *Ibid.*, p. 691.

[5] *Ibid.*, p. 691.

[6] James N. Rosenau, ed., *Domestic Sources of Foreign Policy* (New York: Free Press, 1967); Chapter I, Rosenau, "Foreign Policy as an Issue Area."

as well as in domestic politics),[7] some "foreign" policy issues might well be perceived as more redistributive than others, if they involve significant reallocations of finite national resources among fairly large national political groups.

It would seem, though, that not all actors in a given national political system would perceive the same substantive issues as falling within the same process typology categories of Lowi. It is also possible that important aggregates of actors might perceive many issues of "domestic" or "foreign" nature to fall in the same area of the Lowi distributive-redistributive continuum. For example, groups might perceive that the intersection of planes mentioned earlier occurs in such a fashion that all "domestic" issues fall into the "redistributive" sector of the Lowi continuum, regardless of subject matter (agriculture versus welfare, etc.). As an illustration, Lowi argues persuasively that issues relevant to the tariff were at one time perceived by major participants as falling on a "distributive" dimension, but later most participants exhibited behavior and perceptions which indicated that they ordered tariff questions within the "regulatory" dimension.

It might be useful to ascertain whether, at the aggregate level, significant groups of actors seem to view "foreign policy" issues much more frequently and consistently as distributive, regulatory, or redistributive, insofar as those categories embrace a continuum of perceptions from positive sum to zero sum. While the Bauer, Pool, and Dexter analysis in *American Business and Public Policy*[8] provides interesting data and represents one way to approach this question, the book is a case study of a single issue and generalizations beyond that case might require different lines of analysis. One such line of analysis constitutes the approach taken in this paper.

II. APPROACH AND METHODS

The United States Senate, 87th Congress, First and Second Sessions, constituted the population under study. A set of 115 roll calls on foreign policy issues in that body was subjected to techniques of cluster and factor analysis, the object of which was the inductive specification of content dimensions along which these roll call votes might cluster. Review of most of the literature in which techniques of cluster, scale and factor analysis are employed reveals that their use in the past has been designed to satisfy somewhat different purposes. Often the interest of the analyst has been in categorizing and explaining the behavior of different groups in legislative bodies and the degrees of cohesion or disagreement found to characterize these groups.[9]

As Bruce M. Russett and others have pointed out, factor analysis is basically a data reduction technique; it pivots on the assumption that a group of variables highly correlated among themselves, but which exhibit relatively low correlations with other variables, point to a principal underlying dimension, or factor.[10] Factor analysis

[7] Samuel P. Huntington, *The Common Defense* (New York: Columbia University Press, 1960), pp. 1–10.

[8] Raymond Bauer, Ithiel de Sola Pool, and Lewis A. Dexter, *American Business and Public Policy: The Politics of Foreign Trade* (New York: Atherton Press, 1963).

[9] Summaries of the contributions of these sources may be found in Duncan MacRae, Jr., "A Method for Identifying Issues and Factions from Legislative Votes," this REVIEW, 59 (December 1965), 909–926; Bruce M. Russett, "Discovering Voting Groups in the United Nations," *ibid.*, 60 (June, 1966), 327–339; and Hayward R. Alker, Jr., "Dimensions of Conflict in the General Assembly," *ibid.*, 58 (September, 1964), 642–657. See especially, in addition to the above, Lee F. Anderson *et. al.*, *Legislative Roll-Call Analysis* (Evanston: Northwestern University Press, 1966); John G. Grumm, "A Factor Analysis of Legislative Behavior," *Midwest Journal of Political Science*, 7 (November, 1963), 336–356; L. N. Rieselbach, "The Demography of the Congressional Vote on Foreign Aid, 1939–1958," this REVIEW, 58 (September, 1964), 577–588; and Aage R. Clausen, *Policy Dimensions of Congressional Roll Calls* (unpublished doctoral dissertation, University of Michigan, 1964). Only results of factor analysis will be reported here. Comparison of results using factor analysis and cluster analysis is proceeding with the assistance of Aage R. Clausen and Richard B. Cheney.

[10] Alternative solutions of the correlation matrix according to the factor analytic model are possible. The choice of a principal components solution with orthogonal factors was made to facilitate interpretation, but the disadvantages of such a choice are noted by MacRae, *op. cit.* The principal components method of factor analysis is derived in Harry Harmon, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960). Orthogonal factors are selected by Russett, *op. cit.*, and Gerald Marwell, "Party, Region and the Dimensions of Conflict in the House of Representatives, 1949–1954," this REVIEW, 61 (June, 1967), 380–399. MacRae discusses Marwell's approach and findings in "Partisanship and Issues in Congressional Voting," Paper delivered at the 1968 Annual Meeting of the

LISTING OF VARIABLES

V1 = Nomination of director, Export-Import Bank
V2 = Nomination of director, Export-Import Bank
V3 = Convention Creating Organization for Economic Cooperation and Development
V4 = Eliminate payments to Commodity Credit Corporation for sale of agricultural commodities for foreign currencies
V5 = Deny quota bonus on sugar to Dominican Republic as result of cutback on Cuba
V6 = Require Secretary of Labor to recommend quotas, tariffs on imports from low wage countries
V7 = Confirmation of ambassador to Iran
V8 = Amend Battle Act, bar aid to Communist states in debt to United Nations
V9 = Amend Battle Act, give President authority to give aid to countries other than USSR and Communist Far East
V10 = Inter-American Social and Economic Cooperation Program; Chilean Reconstruction
V11 = Recommit Educational and Cultural Exchange Act to committee
V12 = Members of Commission on International Education and Cultural Affairs be appointed on nonpartisan basis
V13 = Eliminate travel expenses for dependents of participants in cultural exchange
V14 = Finance foreign students in cultural exchange at U.S. schools only
V15 = Prohibit President from making agreements with international organizations to sponsor exchange programs
V16 = Loyalty questions and security risks, in cultural exchange programs, must be investigated by FBI
V17 = Extension of Alien Orphan Act
V18 = Extension of Alien Orphan Act
V19 = Cut funds for purchase of radiological defense equipment by the Office of Civil Defense Mobilization
V20 = Foreign aid. Require annual appropriation for Development Loan Fund
V21 = Foreign aid. Congress must approve loans over $5 million and President must submit such loan plans to Congress 30 days in advance
V22 = Foreign aid. Congress must approve DLF budget annually, and its approval required on loans over $5 million.
V23 = Reduce military assistance for FY 1962, 1963
V24 = Foreign aid. Reduce borrowing authorization for FY 1962–66
V25 = Foreign aid. Limit U.S. contribution to UN Congo operations to 40 per cent of total cost
V26 = Foreign aid. Set ceiling on development loan fund interest rate
V27 = Foreign aid. Reduce DLF borrowing authorization for FY 1963–66
V28 = Foreign aid. Reduce military assistance for FY 1962–1963
V29 = Foreign aid. Require that equipment or property not purchased in recipient country be purchased in U.S.
V30 = Foreign aid. End military grants to Europe, except for national emergency
V31 = Foreign aid. Reduce DLF borrowing authorization from five to three years
V32 = Foreign aid. Earmark 30 per cent of development loan funds for private enterprise
V33 = Foreign aid. Bar aid to any Communist-controlled countries
V34 = Foreign aid. Require grants over $5 million to be approved by Congress
V35 = Foreign aid. Bar aid to countries exporting arms and strategic materials to Soviet-controlled nations
V36 = Foreign aid. Deny DLF loans or grants to nations behind in UN assessments
V37 = Foreign aid. Control interest rates required by recipient country reloaning development loans
V38 = Foreign aid. Establish ceiling on interest rates for relending of DLF funds
V39 = Passage of Foreign Assistance Act
V40 = Peace Corps Act. Reduce authorization
V41 = Recommit nomination of Gen. Williams for promotion of Brigadier General in U.S. Army Reserves
V42 = Nomination of Gen. Williams
V43 = Increase funds for USIA informational media guarantee program
V44 = Conference report, FY 1961 Foreign Assistance Act
V45 = Establishment of Arms Control and Disarmament Agency. Amend to eliminate agency authority to conduct, support, or coordinate studies on disarmament
V46 = Establish Arms Control and Disarmament Agency. Send bill back to Armed Services Committee

LISTING OF VARIABLES—(*Continued*)

V47 = Cut military aid 10 per cent
V48 = Passage of Foreign Assistance Appropriation Act for FY 1962
V49 = Confirm McCone as CIA head
V50 = Withhold U.S. aid from UN members in arrears
V51 = Prohibit U.S. participation in UN military operations without Congressional approval, and relieve UN from repayment to the U.S. of debts incurred in Congo
V52 = U.S. can loan to UN, above $25 million, only amounts matched by loans of other UN members
V53 = Limit U.S. loans to UN
V54 = Authorize President to make three-year, $100 million loan to UN at interest rate U.S. treasury paid on its own borrowing
V55 = President authorized to loan up to $100 million to UN
V56 = Textiles and farm products, imports
V57 = Textiles and farm products, imports
V58 = Textiles and farm products, imports
V59 = Foreign Assistant Act of 1962
V60 = Foreign Assistance Act of 1962. Amend to permit President to sell or give surplus food to Communist countries such as Yugoslavia and Poland
V61 = Bar aid to any nation which exported to USSR, or any nation dominated by USSR, any war implements or strategic articles
V62 = Passage of Foreign Assistance Act of 1962
V63 = Department of Defense Appropriations. Provide funds for development of RS-70
V64 = Department of Defense Appropriations. Limit funds for RS-70
V65 = Sugar Act Amendments of 1962
V66 = Establish space program manpower commission
V67 = Nomination of ambassador to Ireland
V68 = Conference Report, Foreign Assistance Act of 1962
V69 = State Dept., rather than Comsat, should supervise or conduct relevant foreign agreements
V70 = Require President to make arrangements for determining UN role in satellite system of U.S.

proceeds by establishing product-moment correlations of every variable in the analysis with every other variable, and seeks to establish whether large proportions of those variables which are highly inter-correlated exhibit those relationships due to high common "loadings" on, or correlations with, a few significant dimensions or "factors."[11]

The data, as mentioned, consisted of 115 roll calls in the 87th Congress, Senate, which seemed to have any foreign and defense policy component. Initial measures of association among items were obtained by computing a Yule's Q correlation of the vote distribution on each roll call with every other roll call. After these data were obtained, elimination of those roll calls which approached unanimity (defined here as items on which the distribution was more one sided that 90–10 per cent) was facilitated. Elimination of such items from the analysis was necessary because the product-moment correlation coefficient, on which this factor analysis depends, is distorted by a voting distribution more lopsided than 90–10.[12] Fourteen roll calls were dropped from the analysis for this reason. Inspection of the matrix of Q-coefficients among the remaining items led to elimination from the analysis of those items which had the least number of members present and voting. Thus reduc-

American Political Science Association, Washington, D.C., Sept. 2–7. See also Jeanne E. Gullahorn, "Multivariate Approaches in Survey Data Processing: Comparisons of Factor, Cluster, and Guttman Analyses and of Multiple Regression and Canonical Correlation Methods," in *Multivariate Behavioral Research Monographs*, No. 67–1, 1967, and D. M. Wilkins, *Factor Analysis and Multiple Scalogram Analysis: A Logical and Empirical Comparison* (unpublished doctoral dissertation, Michigan State University, 1962). For an interesting argument in favor of the use of "elementary" factor analysis, as opposed to standard Q and R analysis, see Ernest A. Chaples, Jr., *The Voting Behavior of United States Senators for Four Selected Issues, 1953–1964* (unpublished doctoral dissertation, University of Kentucky, 1967).

[11] For an interesting application of factor analysis to a problem involving relatively few variables, which makes clear some of the assumptions and problems associated with use of this technique, see Joseph A. Kahl and James A. Davis, "A Comparison of Indexes of Socio-Economic Status," *American Sociological Review*, 20 (June, 1955), 317–325.

[12] On this point see Russett, *op. cit.*, p. 329.

TABLE 1. LOADINGS, RAW FACTOR MATRIX

Variable	Factor 1	Factor 2	Factor 3
1	.03	.40	.52
2	.04	−.41	−.46
3	.49	.39	−.02
4	−.60	.69	−.09
5	.10	.02	−.41
6	−.75	−.01	−.04
7	.57	−.36	.06
8	−.72	.08	.03
9	.75	.22	.03
10	.68	−.25	−.14
11	−.73	.18	−.00
12	.80	−.30	.07
13	−.81	.16	−.05
14	−.82	.20	−.12
15	−.84	−.12	.01
16	−.62	.18	−.19
17	.33	−.62	−.19
18	.20	.67	.10
19	−.34	.25	.47
20	−.85	−.18	−.07
21	.72	.24	−.20
22	.69	−.05	−.16
23	−.42	−.47	.17
24	−.83	−.10	−.04
25	−.81	−.27	−.00
26	.70	−.40	−.20
27	−.81	−.03	.01
28	−.46	−.20	.27
29	−.84	−.00	−.02
30	−.07	−.42	.54
31	−.88	.03	−.10
32	−.80	−.17	−.01
33	.44	.44	.00
34	−.81	.29	−.09
35	−.83	.00	−.01
36	−.78	.08	−.00
37	.71	−.16	−.23
38	−.33	.14	.14
39	.71	.53	−.05
40	−.80	.23	−.08
41	−.55	.61	−.01
42	.55	−.61	.01
43	.26	.49	.20
44	.70	.53	−.06
45	−.74	−.05	−.20
46	−.74	−.01	−.12
47	−.45	−.33	.29
48	.63	.55	−.04
49	−.15	−.08	−.59
50	−.56	−.23	.14
51	−.71	−.22	.01
52	−.73	−.15	.10
53	−.82	−.08	−.00
54	−.64	.26	.00
55	.73	.31	−.05
56	−.70	.44	−.11
57	.67	−.44	.10
58	.56	−.52	.03
59	−.48	−.33	.09
60	.59	.41	−.07
61	−.85	.05	.01
62	.66	.55	−.09
63	.15	−.06	.47
64	−.11	.14	.30
65	.10	−.22	−.46
66	.27	.10	.56
67	−.62	.66	−.08
68	.68	.40	−.23
69	.33	.05	.67
70	−.32	−.03	−.55

TABLE 1—(*Continued*)

tion of the number of roll call items to the maximum number permitted by the factor analysis program was accomplished. Each legislator was coded on every roll call as either affirmative, abstaining, or negative, with affirmative and negative as upper and lower boundaries and abstention as intermediate.

One unfortunate outcome is that roll calls with a more lopsided vote distribution (in terms of yeas and nays) than 90–10 per cent may be of substantive interest in their own right. Here, there is less choice for the analyst methodologically, for one simply cannot include such items in a factor analysis, based upon a matrix of product-moment correlations, without danger of distortion. But the result is that certain items eliminated by the criterion of "too close to unanimity" might be of interest to the theorist, such as votes on military appropriations for the Defense Department. The same can be said for roll calls eliminated according to the yardstick of least number of legislators present and voting.

All of this reminds us of an important consideration relevant to interpretation of all these results, and similar roll call analyses: roll calls are only partial, and very partial, subsets of data relevant to the issue of Congressional perceptions and orientations on foreign policy issues. They do not capture the bargaining in committees, negotiations on the floor, or powers of persuasion and definition exercised by the executive. They are poor ersatz for extensive interview data, in some cases. Thus any analysis of roll call data is likely to generate inferences which must remain close to the vest of the researcher. Nonetheless, specification of dimensions in roll call data may suggest aggregate level relationships which assist us in framing

TABLE 2. LOADINGS, ROTATED FACTOR MATRIX

Variable	Factor 1	Factor 2	Factor 3
1	.11	.13	−.01
2	−.19	−.07	−.00
3	−.06	.71	−.13
4	.82	.09	.32
5	−.02	.11	−.03
6	.43	−.43	.46
7	−.64	.16	−.29
8	.45	−.30	.41
9	−.20	.43	−.65
10	−.46	.18	−.40
11	.69	−.38	.17
12	−.66	.17	−.50
13	.42	−.27	.70
14	.47	−.20	.71
15	.36	−.60	.55
16	.36	−.12	.54
17	−.61	−.06	−.12
18	.49	.45	−.30
19	.31	.00	−.00
20	.33	−.50	.54
21	−.20	.51	−.37
22	−.45	.32	−.26
23	−.08	−.34	.22
24	.33	−.41	.63
25	.21	−.48	.57
26	−.55	.05	−.45
27	.30	−.31	.71
28	.05	−.16	.35
29	.54	−.51	.34
30	−.23	−.44	−.06
31	.42	−.40	.65
32	.33	−.50	.38
33	.10	.48	−.39
34	.67	−.23	.40
35	.32	−.33	.72
36	.54	−.36	.29
37	−.36	.23	−.52
38	.13	−.05	.25
39	−.11	.87	−.22
40	.55	−.12	.56
41	.78	.05	.25
42	−.78	−.05	−.25
43	.10	.32	−.14
44	−.10	.86	−.23
45	.27	−.25	.68
46	.34	−.21	.55
47	.10	−.47	.06
48	−.05	.89	−.16
49	.07	−.07	.04
50	.07	−.36	.20
51	.27	−.52	.19
52	.29	−.42	.30
53	.44	−.46	.30
54	.67	−.25	.14
55	−.20	.61	−.25
56	.87	−.16	.18
57	−.86	.15	−.16
58	−.81	.07	−.04
59	.03	−.43	.22
60	.01	.59	−.36
61	.45	−.43	.53
62	−.04	.83	−.22
63	−.18	.09	.04
64	.12	.00	.03
65	−.15	.00	.03
66	−.07	.05	−.17
67	.85	.03	.24
68	−.14	.76	−.20
69	−.11	.12	−.20
70	.12	−.09	.15

more specific questions, or which may be of intrinsic interest.

In Table 1, the factor "loadings" of every variable (roll call) on the factors in the raw factor matrix are presented. The program included a normalized varimax rotation of the original factor matrix. Russett has described concisely the purposes of such rotation:

> 'Rotating' the original factors to 'simple structures' maximizes the number of both very high and very low loadings, thus making interpretation easier.[13]

Thirteen factors corresponding to eigen values of the correlation matrix which were greater than one were extracted and rotated. Three of these factors seemed important for substantive interpretation: factor 1 accounted for 18 per cent of total variance, factor 2 for 16 per cent, and factor 3 for 14 per cent. Summary of some of the more important results follows in tabular form.

Substantive interpretations of dimensions resulting from factor analyses can be controversial, especially when the matrix of correlations grows to the size used in this study. Some of this difficulty might be avoided if we could determine, for each observation, both (1) the factor on which it recorded its highest relative loading, and (2) its position within a rank ordering of observations which loaded highest on each factor. Table 3 reports these rankings for loadings of variables in the rotated factor matrix, for all those variables with loadings greater than one-half.[14]

[13] Russett, *op. cit.*, p. 331.

[14] This criterion is incorporated into the program

TABLE 3. RANKS OF LOADINGS FOR THE ROTATED FACTOR MATRIX

Factor 1

1. Textiles and farm products, imports
2. Textiles and farm products, imports
3. Nominate ambassador to Ireland
4. Eliminate payments to Commodity Credit Corporation for sale of agricultural commodities for foreign currencies
5. Textiles and farm products, imports
6. Recommit nomination of Gen. Williams for promotion in U.S. Army Reserves
7. Nomination of Gen. Williams
8. Education and Cultural Exchange Act
9. Authorize President to make loan to UN
10. Foreign aid. Require grants over $5 million to be approved by Congress
11. Members of Commission on International Education and Cultural Affairs be appointed on non-partisan basis
12. Confirm ambassador to Iran
13. Extension of Alien Orphan Act
14. Reduce authorization for Peace Corps
15. Set ceiling on Development Loan Fund interest rate
16. Foreign aid. Require that equipment or property not purchased in recipient country be purchased in U.S.
17. Foreign aid. Deny DLF loans or grants to nations behind in UN assessments

Factor 2

1. Passage of Foreign Assistance Appropriation Act for FY 1962
2. Passage of Foreign Assistance Act of 1961
3. Conference Report, FY 1961 Foreign Assistance Act
4. Passage of Foreign Assistance Act of 1962
5. Conference Report, Foreign Assistance Act of 1962
6. Convention creating Organization for Economic Cooperation and Development
7. President authorized to loan $100 million to UN
8. Prohibit President to make agreements with international organizations to sponsor cultural exchange programs
9. Foreign Assistance Act of 1962. Amend to permit President to sell or give surplus food to Communist countries such as Yugoslavia or Poland
10. Prohibit U.S. participation in UN military operations without Congressional approval
11. Foreign aid. Congress must approve loans over $5 million and President must submit such loan plans to Congress 30 days in advance
12. Foreign aid. Require that equipment or property not purchased in recipient country be purchased in U.S.
13. Foreign aid. Require annual appropriation for Development Loan Fund
14. Foreign aid. Earmark 30 per cent of development loan funds for private enterprise

Factor 3

1. Foreign aid. Bar aid to countries exporting arms and strategic materials to Soviet-controlled nations
2. Foreign aid. Reduce Development Loan Fund borrowing authorization for FY 1963–66
3. Finance foreign students in cultural exchange at U.S. schools only
4. Eliminate travel expenses for dependents of participants in cultural exchange
5. Establishment of Arms Control and Disarmament Agency. Amend to limit agency authority
6. Foreign aid. Reduce DLF borrowing authorization from five to three years
7. Amend Battle Act, give President authority to give aid to countries other than USSR and Communist Far East
8. Foreign aid. Reduce borrowing authorization for FY 1962–66
9. Foreign aid. Limit U.S. contribution to UN Congo operations to 40 per cent of total cost
10. Inter-American Social and Economic Cooperation
11. Establish Arms Control and Disarmament Agency. Send bill back to Armed Services Committee

TABLE 3—(*Continued*)

12. Prohibit President from making agreements with international organizations to sponsor cultural exchange programs
13. Foreign aid. Require annual appropriation for Development Loan Fund
14. Loyalty questions and security risks, in cultural exchange programs, must be investigated by FBI
15. Bar aid to any nation which exported to USSR, or any nation dominated by USSR, any war implements or strategic articles
16. Foreign aid. Control interest rates required by recipient country reloaning development loans
17. Membership of Commission on International Education and Cultural Affairs

Seventeen variables (roll calls) met this criterion for factor 1. Generally, they represent attempts by senators, through amendments to a variety of substantive bills, to reduce aid appropriations and restrict Presidential prerogatives in dispensing with funds to finance programs of agencies which operate overseas. It is interesting to observe that this factor cuts across a number of substantive measures, from the Peace Corps, to Development Loan Fund, to textile and farm imports, to Foreign Assistance Act, to Educational and Cultural Exchange Act, and so on. Loadings on this factor range from a high of .87 (textiles and farm product imports) to a low of .54 (deny Development Loan Fund aid to nations defaulting UN assessments), among these seventeen items.

On factor 2, fourteen items registered loadings meeting the one-half criterion. It is more difficult to discern a consistent pattern in this factor other than one very obvious tendency: the factor picked up items representing final passage and approval of a variety of substantive measures, including the Foreign Assistance Act of 1961, the Convention creating the Organization for Economic Cooperation and Development, the Foreign Assistance Act of 1962, U.S. aid to the United Nations, and so on. Initial suspicions were that this merely represented a "general acquiescence" phenomenon: the battles over amendment and committee squabbles having been concluded by the time the final vote is taken, senators are in widespread agreement to push a bill to passage. But, recall that all roll calls which approached too closely to unanimity were eliminated from the analysis. Considering this, it is more likely that this factor might represent the relatively stable alignments, associated with positions favoring or opposed to additional foreign aid, as they persisted through to the final "approve or disapprove" voting on the measures in their entirety.

Factor 3 contains echoes from factor 1, but seems more specifically referent to foreign aid per se. The high rank ordering of the amendment to limit the authority of the Arms Control and Disarmament Agency, for example, is redolent of efforts to limit agency prerogatives which marked factor 1.

III. INTERPRETATIONS AND CONCLUSIONS

The patterns in these data suggest the following inferences.

1. The primary dimension emerging from this analysis seems to be a tendency on the part of senators in the 87th Congress to restrict American economic spending abroad and to limit Presidential prerogatives in this respect. This suggests a generalized orientation to these foreign policy issues of "guardians of the public purse," perhaps related to views of these programs as competing with "domestic" programs for (what are felt to be) limited resources. Factor analysis of foreign policy roll calls in the 87th Senate, then, detected a primary dimension representing attempts to reduce spending, and to limit economic prerogatives, largely in the substantive area of foreign economic assistance. We are not the first to observe that foreign aid issues, of the general area of all "foreign policy" issues, might be most similar to "domestic" issues in form and process.[15] Perhaps, in the subset of foreign policy issues probably closest to the "domestic" and most distant from the "foreign" poles of that continuum, a predominant pattern of redistributive orientations appears. This confirms to some extent the implications of Rosenau's extrapolation of Lowi's argument: *the more a foreign policy issue falls toward the "domestic" end of the domestic-foreign continuum, the more likely it is to be perceived as redistributive, as opposed to distributive.*

used to perform the factor analysis, *Factor 1*, of the University of Wisconsin Computing Center.

[15] See, for example, the insightful study by Michael Kent O'Leary, *The Politics of American Foreign Aid* (New York: Atherton Press, 1967). O'Leary notes that Congress, pressure groups, and members of the general public often tend to react to foreign aid "in terms of domestic standards and expectations" associated with it, *ibid.*, p. 123.

2. Loose discussions about "isolationist" dimensions within Congress or "isolationist" Congresses may be misplaced. The pattern represented in these data supports claims (which find widespread support in the literature) that Congressmen often feel bewildered by the complexity of foreign affairs issues and would be unlikely to order their orientations on these issues in terms of a single strong ideological continuum. Instead, they are encouraged by the committee structure and fragmentation of duties to consider bills as discrete proposals, and are often motivated to view these bills, regardless of substance, in terms of profligacy or husbandry of government funds.

3. Cynical views of Congress often lead to the suspicion that senators' knowledge of foreign affairs, in many cases, is scant and that their preceptions of these issues, apart from cues by key leaders, are idiosyncratic. This may be the case, and I would not dispute that claim on the basis of my very partial data. Yet it seems, on the basis of the evidence presented here, that there may be latent if not manifest structure to these perceptions. Perhaps the Congressman tends to translate foreign issues into terms by which he orients himself on domestic issues—such as cut or not cut.

Final note must be made of the limitations of this type of analysis as applied to this type of problem. Factor analysis, as used in the fashion I have described, maximizes the possibility of producing order out of chaos, and in the process makes certain assumptions which might be "unrealistic." It is entirely possible that the "real" pattern may be chaos. As applied to foreign policy issue analysis, roll call data may be two steps removed from reality: (1) the executive monopolizes the expertise on much foreign policy decision making and may be the locus for most of the important decisions, with Congress merely exercising its right (and rite) of ratification; (2) roll call data, even for Congress, are only partial information on orientations toward these issues. Further, any attempt to operationalize the formulations of Rosenau, while in the process of extrapolating arguments of Lowi, with roll call data, raises the possibility of an enormous gap between theoretical and operational definitions of identifiable dimensions. The crudity of the data and the operations performed on them could never do complete justice to Rosenau's hypotheses, which require improved longitudinal analyses of this and other types before they can be either verified or dismissed.

INCOME INEQUALITY AND AMERICAN STATE POLITICS*

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The rapidly expanding research literature in comparative state politics typically measures its dependent and independent variables with references to means, averages, and per capitas for each state.[1] (For example, environmental inputs may be expressed as per capita personal income, percent of population living in urban places, median school year completed by the population over 25, etc.; political characteristics may be expressed as average voter turnout levels, percent of the total vote cast for the winning party, etc.; policy outputs are often expressed as per capita expenditures for education, average monthly payments for old age assistance, per capita tax revenues, etc.) With measures such as these, the comparative state politics research has systematically explored many of the linkages between environmental inputs, political system characteristics, and public policy outcomes.

Perhaps the most serious reservation regarding this research is its failure to examine distributive and redistributive aspects of state politics. Both dependent and independent variables are generally expressed as *levels* or *amounts* or *averages* for whole states; these can be neatly arranged for comparative analysis both longitudinally and cross-sectionally. But what about the *distribution* of wealth within a state? Or the *distribution* of public monies among high and low income groups, rural and urban populations, or other divisions within a state's population. The linkages between the distribution of resources within states, the distribution of influence within state political systems, and public policies reflecting distributional decisions, remain largely untested.

For example, scholars have explored the relationship between per capita personal income and levels of public expenditures and services in the 50 states.[2] Income consistently shows up as a very influential variable in determining expenditure levels of state and local governments, and also correlates nicely with party competition and voter turnout. But what is the effect of *inequality in the distribution of income* on politics and public policy within states? Certainly there are good *a priori* reasons for hypothesizing that the degree of income inequality in a state affects both the character of its political system and the content of its policies. One would reasonably expect to find political differences between a state in which wealth is concentrated in the hands of a few, and a state in which wealth is more widely distributed, even if the median incomes of these states are similar.

Fortunately, economists have come up with a way of measuring income distributions within political systems. Income distributions may be observed by means of a Lorenz curve which shows the cumulative proportions of aggregate income (on the vertical or y axis) accruing to cumulative proportions of the population ranging in order from the lowest to highest income-earners (on the horizontal or x axis).[3] In a political system with perfect income inequality, income would cumulate in direct relation to cumulated proportions of the population. Perfect equality is represented on a Lorenz chart by a diagonal straight line rising from the lower left-hand corner to the upper right-hand corner. But in real political systems, the lowest income-earners earn a smaller proportion of the aggregate income than their proportion of all income-earners. Therefore, when cumulative proportions of the population ranging from lowest to highest

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[1] See, for example Richard E. Dawson and James A. Robinson, "Interparty Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics,* 25 (May, 1963), 265–289; Richard I. Hofferbert, "The Relationship Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW, 60 (March, 1966), 73–82; Ira Sharkansky, "Economic and Political Correlates of State Government Expenditures," *Midwest Journal of Political Science,* 11 (May, 1967) 173–192; John Crittenden, "Dimensions of Modernization in the American States," this REVIEW, 61 (December, 1967), 989–1001; Thomas R. Dye, *Politics, Economics and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966).

[2] See Footnote 1.

[3] M. C. Lorenz, "Methods of Measuring the Concentration of Wealth," *Publications of the American Statistical Association,* 9 (1905), 209–219; see also Mary J. Bowman, "A Graphical Analysis of Personal Income Distributions in the United States," *American Economic Review,* 35 (September, 1945), 618–628.

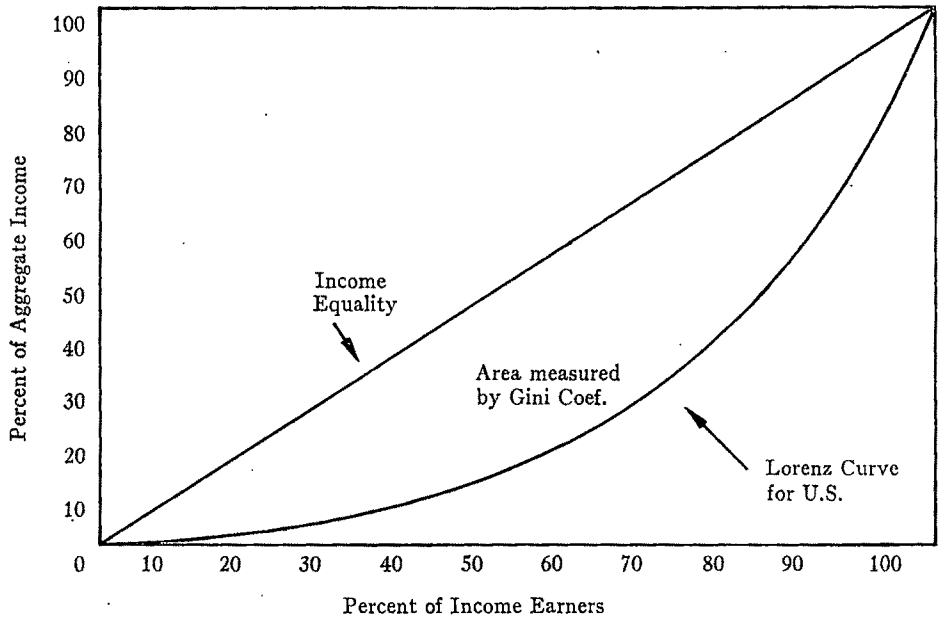


FIG. 1. The Lorenz Curve of Income Inequality.

income-earners are plotted against the proportion of aggregate income they actually receive, the result is a convex "Lorenz" curve. The greater the degree of convexity of this plotted curve, the greater the degree of inequality in the political system. The total area on a diagram which falls between the Lorenz curve, representing the actual income distribution, and the straight diagonal line, representing perfect income equality, expresses the extent of income inequality within a political system. This area is measured by a Gini coefficient or Gini index, which ranges from a +1.00 (theoretical perfect inequality) to 0.00 (theoretical perfect equality).[4]

Gini indices for the 50 states and the United States are presented in Table 1.[5] Income inequality is greatest in Mississippi and least in Utah. The utility of this measure is obvious: It provides a single comparable measure for each state and permits us to systematically explore linkages between income inequalities and other measurable political system and public policy variables.

But before we turn to an examination of the relationships between income distributions and public policy, it is important to observe the relationships between income distributions and other environmental variables in the states. Table 2 shows that income inequality is closely associated with levels of income, urbanization, industrialization, and adult education in the states. The lower a state's median income, the greater the income inequality. Inequality is also associated with ruralism, agriculturalism, and lower adult education levels.[6] These environ-

[4] Corrado Gini, "On the Measure of Concentration with Especial Reference to Income and Wealth," Paper Delivered before the Cowles Commission, 1936; see also James Morgan "The Anatomy of Income Distributions," *The Review of Economics and Statistics,* 44 (August, 1962), 270–280.

[5] These Gini indices were computed by Thomas D. Hopkins from income distributional data for total families and unrelated individuals in 1959 from U. S. Bureau of the Census, *U. S. Census of Population: 1960* serves PC (1) 1C to 53C, "General Social and Economic Characteristics," Table 65. See Thomas D. Hopkins, "Income Distribution in Grants-in-Aid Equity Analysis," *National Tax Journal,* 18 (June, 1965), 209–213. Slightly different Gini indices were computed from the same source by Al-Samarrie and Miller which correlate .89 (product moment) with the indices above and produce substantially the same results. See Ahmad Al-Samarrie and Herman P. Miller, "State Differentials in Income Concentration," *American Economic Review,* 57 (March, 1967), 59–72.

[6] These findings parallel those of Al-Samarrie and Miller who undertook a comprehensive explanation of income inequalities in the states. They regressed Gini coefficients on ten independent variables, including property income as a percent of total

TABLE 1. INCOME DISTRIBUTIONAL INEQUALITY IN THE AMERICAN STATES

State	Gini Coefficient	State	Gini Coefficient
1. Mississippi	.510	26. Minnesota	.431
2. Arkansas	.486	27. North Dakota	.430
3. Alabama	.478	28. New York	.429
4. Tennessee	.478	29. California	.427
5. Louisiana	.477	30. Colorado	.425
6. South Carolina	.474	31. Maryland	.424
7. Kentucky	.474	32. Illinois	.423
8. Georgia	.469	33. Rhode Island	.418
9. Oklahoma	.465	34. Montana	.415
10. North Carolina	.465	35. Indiana	.414
11. Texas	.464	36. Nevada	.414
12. Florida	.462	37. Massachusetts	.414
13. Virginia	.461	38. Washington	.413
14. Missouri	.459	39. Pennsylvania	.412
15. South Dakota	.456	40. Wisconsin	.412
16. Alaska	.456	41. Maine	.412
17. West Virginia	.451	42. Oregon	.411
18. Hawaii	.446	43. Michigan	.409
19. Arizona	.445	44. Ohio	.408
20. New Mexico	.440	45. New Hampshire	.407
21. Nebraska	.440	46. Connecticut	.404
22. Kansas	.439	47. New Jersey	.403
23. Iowa	.439	48. Idaho	.402
24. Delaware	.434	49. Wyoming	.399
25. Vermont	.434	50. Utah	.394
U.S. as a whole: GINI = .439			

mental variables and many policy outcomes in the fields of education, health, welfare, highways, and taxation.

personal income, agricultural earnings as a percent of total labor earnings, median school years completed by persons 25 years and over, percent nonwhite population, and employment as a percent of population. Percent non-white turned out to be their single most explanatory variable; they did not employ a measure of family income. See Ahmad Al-Samarrie and Herman P. Miller, "State Differentials in Income Concentration," *American Economic Review*, 57, (March, 1967), 59–72.

Table 2 also shows the relationships between income distributions and two environmental factor scores derived by Richard I. Hofferbert.[7] These factor scores purport to show underlying dimensions of socio-economic variables in the states; Hofferbert employed factor analysis to identify two independent environmental dimensions which he labeled "Industrialization" and

[7] Richard I. Hofferbert, "Socio-economic Dimensions of the American States: 1890–1960", *Midwest Journal of Political Science*, 12 (August, 1968), 401–418.

TABLE 2. INCOME INEQUALITY AND ECONOMIC DEVELOPMENT IN THE STATES

Figures are simple correlation coefficients with Gini Index*			
Variable	Coefficient	Variable	Coefficient
Per Capita Personal Income	−.64	Median School year completed	−.69
Median Family Income	−.77	Nonwhite Population	.72
Urbanization	−.50	Industrialization Factor Score	−.35
Industrialization	−.36	Cultural Enrichment Factor Score	−.67

* All correlation coefficients reported in this study are product moment and not rank order coefficients.

TABLE 3. RELATIONSHIPS BETWEEN INCOME INEQUALITY AND SELECTED POLITICAL SYSTEM VARIABLES IN THE AMERICAN STATES

Selected Political Variables	Coefficients Obtained with Gini Index	Compared to coefficients obtained with specific levels of: Income	Industrialization	Education	Urbanization	Compared to coefficients obtained with factor scores for: Industrialization	Cultural Enrichment
Party Competition							
Governor	−72*	66	18	61	29	22	63
Lower Houses	−61*	52	00	56	26	19	54
Upper Houses	−59*	51	03	49	30	23	52
Voter Participation							
Governor	−71*	52	05	49	17	18	53
Congress	−80*	66	09	63	26	19	60
Democratic Success							
Governor	66*	−56	02	−59	−22	−11	−60
Lower Houses	72*	−46	−10	−54	−05	−13	−52
Upper Houses	74*	−48	−03	−51	−15	−16	−51
Malapportionment							
Index of Rep.	06	−21	−19	−18	−24	−03	−21
Urban Under-rep.	−47*	36	33	06	27	54	05
Apportionm. score	−32*	14	15	13	00	19	06
Interest Group Strength	42*	−42	08	−25	−06	−15	−21
Length of Constitution	30*	−20	03	−28	05	−04	−24
Elected St. Agency Heads	43*	−53	−42	−32	−28	−41	−26
Governor's Power Index	−42	60*	38	37	44	37	36

* Highest simple coefficient among independent variables.

"Cultural enrichment."[8] All of Hofferbert's base variables are per capitas, means, or averages for whole states; they do not include distributional variables. Apparently income inequality is negatively associated with both the "industrialization" factor and the "cultural enrichment" factor; an increase in industrialization or cultural enrichment is associated with a reduction in income inequality.

Table 3 shows the relationships between income inequality and several common political system variables in the states.[9] One may compare the coefficients obtained with Gini index, with the coefficients obtained with specific socio-economic variables and with factor-analyzed socio-economic dimensions.

[8] The composition of Hofferbert's factors can be observed in the following factor loadings: "Industrialization" — value mfg. .91, pop. in mfk. .88, farm value .83, density .78, foreign born .70, population .67, urban .66, telephones .65, number of employees .64, income .57, business failures .42, property .13, negro .07, illiteracy .04, pop. increase −.00, school years −.03, tenancy −.27, owner-occupied −.32, divorce −.33, acreage −.50, motor vehicles −.57; "cultural enrichment"—school years .91, property .79, income .73, motor vehicles .70, telephones .68, pop. increase .55, urban .52, acreage .49, divorce .43, business failures .29, owner-occupied .24, foreign-born .23, population .05, farm value .02, value mfg. .01, density .01, pop. in mfg. −.13, number of employees −.35, tenancy −.47, illiteracy −.74, negro −.75: *ibid.*

[9] These political system variables are: Competition: one minus the percentage of votes cast in Gubernatorial elections 1954 to 1964 for majority party, one minus the proportion of seats held by the majority party in the upper and lower chambers of the state legislature 1954 to 1964; voter participation: average voter turnout in gubernatorial elections 1954 to 1964, average voter turnout in congressional elections 1962 and 1964; Democratic success: the Democratic percentage of the two-party vote for Governor 1954 to 1964, and the Democratic proportion of seats in the upper and lower chambers of state legislatures 1954 to 1964; malapportionment: the index of representatives suggested by Manning J. Dauer and Robert G. Kelsay "Unrepresentative States," *National Municipal Review,* 44 (December, 1955), 571–575, updated to 1960; the index of urban underrepresentation suggested by Paul T. David and Ralph Eisenberg, *Devaluation of the Urban and Suburban Vote* (Bureau of Public Administration, University of Virginia, 1961); and the apportionment score suggested by Glendon Schubert and Charles Press. "Measuring Malapportionment," this REVIEW, 58 (June, 1964), 302–327, with corrections published December, 1964, pp. 966–970; interest group strength: judgements of respondents reported by Belle Zeller, *American State Legislatures* (New York: Crowell, 1964), pp. 190–191; length of constitution, number of constitutional amendments

TABLE 4. RELATIONSHIPS BETWEEN INCOME INEQUALITY AND SELECTED POLICY VARIABLES IN THE AMERICAN STATES[11]

Selected Policy Variables	Coefficients Obtained with Gini Index	Compared to coefficients obtained with specific levels of: Income	Industrialization	Education	Urbanization	factor scores for: Industrialization	Cultural Enrichment
Education							
Per pupil Expenditures	−.61	.83*	.36	.59	.51	.45	.61
Educ. Exp. % of Income	.19	−.28	−.44*	.05	−.27	−.58	.12
State % Educ. Exp.	.45*	−.23	.18	−.35	.09	.09	−.39
Average Teach. Salaries	−.54	.88*	.64	.57	.69	.58	.53
Size School Districts	.36	−.18	.26	−.39*	.06	.04	−.43
Teacher-Pupil Ratio	.55	−.43	.19	−.50	−.12	−.05	−.56*
Drop-out Rate	−.62	.54	−.08	.60	.40	−.22	−.64*
Mental Failures	−.62	.46	.13	.70*	−.05	.20	−.66
Health & Welfare							
Unemployment Benefits	−.68	.80	.30	.67	.54	.39	.66
ADC Benefits	−.78*	.74	.26	.55	.61	.42	.56
General Asst. Benefits	−.68	.76*	.39	.43	.58	.55	.42
ADC Recipients	.41	−.30	.16	−.42	−.15	−.04	−.44*
Unemploy. Recipients	−.53	.58	.69*	.23	.39	.60	.23
Per Cap. Welfare Exp.	.04	−.01	.07	.07	.19	−.02	.12
Per Cap. Health Exp.	−.34	.56*	.39	.42	−.45	.27	.44
State % Welfare	.37	−.35	−.14	−.17	−.11	−.38*	−.12
State % Health	.09	−.08	−.07	−.14	−.30*	−.04	−.24
Taxation							
Total Revenue Per Cap.	−.50	.64	.03	.75*	.29	−.13	.75*
Per Cap. Tax Revenue	−.66	.76*	.23	.74	.59	.28	.77*
Taxes % of Income	−.07	−.17	−.51*	−.16	−.15	−.45	.16
State % Taxes	.49*	−.33	−.08	−.24	−.28	−.39	−.32
Sales Tax Reliance	.13	−.15	−.01	−.18	−.03	−.08	−.12
Income Tax Reliance	−.05	.20	.02	.19	−.04	.08	.05
Highways							
Per Cap. Highway Exp.	.26	.01	−.51*	.36	−.37	−.54	−.36
State % Highway Exp.	.06	−.15	.04	−.03	−.29	−.30*	−.01
Highway Fund Diversions	.05	.06	.29	−.05	.42*	.31	.02
Rural-Urban Distribution	.29	−.38	−.45*	−.07	−.35	−.42	−.16
Highway User Revenue	.19	−.32*	−.20	−.26	−.04	−.12	−.17

* Highest simple coefficient among independent variables.

Cross-sectionally speaking, an increase in income inequality is associated with a reduction in party competition for state office, a reduction in voter participation, an increase in Democratic voting for state offices, an increase in interest group strength, more fragmentation in state executive organization, and a reduction in the governor's formal powers. But what is even more noteworthy is the fact that the distribution of income is *more* closely related to these political system variables than absolute measures of social and economic resources in the states or factors derived from such absolute measures. The coefficients obtained with the Gini index are higher than those obtained with measures of the level of social and economic resources or with environmental factor scores.[10] These admittedly rough calculations suggest that the *distribution* of social and economic resources within a state may be more important politically than the *level* of social and economic resources.

A different picture emerges, however, when we examine the relationships between income in-

and number of elected state agency heads: see Lewis A. Froman, "Some Effects of Interest Group Strength in State Politics," this REVIEW, 60 (December, 1966), 952–962; governors formal power index: see Joseph M. Schlesinger, "The Politics of the Executive" in Herbert Jacob and Kenneth Vines (eds.), *Politics in the American States* (Boston: Little, Brown, 1965), p. 229.

[10] Unfortunately, the degree of inter-relatedness between income inequality (Gini) and income levels as well as other socio-economic variables render partial correlations and beta values unreliable as a means of testing the independent effect of income inequality on political system variables or public policy. A simple comparison of the size of the correlation coefficients is probably the most efficient method of comparing income inequality with levels of socioeconomic resources.

[11] The variables in Table 4 were selected from over 100 policy measures for which similar results were obtained. The specific measures in Table 4

equality and measures of the *level* of public spending and service in education, health, welfare, taxation and highways. While it is true that the Gini index correlates with a large number of policy outcome measures in these fields (see Table 4) the coefficients obtained with the Gini index are usually not as high as those obtained with specific socio-economic indicators or with factors reflecting environmental dimensions. (The only policy variables which are more closely related to Gini than to levels of socio-economic resources deal with centralization in state-local relations, e.g., state percent on total educational expenditures, state percent of total welfare expenditures, and the state percentage of total tax revenues. This means that income inequalities are associated with centralization of governmental spending and services at the state level.) This suggests that overall *levels* of public service in the states are more closely related to *levels* of social and economic resources than to the *distribution* of these resources.

The purpose of this note is to suggest that the distribution of resources within the states may have a significant impact on the character political systems and the content of policy output. Levels of economic development are certainly influential determinants of politics and public policy, but it is also probable that the distribution of economic resources also shapes politics and public policy in the states.

This brief exercise with a single measure of the distribution of resources suggest an interesting hypotheses for further study: The *distribution* of resources within American states are more important in shaping state *politics* than *levels* of socio-economic development; but *levels* of socio-economic development are more influential in determining *levels of public spending and service* in the states than the *distribution* of resources.

are: Education: per pupil expenditures in ADA 1961–62, educational expenditures as a percent of total personal income 1961, state percentage of total state-local expenditures for education 1961, average teachers salaries 1961–62, average size of school district 1961–62, teacher pupil ratio, high school graduates in 1963 as a percent of 9th graders in 1959, percent of selective service examinees disqualified for failing mental test 1962; Health and Welfare: unemployment compensation average weekly payment 1961, ADC average monthly payment 1961, general assistance average monthly payment 1961, ADC recipients per 10,000 population 1961, per capita state-local welfare expenditures 1961, per capita state-local health expenditures 1961, state percentage of total state-local expenditures for welfare, state percentage of total state-local expenditures for health; taxation: total state-local revenue per capita 1961, taxes as a percent of personal income 1961, state percentage of total state-local tax revenue 1961, sales taxes as a percent of state tax revenue 1961, income taxes as a percent of state tax revenue 1961; Highways: per capita state-local expenditures for highways 1961, state percentage of total state-local expenditures for highways 1961, percentage of highway revenues diverted to non-highway purposes 1961, percent of state highway grants going to counties rather than municipalities 1961, highway user revenues as a percent of total state revenue 1961.

THE CLASS BASIS OF ARGENTINE POLITICAL PARTIES*

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Students of Latin American political parties have long assumed a strong correlation between social class and party identification, yet this assumption has never been tested empirically in any of the Latin American nations. This is probably due in large part to the lack of reliable survey data; however, even the mass of aggregate voting data has seldom been analyzed systematically. As a result, most of what we know —or think we know—about voting behavior in Latin America is based upon the intuitive assumptions of North American scholars. "If I were a member of the Chilean middle class, I would probably vote for the Radicals or Christian Democrats, but on the other hand if I were quite wealthy I would probably vote for the Conservatives."

Students of Argentine politics assume that the Conservative parties, always representing the interests of the nation's aristocracy, have received the bulk of their votes from the large landowners; that the interests of this group consistently have been opposed by the Radical parties who receive their electoral support from the urban middle classes; and that the more recently formed Peronist parties have championed the cause of, and been supported at the polls by, the industrial workers. It is the purpose of this article to test these assumptions, primarily through analysis of aggregate voting data, but also by examining the social backgrounds of party leaders and their actions while in power.

I

Contrary to the popular myth, it now appears that prior to the 1943 revolution very few differences existed between the social backgrounds of the leaders of Argentina's major political parties, and also that correlations between social class and voting behavior were quite insignificant. Gallo and Sigal in their study of candidates for major elective offices in 1916 conclude that "as far as occupation, nationality and education are concerned, the Radicals do not differ in any way from their Conservative counterparts."[1] In each case the vast majority of the candidates came from the well-educated upper and upper-middle classes.[2] The same was true of the members of the national committees of these parties, almost all of whom were recruited from the economically privileged classes prior to 1943.[3]

TABLE 1. RANK ORDER CORRELATIONS BETWEEN OCCUPATION AND VOTING IN THE FEDERAL CAPITAL: 1940

Occupation	Conservatives	Radicals
All workers	−.076	−.243
Industrial workers	−.268	−.340
Public service workers	−.004	−.196
All employees	+.104	+.353
Public employees	+.238	+.250
Professionals	+.316	+.226
Industrialists	−.262	−.032
Businessmen	−.217	+.272

Source: Gino Germani, *La estructura social de la Argentina* (Buenos Aires: Editorial Raigal, 1955), pp. 253–255.

There is also some evidence which indicates that occupation had little effect upon voting behavior during this period. Using aggregate data, Germani found only very weak correlations between occupation and party vote in the 1940 congressional elections in the Federal Capital—

[1] Ezequiel Gallo and Silvia Sigal, "La formación de los partidos políticos contemporáneos: la UCR (1890–1916)," in Torcuato DiTella et al. *Argentina, Sociedad de Masas* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1965), p. 163. A separate study shows the same to have been true of those who were elected to Congress in 1916. See Dario Canton, *El Parlamento Argentino en épocas de cambio* (Buenos Aires: Editorial del Instituto, 1965), pp. 52–66.

[2] According to Gallo and Sigal (*op. cit.*, pp. 163–164) 69% of the Conservatives and 71% of the Radicals came from these classes, while 87% of the Conservatives and 91% of the Radicals had a university education.

[3] See José Luis de Imaz, *Los que mandan* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1964), p. 185.

* Much of the data included in this article was obtained between September 1966 and August 1967 while the author was in Argentina. This trip was made possible by the generous financial assistance of the Henry and Grace Doherty Foundation and the University of Iowa Research Council.

and these correlations are even less significant when compared to those of 1946.

Huerta Palau comes to the same conclusion with regard to the 1935 gubernatorial election in Córdoba. There, in spite of the fact that the Radical candidate (a country doctor) waged a vigorous campaign designed to appeal to both urban and rural laborers, while the candidate of the Conservatives (himself a large landowner) obviously represented the interests of the traditional upper class, there was virtually no correlation between occupation and party vote.[4]

The popular view of the Radicals of this period as representatives of the urban middle classes who consistently opposed the interests of the landowning aristocracy is further refuted by the actions of Radical congressmen. For example, Peter Smith has shown that between 1916 and 1930, while the Radicals were in control of the national government, ninety bills which obviously were favorable to the interests of the livestock producers were presented in Congress; Radicals introduced sixty per cent of these bills, the Conservatives twenty-nine per cent, and Progressive Democrats and Socialists the remaining eleven per cent.[5]

II

The picture suggested by the above studies changed a great deal after the rise of Peronism, and it now appears that since 1943 there have been substantial differences in the social composition of Argentina's major political parties—at least between the Peronists on the one hand, and the non-Peronists on the other. As far as party leaders are concerned, this difference may be seen in the class structure of the various national committees. In the twenty-five year period between 1936 and 1961 over ninety per cent of the Radical and Conservative national committee members were from the upper or upper-middle classes, while the remainder were uniformly from the lower-middle class; in contrast, over a third of the Socialists came from the lower-middle class, and almost a third of the Peronists were lower class in origin.

Additional differences in party leadership may be seen in Table 3 which gives the primary occupation of Argentine congressmen serving in 1890 (when the Conservatives ruled unchallenged), in 1916 (the first year in which a large number of Radicals were elected), in 1946 (the first year of Peronist domination), and in 1966 (the last year the Congress met). In 1916 the differences in occupation between Conservative and Radical congressmen were relatively insignificant. Members of the liberal professions dominated both delegations; however, about one and a half times as many Radicals as Conservatives were landowners (just the opposite from what the popular myth would lead one to expect). In the 1946 Congress the differences between Radicals and Peronists were far greater. Over half the Radical congressmen were lawyers and 84% were members of one of the liberal professions; less than a third of the Peronists were lawyers, and only half of them were professionals of any type. More important, 17% of the Peronist congressmen were classified as white collar workers (*empleados*), and another 17% as laborers (*obreros*). None of the Radicals fell into these categories; in fact it seems quite likely that these Peronists were the first of their occupation ever to serve in Congress.

Basically the same differences were repeated in 1966 when three-fourths of the Radicals and almost all of the Conservatives were members of the liberal professions, while this was true of less than half of the Peronists. In contrast, 31% of the Peronist congressmen were either white collar workers or laborers versus 4% of the Radicals and none of the Conservatives. It might also be noted that the largest percentage of landowners was to be found not among the Conservatives, but among the Peronist delegation.

It also appears that occupation became an important factor in voting behavior after 1943. As was mentioned above, Germani has shown that in the 1946 congressional elections in the Federal Capital the correlations between occupation and party vote were extraordinarily high —and one should remember that just six years earlier such correlations virtually did not exist. The predominantly working class districts tended to vote Peronist, while Radical candidates fared best in districts in which there were large numbers of white collar workers, members of the liberal professions, and businessmen (the

[4] Pedro Huerta Palau, *Análisis electoral de una ciudad en desarrollo* (Córdoba: Universidad Nacional de Córdoba, 1963), p. 27.

[5] Peter Smith, "Los radicales argentinos y la defensa de los intereses ganaderos, 1916–1930," *Desarrollo Económico* 7 (April–June, 1967), p. 826.

TABLE 2. SOCIAL COMPOSITION OF NATIONAL COMMITTEES: 1936–1961

Class	Conservative	Radical	Socialist	Peronist
Upper and upper middle classes	97%	90%	62%	63%
Lower middle class	3%	10%	36%	5%
Lower class	—	—	2%	32%
N	39	53	52	79

Source: José Luis de Imaz, *Los que mandan* (Buenos Aires: Editorial Universitaria de Buenos Aires, 1964), p. 102.

TABLE 3. PRIMARY OCCUPATION OF ARGENTINE CONGRESSMEN

Occupation	1890	1916		1946		1966		
	Con	Con	Rad	Rad	Per	Con	Rad	Per
Lawyers	65%	66%	57%	51%	32%	71%	36%	31%
Doctors	10%	10%	13%	18%	9%	18%	17%	4%
Other liberal professions	4%	7%	7%	15%	9%	—	21%	10%
Landowners	8%	15%	23%	10%	12%	6%	7%	10%
Teachers	4%	2%	—	3%	2%	—	1%	4%
Military	9%	—	—	3%	3%	—	—	—
White collar	—	—	—	—	17%	—	3%	23%
Laborers	—	—	—	—	17%	—	1%	8%
Other	—	—	—	—	—	6%	13%	10%
N	78	59	44	39	106	17	94	49

Source: Dario Canton, *El parlamento argentino en épocas de cambio* (Buenos Aires: Editorial del Instituto, 1966), pp. 40, 55–56; and data supplied the author by the División Información Parlamentaria de la Cámara de Diputados de la Nación.

last mentioned to a somewhat lesser extent). In contrast, there was no significant correlation between the number of industrialists in a district and the vote received by either party.

Huerta Palau's study of the 1962 election in Córdoba shows basically the same thing—although the correlations are lower. The Peronists received the bulk of their votes in districts in which there were large numbers of workers and/or farmers, while faring quite poorly in areas dominated by students, professionals and businessmen. Virtually the reverse was true of the two Radical parties, although the Intransigent Radicals fared much better than the People's Radicals in districts composed of large numbers of businessmen.

III

Some additional indication of the effects of social class upon Argentine voting behavior can be gained from an examination of election returns in each of the twenty districts into which the Federal Capital is divided. In each of the six post-Perón elections the wealthier northern districts cast more Conservative and fewer Peronist votes than did the poorer southern districts. However, these districts are far too large and heterogeneous to be of great use. Fortunately, the results of the 1957 election are available for each of the Federal Capital's 209 circuits (roughly equivalent to precincts in the United States). As far as social composition is concerned these smaller units are much more homogeneous than the districts, and thus more suitable for an analysis of the relationship between class and voting.

Fifteen circuits were selected for analysis. Four of them would be considered upper class by almost any standard; two are in the Retiro

TABLE 4. RANK ORDER CORRELATIONS BETWEEN OCCUPATION AND VOTING IN THE FEDERAL CAPITAL: 1946

Occupation	Peronists	Radicals
All workers	+.973	−.908
Industrial workers	+.898	−.966
Public service workers	+.920	−.802
All employees	−.653	+.692
Public employees	−.698	+.720
Professionals	−.869	+.918
Industrialists	−.095	−.036
Businessmen	−.515	+.435

Source: Gino Germani, *La estructura social de la Argentina* (Buenos Aires: Editorial Raigal, 1955), pp. 253–255.

TABLE 5. PRODUCT MOMENT CORRELATIONS BETWEEN OCCUPATION AND VOTING IN THE CITY OF CORDOBA: 1962

Occupation	Conservative	Peronist	UCRP	UCRI
Farm laborers	−.558	+.587	−.544	−.471
Skilled labor	−.541	+.631	−.791	−.931
Farmers	−.454	+.324	−.148	−.304
Employees	+.020	−.052	+.024	−.006
Students	+.908	−.895	+.489	+.714
Professionals	+.406	−.866	+.519	+.742
Businessmen	+.330	−.999	−.295	+.267
% of the total vote	4%	36%	28%	14%

Source: Pedro Huerta Palau, *Análisis electoral de una ciudad en desarrollo* (Córdoba: Universidad Nacional de Córdoba, 1963), p. 19.

TABLE 6. VOTING BEHAVIOR IN SELECTED CIRCUITS IN THE FEDERAL CAPITAL: 1957
(in percentages)

Circuit type	Com	Soc	Peron	UCRI	UCRP	PDC	Con	PCI	other
Upper class	2.0	7.2	9.2	13.9	26.1	13.4	6.7	12.1	8.6
Upper-middle	2.4	11.2	13.2	15.8	28.5	8.7	2.8	8.7	8.9
Lower-middle	4.0	16.2	21.3	19.4	25.2	4.4	0.9	1.1	7.5
Lower class	6.2	10.6	28.1	20.7	21.8	2.8	0.9	0.8	6.2
Entire capital	4.1	14.7	18.4	19.5	26.3	5.7	1.7	2.7	6.9
All Argentina	2.6	6.0	24.3	21.2	24.2	4.8	6.5	1.0	9.4

Note: The upper class circuits are numbers 194, 200, 203 and 204; the upper-middle class circuits are 163, 166 and 168; the lower-middle class circuits are 32, 33, 174 and 175; the lower class circuits are 7, 8, 10 and 13.

area, which according to Imaz[6] has almost half of the Buenos Aires aristocracy, while the other two are in Barrio Parque, the one part of the city composed exclusively of large expensive houses. The three circuits labeled upper-middle class are in Barrio Belgrano near the *barrancas;* this is an area composed primarily of large, new, expensive apartment houses. Two of the lower-middle class circuits are located in Colegiales and the other two in La Boca; in each area older and cheaper apartment houses are predominant. These two widely separated neighborhoods give the impression of being "run down" but not poverty stricken. The four lower class circuits are in the slaughter house and meat packing *barrios* of Nueva Chicago, Villa Lugano and Nueva Pompeya.

One additional point pertaining to the social composition of these circuits must be mentioned. This is the fact that almost all upper and upper-middle class families in Buenos Aires employ "live-in" domestic servants who vote in the same circuits as their employers. While many lower-middle class families also have servants, they are much less likely to live-in, and thus do not affect this analysis to any appreciable extent. It is thus only the lower class circuits that are completely homogeneous in character.

As can be seen in Table 6, the percentage of the vote received by the two conservative parties (the business-oriented Independent Civic Party and the more rural-based Conservative Party) declines as one goes down the social scale. Virtually the same is true of the Christian Democrat vote, except that while the conservatives received almost as few votes from members of the lower-middle class as from the lower class, the former was somewhat more inclined to vote for Christian Democrats. This picture was reversed in the case of the Communists and Peronists and, to a limited extent, the Intransigent Radicals. The first two parties received more than three times as many votes in lower class circuits as in upper class ones, while the UCRI vote increased gradually from 13.9% to 15.8% to 19.4% and finally to 20.7%.

The People's Radicals fared slightly better among upper-middle class voters (28.5%) than among upper class and lower-middle class (26.1% and 25.2% respectively), while their worst showing was a still quite formidable 21.8% among the lower class. Virtually the reverse was true of the Socialists who received somewhat more votes from the lower-middle class than from either the upper-middle or lower classes (16.2% versus 11.2% and 10.6%); their poorest showing was in the upper class circuits where they received only 7.2% of the vote.

If one assumes that the Independent Civic and Conservative parties are rightist, the two Radical parties and the Christian Democrats centrist, and the Communists, Socialists and Peronists leftist—as both their programs and actions would indicate—it becomes apparent that the vote for center parties is remarkably stable across class boundaries, with only the extremist vote shifting appreciably with social class.

It is most important to note that while the conservative parties derive virtually all their electoral support from the upper and upper-middle classes, Peronist support is by no means limited to the lower class, nor are the Radical voters uniformly from the middle classes. While most of the Peronist votes cast in the upper and upper-middle class circuits may well have come from domestic servants, this is almost certainly not the case in the lower-middle class circuits where over 20% of the voters supported Peronist candidates. It should also be noted that the sup-

[6] José Luis de Imaz, *La clase alta de Buenos Aires* (Buenos Aires: Universidad Nacional de Buenos Aires, 1965), p. 15.

posedly middle class Radicals received far more votes in the lower class circuits than did the Peronists (42.5% versus 28.1%). Likewise, the two Radical parties garnered twice as many votes as the Conservative parties in the upper class circuits.

Thus, judging from the electoral behavior of the inhabitants of these fifteen circuits, it seems safe to conclude that Argentine conservatism is limited in appeal to the upper and upper-middle classes, and that Peronism is largely a movement of the lower and lower-middle classes; however, it is obviously inappropriate to turn this around and claim that the upper class is predominantly conservative in its voting habits, or that "the Peronists speak for a majority of the wage earners."[7] Also, the Radicals, who received between forty and forty-five per cent of the total vote in each of the four categories of circuits, must be considered multi-class rather than merely representative of the middle sectors of society.

[7] Robert J. Alexander, "The Emergence of Modern Political Parties in Latin America," in Joseph Maier and Richard W. Weatherhead (eds.), *The Politics of Change in Latin America* (New York: Frederick A. Praeger, 1964), p. 107.

PROCEDURES FOR REVIEWING MANUSCRIPTS

AUSTIN RANNEY, MANAGING EDITOR

In response to several inquiries and a suggestion from the Editorial Board, I present here a brief description of the REVIEW's procedures for reviewing manuscripts. The reader should be clear that what follows is *not* a discussion of substantive editorial standards comparable to that published by my predecessor (Harvey C. Mansfield, "Toward a Definition of Editorial Policy for the REVIEW," 56 [March, 1962], 129–138); it is simply an account of the procedures we follow in evaluating and deciding on the manuscripts submitted to us.

Objectives. Our reviewing procedures are intended to accomplish four main objectives: First, to select from among the manuscripts submitted those most suitable for publication. Second, to work the manuscripts we accept into the best possible shape before they appear in the awful finality of print. Third, to give every author who submits a manuscript a serious professional critique of his work whether we publish it or not. And fourth, to get a decision to the author within a reasonably short period of time (we try for, but do not always achieve, a maximum of two months).

Preliminary Screening. After each new manuscript is received, acknowledged, and logged, I leaf through it to determine whether it meets at least a minimum standard of scholarly quality. I estimate that about 95 per cent of all manuscripts pass this not-very-demanding test (not nearly demanding enough according to some of my reviewers), but a few do not. An example of the latter is a mimeographed speech on the Perils of Big Government made to a convention of commercial advertisers by the president of an advertising firm and submitted to the REVIEW by one of the firm's account executives.

Professional Review. The manuscript is then sent out for review, usually to at least two referees but sometimes to one. Each referee is chosen by me, often on the recommendation of a member of the Editorial Board, because he is regarded as an expert in the content area and/or special methodology covered by the manuscript and also as a good bet to answer his mail and meet a deadline. In the past three years I have used well over two hundred different persons as referees for one or more—usually more—manuscripts each.

We try, not always successfully, to ensure maximum objectivity in the reviews by preserving the author's anonymity; hence, his name and identifying footnotes and other references are removed before the manuscript is sent out. The reviewer is asked in a covering letter to comment on the manuscript's strong and weak points and to give us his net judgment as to whether the article is (1) publishable as it stands, (2) potentially publishable if certain changes, which the reviewer is asked to spell out, are successfully made, or (3) not publishable with any foreseeable changes. The reviewer is also given a deadline for delivering his comments to us, generally set at a month to six weeks after his receipt of the manuscript.

Most referees meet these deadlines faithfully and deliver thoughtful and often extended comments. I repeat here my conviction, expressed in each of the last three Annual Reports, that the reviewers' prompt, skilled, and conscientious performance of these demanding but unpaid professional chores is by far the most important single contribution made to whatever approach we make to achieving the objectives set forth above. A few referees, for one reason or another, do not meet their deadlines. They are often rewarded with a telephone call from the Managing Editor, which usually, though not always, stimulates delivery of their comments.

If the referees of a particular manuscript report substantially different evaluations and/or if I remain in doubt about what to do with it, I send it to one or two additional referees. The final decision, of course, is the Managing Editor's alone. But the content specialties in contemporary political science are so multifarious and the methodological problems and techniques so diverse that I—and, I like to think, any Managing Editor—must rely heavily on expert advice for evaluating manuscripts in most fields other than my own.

Decision. When all the reviews are in, I read them carefully, compare and synthesize their evaluations, often look over the manuscript again myself in the light of the reviews, and eventually come to one or another of three decisions:

(1) To accept for publication. In this case I write to the author informing him that his article is accepted and indicating the issue in which it is scheduled to appear. I also stipulate a deadline for receiving the final version of the manuscript to be copy-edited and prepared for the printer (the deadline at present is approximately three-and-one-half months prior to the

month of publication). I enclose copies of the readers' comments* and make whatever suggestions for changes seem desirable in preparing the final version.

(2) To reject. In this case I write to the author informing him that we cannot publish his manuscript and stating the reasons for the decision. Copies of the readers' comments, which usually set forth these reasons in more detail, are also enclosed so that the author has at least gained a serious professional critique of his work.

(3) To suggest revision and re-submission. In this case I inform the author that we cannot publish his manuscript in its present form, state the reasons for the decision, and enclose copies of the readers' comments. I then invite him to re-analyze his data and/or re-write his presentation along certain specified lines and to submit the revised version for further consideration. It is made clear that this invitation cannot be a guarantee to publish the revised version but is, rather, an expression of interest in considering it. I also acknowledge that the author may feel his article is publishable as it stands and may wish to submit it elsewhere, and I ask him to tell us what he intends to do. If he then prepares and submits a revision, as many in this category do, the new version is put through the reviewing process again, usually with one of the readers of the first version serving as a referee of the second.

The Results. From August 1, 1965 to July 31, 1968—the period covered by the first three Annual Reports of my tenure as Managing Editor—a total of 727 manuscripts were submitted. Of these, 122 (16.8 per cent) were accepted for publication in either their original or revised versions; 60 (8.2 per cent) were returned with an invitation to revise and re-submit, but as yet no revised versions have been received; and 545 (75.0 per cent) were rejected.

I mentioned at the outset that this note would not deal with the substantive standards applied by the journal's advisers, referees, and Managing Editor in evaluating and making decisions on manuscripts—a much larger and more complex topic than any discussed here. I hope, however, that this brief account of our decision-making machinery will be of interest to all our readers and useful to those who contemplate submitting manuscripts.

*We also try to ensure maximum objectivity and frankness in our reviews by making every effort to conceal the referees' identities from the authors. Hence we remove all identifying marks and phrases from the reviews before sending them on.

COMMUNICATIONS

To THE EDITOR:

I have read with some astonishment in the appendage to the lead communication in the December REVIEW that "the Managing Editor regrets that inadequate copy-editing and proofreading allowed this unwarranted statement to appear in the REVIEW and apologizes to Professor Spiro and the Dorsey Press. . . ." I want to make it clear that I do not doubt the good intentions of the Managing Editor. However, in view of the fact that I refused to retract in that kind of form and of the possible, and I believe unintended, inferences that can be drawn from the wording of the retraction, I consider it essential for the good of the profession and the reputation of the REVIEW that the Managing Editor affirm the following three points in the next issue of the REVIEW. 1. He will not change a review substantively without permission of the writer. 2. He will permit criticism of publishers in reviews. 3. If he had asked me to revise my review, he would not have asked me to retract my criticism of the Dorsey Press but instead would have asked me to revise it in less colloquial language.

One issue—and I believe it will disappear when the Managing Editor replies—is whether pressure from a publisher will produce retractions or deletions before publication. A second involves the responsibility of a publisher of an elementary undergraduate textbook to the thousands of young students who might use it and to the many teachers, who, as is true of all of us, cannot be equally competent in all aspects of the subject matter, for the accuracy of the textbook in the presentation of facts and in the representation of the intellectual positions of other scholars. Accurate representation is essential if the introductory textbook, as is true in this case, undertakes a polemical criticism of other points of view. I concede the author's contention that he received more advice than he used. However, the book did appear under the Dorsey imprimatur and they retain a responsibility regardless of whether they did not engage enough readers, chose their readers unwisely, or merely failed to insist that the author use the good advice that was offered to him. The publisher need not be held responsible for advanced or specialized books that are intended for professionals or for use in class by specialists. Subject matter introductions entail a definite degree of responsibility on the part of the publisher. It is essential in view of this that reviewers be free to call attention to this issue.

MORTON A. KAPLAN
University of Chicago

(EDITOR'S NOTE: In response to Professor Kaplan's first two points, I am happy to reaffirm longstanding policies of the REVIEW: (1) Neither the Book Review Editor nor I have changed or will change a review substantively without permission of the reviewer—as, for example, we did not in Professor Kaplan's review of Professor Spiro's book; (2) both the Book Review Editor and I have permitted and will continue to permit criticism of publishers in reviews—as we did in Professor Kaplan's review; and (3) no pressure from a publisher will produce retractions or deletions before publication—as they did not in Professor Kaplan's review.

His third point, and indeed this whole controversy, turns on one's interpretation of the following statement in his review: "Professor Spiro is not to be blamed for [the inadequacy of the discussion of strategic doctrine], but it is a comment on the Dorsey Press that they *could not afford* proper critical help to the author for a book being offered as an undergraduate text" (issue of June, 1968, p. 694, italics added). The Dorsey Press contended that this statement would be construed to mean that their financial condition did not enable them to pay for proper critical help for Professor Spiro. Professor Kaplan contended, if I understand him rightly, that the phrase "could not afford" was a colloquial usage meaning "did not provide" and was not intended as a literal comment on the Dorsey Press's financial condition.

Had the Book Review Editor (Professor James W. Prothro at that time) or I caught this phrase in our copy-editing or proof-reading, we would have suggested to Professor Kaplan a wording not subject to the construction the Dorsey Press placed on it; but we would *not* have suggested any alteration in Professor Kaplan's substantive judgment that the content of the section concerned seemed evidence of inadequate prepublication review and advice. That, it seems to me, is fair comment for a scholarly reviewer; but a phrasing which suggests that the inadequate advice stemmed from inadequate financial resources is not. However, neither of us caught the phrase, and it was published as Professor Kaplan wrote it.

That being the case, on legal advice I made the apology published in Communications in the December, 1968 issue (p. 1268). I also informed both the Dorsey Press and Professor Kaplan that we would publish any statement by Professor Kaplan acceptable to both him and the Press. Professor Kaplan prepared a statement which was rejected by the Press as inadequate, and so I did not publish it. I then informed the Press that (1) neither I nor the American Political Science Association could or would try to persuade Professor Kaplan to alter his statement; (2) my statement discharged the REVIEW's obligation; and (3) the REVIEW and the Association would not be involved in any subsequent negotiations between the Dorsey Press and Professor Kaplan. That continues to be my position.)

TO THE EDITOR:

What with all the sophisticated, divergent, and often highly abstract approaches to international politics appearing in this journal and elsewhere, this correspondent wishes to report a scheme which is as simple and conclusive as is its promise for general acceptance by the profession. Entitled—tentatively, of course—"The Summer Olympics of 1968: The Index of RP&I in the International System," it goes like this:

I. Problem and Solution: Students of international politics have long sought quantitative indices to determine a nation-state's RP&I ("relative power and influence") in the international system. Several indices have been suggested so far, including military force, GNP, size of population, level of economic development, economic aid, length of foreign broadcasts, and the number of diplomats received or sent abroad. Some have attempted to combine these indices in multivariate analyses. That such indicators or indices—separately or collectively—are unsatisfactory becomes obvious, however, when one postulates that only athletic events in general and the Olympic Games in particular generate world-wide attention, to wit, they are universally conceived as the only reliable indication of power. Accordingly, the systematic analysis of the results of olympic competition will provide a verifiable empirical referent to the measurement of power at the same time as it will narrow our understanding of the subject at hand.

II. Power Performance Quotient (PPQ), or a Word about Methodology: The frequently employed index of medal standing by nation-states was considered unrevealing and unsophisticated. Instead, this correspondent's own method of PPQ ("power performance quotient") was used: it relates NMW ("number of medals won") to the nation-state's population. PPQ, therefore, answers the question: how many people did it take to win a medal? It goes without saying that the smaller the PPQ, the larger the RP&I.

III. Revealing Results:

Ranking	*Nation-State*	*NMW*	*Population** (in millions)	*PPQ* (in thousands)
1	Mongolia	4	.9	230
2	Hungary	32	10	310
3	Denmark	8	5	620
4	Australia	17	11	640
5	E. Germany	25	17	680
6	Kenya	9	7	780
7	Bulgaria	9	8	890
8	Switzerland	5	5	1,000
9	Czechoslovakia	13	14	1,070
10	Finland	4	7	1,220
11	Rumania	15	19	1,260
12	Netherlands	7	11	1,570
13	U.S.	107	179	1,670
14	Poland	18	31	1,720
15	Austria	4	7	1,750
16	Cuba	4	7	1,750
17	Sweden	4	8	2,000
18	W. Germany	25	58	2,320
19	USSR	91	214	2,350
20	Yugoslavia	8	19	2,370
21	Italy	16	50	3,120
22	France	14	47	3,350
23	Canada	5	18	3,600
24	Japan	25	93	3,720
25	Mexico	9	35	3,900
26	Gr. Britain	13	53	4,070

* Based on census figures.

IV. Conclusions and Suggestions for Further Research: The systematic exploitation of our data must be left to others similarly committed to the scientific study of the allocation of power in international politics. Only one important conclusion—the urgent policy implication—will be offered here; to wit, since our data irrefutably point to the supremacy of Mongolia in the international system (with the Mongolian PPQ being ten times that of the USSR), those who have treacherously and irresponsibly promoted the idea that the USSR is the most potent challenger to U.S. power must, from now on, concentrate on the real threat: Mongolian power. In short, it is the imperative task of the new Administration to establish "clear-cut American

superiority" over the Mongolian People's Republic.[1]

As to further research, it is to be hoped that it will be pursued rigorously and systematically to determine once and for all the balance of power in world politics on both the systemic and sub-systemic levels, including the distribution of power between East and West, North and South, and perhaps even between Northeast and Southwest and Southeast and Northwest. Moreover, studies of past Olympic Games could lead researchers to undertake trend analyses. Finally, our modest exploration also indicates the necessity of establishing a data bank on, and an institute for the study of, RP&I and PPQ in world politics. Such an institute could then generate further studies on the problems and symptoms of BS.

Union College and The
University of Kansas
(visiting)

CHARLES GATI

TO THE EDITOR:

Permit me to respond in the REVIEW to the important gap between findings and interpretation offered by Langton and Jennings in the September, 1968 issue. Ken Langton and Kent Jennings are two of the most accomplished students of political socialization, and their large-scale Michigan study ought to have a major impact upon the profession. Therefore, "Political Socialization and the High School Civics Curriculum" raises several questions that are of interest to us all:

1. Why call for "a radical restructuring of these [civics] courses in order for them to have any appreciable pay-off" when it has been concluded that "our findings certainly do not support the thinking of those who look to the civics curriculum in American high schools as even a minor source of political socialization"?

2. Why conclude that the civics curriculum has so little impact, when the reported data show that courses in American Government and Democracy are important sources of knowledge, political efficacy, and civic role for Negro students? After all, haven't the major public efforts in curriculum and educational development been directed toward the black community and its school system?

3. Why consider the policy and value dimensions of "political socialization" (and the curriculum) when the operational definitions are so narrow (taking 0 versus 1 or more civics courses), there are no behavioral dimensions of political learning and participation (I wonder what those seemingly compliant Blacks were doing between classes in the Afro-American Student Association) and the concept and method of political socialization itself smuggles in numerous assumptions about political impact? I would now give one cheer for the impact of Problems in American Democracy on Black students *because* of Langton and Jennings' findings and *despite* their assumptions and methods.

4. Why reinforce conclusions about curriculum irrelevance by noting that selection for higher education may be confounded with socialization in the schools? Of course it may, but the more important point is that socialization (and the curriculum) influences who is selected. Changing the quality and rewards of school socialization are reflected in college admission practices. Enhanced personal and political efficacy among Blacks is likely to increase the number of Black students selected by the University of Michigan.

5. Why stress "information redundancy" rather than politically relevant uses of acquired attitudes and information in accounting for reported racial differences?

I raise these questions in a spirit of concerned colleagueship because the work of Langton and Jennings is of such high quality in professional and behavioral terms. But the terms of Political Socialization concept and method screen out significant public questions about education's impact on political learning and behavior. Robert Hess, David Sears, and I speak to this point independently in the Summer, 1968 *Harvard Educational Review* where Hess raises the issue most pointedly: "Political socialization is a concept which is difficult to apply to the process of political learning during periods of rapid social change or open conflict between major segments of a society. It is of little usefulness in the United States today." Beyond some constructive criticism about a single article the point ought to be taken seriously in deciding what questions are most important in research for the professional knowledge base of the current generation.

The University of Connecticut EDGAR LITT

TO THE EDITOR:

We appreciate the opportunity to reply to the points raised by Professor Litt.

1. The logic of his first point appears to be based on a misreading of our article. For the great majority (90%) of high school students there is no evidence that the civics curriculum has a significant effect. We *did not* conclude,

[1] In this context some significance must be attached to the report that, during a planned tour of Asia, Senator Edward M. Kennedy intends to stop in Ulan Bator.

however, that civics courses could not have an impact, only that it would take ". . . radical restructuring . . . for them to have an appreciable pay-off."

2–3. The importance of civics for the Negro subpopulation was emphasized in our analysis. We do not share, however, the reasoning behind Professor Litt's "cheer" regarding the impact of these courses. Negative relationships between politicization and course exposure among high status Negroes leaves little room for cheer. Their increasingly passive image of their citizenship role is particularly distressing. It should also be emphasized that the vigor of Afro-American movements had not yet penetrated this generation of Black students when the data were gathered (1965). Nor should we delude ourselves concerning the extent to which Negro high school students are even now intensely involved in these movements. Finally, it is not obvious that the major thrust of curriculum reform is directed toward Blacks; such was definitely not true around 1965.

4. Our data do suggest that differences in political orientations between those with high school versus college education—differences usually ascribed to the "education process" at the college level—could be laid to selection rather than collegiate socialization. This does not mean that prior socialization is unimportant in determining who enters college. Increased political efficacy associated with taking civics courses *might* raise the chances of lower status Negroes going to college. But there are two important qualifications. First, civics has no measureable impact on the efficacy level of either high status Negroes or Whites of any status. Second, most studies reveal that familial factors are the prime predictors of who enters college.

5. We do not understand completely the thrust of Professor Litt's fifth point. Information redundancy was stressed because it seemed useful in explaining our findings.

6. As for what comes under the rubric of political socialization, we apparently prefer a broader construction of the term than do some scholars. To suggest that the process stops with the advent of the first urban disorder or student protest strains our sense of what constitutes political socialization.

KENNETH P. LANGTON
M. KENT JENNINGS
University of Michigan

TO THE EDITOR:

Mickiewicz's review of my book, *Soviet Institutions, the Individual and Society* (Boston: Christopher Publishing Co., 1967, co-authored with Irene M. Hulicka) conveys more information about the calibre of the reviewer's scholarship than about the contents of the book. Whereas one might hope that the misinterpretations on which she bases her review reflect only ignorance, there is room for suspicions about lack of professional ethics. A goodly portion of her discontent with our book is based on our "failure" to fulfill claims invented by her. As nearly as I can tell, the claims which she attributes to us are derived from her inaccurate interpretation of advertising material prepared by the publisher. It is, of course, possible that Mickiewicz is ignorant of the fact that authors are not responsible for the advertising statements made by publishers, but it would be unfair to her teachers to assume that she had never been taught that quotations which are attributed to others must be accurate both with respect to source and contents.

Since APSR policy allows a reviewer several thousands words to comment on a book, but allows the author only 400 words to reply to the comments, space limitations prevent me from discussing in detail Mickiewicz's misinterpretations, her amazing lack of differentiation between analysis and speculation, and her naïveté about institutions, events and processes in the Soviet Union. Moreover, her comments suggest that she failed to read the book; from the entire 680 pages she apparently scanned the 103 page chapter on the Communist Party of the Soviet Union, misread the publisher's statements on the cover pages and the authors' statements in the preface and introduction, and read the occasional sentence here and there elsewhere in the book. Had her comments been published in any journal other than the A.P.S. Review, I would not have even bothered to answer them.

KAREL HULICKA
State University of New York at Buffalo

TO THE EDITOR:

Professor Hulicka's intemperate charges are unsupported and essentially unimportant. The major question, it seems to me, centers on the role of the reviewer, or for that matter, of the political scientist. Should an analyst of political affairs describe or reproduce official statements about a political system, or should he impose on that material categories of analysis derived from a theoretical source explaining or purporting to explain a wide variety of data? The official political doctrine of the Soviet Union does claim to provide the theoretical framework for meaningful categories of social and political analysis, but it is open to question whether Soviet

Marxism represents a systematic epistemology or simply a series of ad hoc explanations. In any case, the Western social scientist, unlike his counterpart in the Soviet Union, can openly look to more fruitful, more modern, more comparative theoretical sources. The chief failing of the Hulickas' book is that they have refused to go beyond the rather tautological theory of Soviet Marxism. Professor Hulicka is incorrect when he states above that I have invented claims which must be fulfilled by his book; I have imposed categories of social science analysis, it is true, but they are neither personally invented nor personally directed. They simply reflect the role which Western social science has set for itself. As I state in the conclusion to my review, the Hulickas' book "does present an unusually large collection of official Soviet statements and directives, but in comparison with the many excellent works currently available, it cannot stand as an adequate analytic study of the Soviet political system."

ELLEN MICKIEWICZ
Michigan State University

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BOOK REVIEWS

The Politics of School Desegregation. By Robert L. Crain. (Chicago: Aldine Publishing Company, 1968. Pp. 390. $7.95.)

This insightful analysis of school desegregation in fifteen American cities makes an important contribution to the urban politics literature. The skillful use of statistical methods (primarily rank order correlations) clearly demonstrates the feasibility of combining methodological rigor and sophistication with an interest in the most relevant political issues. The number of cases was small (usually N=7 or 8) because Crain wisely focused on systemic rather than individual attributes in explaining the inter-city variation in acquiescence to desegregation demands. For example, an ideological variable, the civil rights liberalism of the northern school boards, was directly and closely associated with acquiescence. Yet such liberalism was greatest in those cities where the largely reformist, good government—and civil rights oriented—social and civic elite had not moved into the suburbs. In fact, civil rights liberalism was more closely associated with political reformism than with conventional pro-New Deal economic liberalism—suggesting a major role for political theory in explicating the shift in ideological alignments that are now re-shaping the politics of American race relations. Acquiescence was also greater, however, in those northern cities where the low economic status of the entire population, and often a machine tradition, meant limited participation in school politics by anti-integrationist middle income whites.

In the South, the decisive factor was the elites' attitude rather than their power. Like many other southern, but few northern cities, New Orleans had an unmistakable, economically based political elite. But this elite's lack of sympathy for desegregation, and its lack of interest in school politics, permitted a violent, chaotic crisis to develop.

In some ways, school desegregation politics conforms to the pluralist model of interest group conflict, bargaining, and compromise over specific policies. Yet Crain's evidence at least implicitly suggests the presence of a fundamental, hardly pluralist cleavage which is more likely to emerge under some specifiable conditions than others. For example, concessions by the San Francisco school board only encouraged further civil rights militancy. We can hypothesize that, given the city's unusually participant political culture, such gestures encouraged expression of the fundamental but previously often latent interests of black Americans in eliminating pervasive racial inequality. Again, Crain found that across the entire country, the groups concerned with the symbolic or status goals of integration were less militant than some groups articulating the black community's material or welfare goals. This reversal of earlier findings may well reflect recognition by black Americans of their collective but previously latent interest in fundamental social change.

A similar latent interest analysis can be applied to important political structures. Crain and his colleagues suggest socio-psychological variables (rural small town backgrounds and initially low status) in explaining many school superintendents' civil rights conservatism. But desegregation demands may also have manifested the basic professional and bureaucratic interests of school administrations in resisting laymen's efforts to impose social change. Crain and his colleagues similarly suggest that "the professional (often machine) politicians . . . reject politics," (page 177), by resisting desegregation demands, because these politicians share the anti-Negro attitudes of lower status, poorly educated whites. Yet this conservatism may also reflect a patronage organization's interest in avoiding conflicts over fundamental social change lest it upset established patterns of pluralist bargaining and group accommodation. Finally, the relatively acquiescent attitude of many business elites may reflect a basic interest —made manifest both by the threat of civil conflict and by commitment to an entreprenurial ideology favoring economic growth—in preserving a peaceful business climate through concessions on desegregation.

The most problematic aspect of this generally excellent study is its method of data collection, since Crain sent two or three interviewers to each city, usually for one week. Their findings sometimes left important substantive questions unresolved (see page 80 on Baltimore's political culture) and produced few strictly quantitative indicators. Crain and his colleagues therefore tried to estimate the underlying variables directly, and thus avoid the unknown discrepancy between such variables and their indicators. But just because these direct rankings also lacked intensive *detailed* field work, the ambiguities which emerge were only resolved through research team discussions. These collective decisions were thus subject not only to the groups' shared biases as Crain recognizes, but less ob-

vious distortions inherent in group dynamics—which may explain the otherwise inexplicable equating of Baltimore and Pittsburg when the authors' own criteria made Baltimore slightly more acquiescent (page 145).

The book is also somewhat perplexingly organized. The civil rights movement, which did not affect the policy outcomes, is considered entirely separately and might better have been written up only as a journal article, permitting expansion of the insightful, but overly dense and confusing chapter summarizing the northern cities. Two political variables, civil rights liberalism and election versus appointment to northern school boards, together account perfectly for inter-city variation in acquiescence. In turn, these two variables were associated with such other political factors as board cohesion, reform orientation, conflict tolerance and party strength. Yet Crain and his associates finally assert that the very high multiple correlation ($r=.89$) between acquiescence and low suburbanization and low socio-economic status eliminated most of the independent, as opposed to merely intervening, causal importance of all these political variables. But the evidence also indicates that the high correlation is, in part, spurious. In particular, we can explain St. Louis' highly acquiescent response not only because low SES offsets the high suburbanization of its elite, but because of the atypically pro-reform sentiments of its many relatively low status Germans. Again, suburbanization and socio-economic status evidently operate through several political variables in widely varying ways from city to city. Although SES and suburbanization predict Newark's moderate acquiescence correctly, this outcome was actually the result of the city's powerful political machine whose influence obliterated the effect of all other political as well as socio-economic factors.

These difficulties can be avoided by introducing new explanatory variables, which are constructed by the analyst in order to designate behavioral processes, interactions or in some cases "functions." For example, Crain's analysis suggests as such explanatory analytic variables the northern school boards' collective ideology and its independence from anti-integrationist pressures. The "concrete," largely static or structural attributes in Crain's analysis such as suburbanization, socio-economic status and machine strength and percent German in fact contributed to the boards' ideology and independence, but in different amounts in different cities. Introducing such analytic variables might well have eliminated both the unexplained variance and partly spurious relationships among their northern cities. This step would have also specified more precisely the casual role of the political variables which Crain had "left over" when suburbanization and SES were introduced.

The same procedure would also have permitted a single analysis of northern and southern cities. Of course, desegregation in the north, where the board was crucial, did not involve court litigation as in the south where the civic elite was decisive. Yet we would like to know, at least, if acquiescence within each region by itself had similar correlates, so that the most acquiescent cities in each region differed in the same ways, on the same explanatory dimensions, from the least acquiescent cities. To be sure, the most acquiescent cities in the North were low in status while the most acquiescent cities in the South were high. Nevertheless, as Crain and his colleagues suggestively observe, a pro-civil rights predisposition by the civic-economic elite was fairly wide-spread in the North, but its resources for influencing the school board in pluralistic local politics varied markedly with low suburbanization and low socio-economic status. In the South, the socio-economic elite usually had the necessary resources, but was likely to use them on behalf of desegregation only if—primarily in high status cities—it had appropriate traditions and values (page 360). In terms of analytically specified processes, the northern school boards and southern civic-economic elites were thus *behaviorally equivalent actors*, just as high status in the north and low status in the south were *behaviorally equivalent variables*. Acquiescence, therefore, depended in both regions on the independent position and pro-desegregation attitudes of the relevant elite. The low status of the entire population in northern cities, and high status in southern cities, were both correlated with acquiescence, because each of them was associated with the strength of the relevant elite and its pro-desegregation attitudes.

This reformulated proposition is neither tautological nor obvious. Logically, the concept of school desegregation does not specify an overwhelmingly decisive role for local elite attitudes and influence. Empirically, these two factors are much less important on such similar local issues as the War on Poverty or trade union influence. Of course, this proposition on elite attitudes and influence can only be tentative. The reliable specification of analytic processes requires that we identify those variables which have the same causal impact—and thus mean the same—to actors in different contexts. Even scholars trained in the comparative analysis of politics, specification requires m

than that provided by three men doing a week's research. Since *The Politics of School Desegregation* is distinguished through out by admirable care as well as important insights, the author's failure to employ the analytic variables implicitly suggested by their own findings may well reflect a scholarly respect for the limitations of their data.

DAVID GREENSTONE
University of Chicago

Political Development in Modern Japan. EDITED BY ROBERT E. WARD. (Princeton: Princeton University Press, 1967. Pp. 637. $12.50.)

This large book is the fourth in a series of six large books dealing with the modernization of Japan. The series grows out of a special project of the Association of Asian Studies, financed by the Ford Foundation and the Carnegie Corporation, and is published by the enterprising Princeton University Press. Earlier volumes have dealt with concepts and theories of Japanese modernization (*Changing Japanese Attitudes Toward Modernization,* Marius B. Jansen, ed.), Japan's economic development and its political context and ramifications (*The State and Economic Enterprise in Japan,* William W. Lockwood, ed.), and with *Aspects of Social Change in Modern Japan* (R. P. Dore, ed.). Two more volumes will deal with cultural change and with dilemmas of growth in prewar Japan. Thus, like the "modern" society which is being analyzed, the present volume is a specialized and differentiated function of a well-financed bureaucratic structure, and must be appreciated on such terms.

Aside from the editor's marvelously succinct and thoughtful introduction and epilogue, which define modernization and summarize the substantive chapters in fourteen propositions, *Political Development in Modern Japan* is organized around seven factors which are presumably central in any process of political development. Professors Hall and Hackett deal perceptively and informatively with leadership: the institution of the emperor, and the Meiji *genro* (the remarkable group of oligarchs who "ran" Japan in her early days of constitutional government). Professor Craig analyzes the cultural content of late-19th century nationalism on the basis of a thorough examination of the interesting evolution of the thinking of Fukuzawa Yukichi, an outstanding philosopher and educator of the [illegible]. Professors Jansen and Ike deal, respectively, with Japanese foreign policy and military aspects of the process of modernization. Jansen suggests that several "diplomatic" problems are integral parts of a nation's political development. Professors Steiner and Scalapino describe the halting evolution of popular political participation at the rural local government and the national electoral levels, respectively; Scalapino's study also includes the only treatment of political parties in the volume. Professors Ishida and Silberman take on the concept of differentiation: Ishida deals with the structural and behavioral characteristics of Japanese interest groups (emphasizing their weak organization and lack of autonomy), while Silberman presents a quantitative analysis of the increase in the administrative machinery and tasks performed at the prefectural level of government. Professors Henderson and Tsuji describe aspects of the secularization and rationalization of Japanese law and bureaucratic decision-making; Henderson's chapter is a breath-taking panoramic view of the evolution of Japanese law over the past 125 years. And, finally, professors Ward and Burks analyze the importance of leadership decisions and planning in modernization; Ward's provocative study deals with the postwar Occupation as an "experiment" in planned political change.

The volume is difficult to describe. On the whole it is a comprehensive and competent survey of the subject, and should be very useful as such. Most of the studies focus on events of the early years of modern Japan before the First World War; only Steiner (who contrasts local government in the 1880's, 1930's, and postwar), Ishida, and Henderson attempt to deal with the sweep of developments over the past hundred years. The authors are historians, political scientists, and a lawyer. In general, the political scientists present their studies in connection with a theory of modernization (evolution of political culture in the Almond-Verba sense, mobilization regimes, differentiation, guided democracy, the relative importance of socio-economic versus political factors, etc.), while the historians attempt to resolve issues which have vexed students of Japan (how much and in what ways did the pre-modern, feudal system influence later developments?, "what went wrong" with modernizing Japan to lead her down the path of militarism?, what was the impact of the West?, etc.), but some authors do both. Most of the authors have already published lengthy monographs on the topics of their chapters; these chapters have the quality of a "summary and conclusions" (which can be very helpful for students and for scholars in related fields looking for an overview), and so are occasionally unpersuasive without the omitted documentation. The chapters share an interest in things Japanese

which other scholars of comparative politics may have a hard time matching. Nevertheless, because of the organization of the book, the competence of the authors, and the editor's opening and conclusion, the volume does deal with several important general questions. Most interesting, to me anyway, are the issues of "conservative modernization," of the preconditions for modernization, and of the desirability of "modernization."

Most of the authors and the editor are agreed that the modernization of Japan in the Meiji period (1868–1912) was conservative in the root sense of the word. *Given* that Japan was to become economically and militarily strong and politically sovereign and secure, existing institutions, customs, and social class relations were to be changed as little as possible. Traditional structures and ways of thinking were to be renovated: maintained, but altered so as to be useful in the pursuit of the leaders' economic, military, and political goals. That is, the strategy of the Meiji leaders was to modernize while conserving as much as possible. Further, although the authors do not explicitly say so, the *goals* as well as the strategy of Meiji modernization can be seen as conservative—in contrast, anyway, with the goals of 20th century social revolutions. (Ward's analysis of postwar democratization provides a helpful contrast. The Occupation, he says, when it was successful, pursued a strategy of "the creation of new vested interests." The Meiji elite, on the other hand, could pursue a strategy of satisfying existing vested interests as much as possible, since their destruction was not desired nor usually required.) The results of this conservative modernization were national integration, stability, continuity of policy, and of course success. (The four chapters on leadership and participation make this point clearly, as does the editor's epilogue.) Furthermore, many of these authors imply, Meiji conservative modernization did *not* necessarily lead to the "fascism' and militarism of 1931–1945. To the contrary: there was "substantial change and liberalization between 1890 and 1932;" the problem lay, rather, in the fact that "Social institutions or forces that are constructive and functional for one set of circumstances, stage of development or level of input may be destructive and dysfunctional for the modernization process in others." (Ward, pp. 588, 586.) But there is neither agreement upon nor much sustained consideration of this fundamental point. Steiner and Scalapino document the extremely limited nature of popular participation in Meiji modernization, but avoid getting involved in questions of historical inevitability. Ishida does attempt "a statement of the nature of the pattern of modernization in Japan that can explain the whole historical development of Japan, including the Meiji period and the relatively liberal and militarist stages that followed," and Ike ("war represented an integral part of Japan's modernization process") and Henderson ("in the Meiji legal order . . . law became the implement of, but not a limitation upon, legislative power;" various "factors prevented the realization of a justiciable rule-by-law in the administrative field, except to a token degree;" the private law codes "often perpetuated old power relationships in the name of the codes' contractual, personal, or property rights") do see in Meiji Japan some of the seeds of the later militarism and "fascism." The other authors either avoid the problem or implicitly accept Ward's formulation. It is too bad that this massive volume treats this important question so casually.

All the authors who consider the question of the preconditions for modernization are agreed that the process was significantly shaped by conditions in the pre-modern, feudal period before 1868. Hall on the emperor, Craig on nationalism, Steiner on rural political culture, and Henderson on law are explicit about the need to consider arrangements and developments in the earlier period. The arguments given are persuasive. But I question whether anything has been shown about the *preconditions* for or the *beginnings* of modernization. Recall the Japanese who went to Europe and the United States in the years soon after Perry, who sometimes thought that almost everything they saw was an integral part of Western modernity and strength. Only partly facetiously I would suggest that some of the authors of this volume have gone to Meiji Japan and have assumed that almost everything they saw there was part of Japan's modernization. Beginning in this way, they can easily argue that Japan's modernization was "a cumulative product of two and a half to six or seven centuries of gradual preparation." (Ward, 580.) But to show that pre-1868 Japan had a well-worked-out "legal" system of "rule-by-status" which significantly influenced (retarded) later attempts to institute a new, modern legal system (which Henderson shows) does not allow the conclusion that the pre-modern system was a precondition or the origin of the modern system (and Henderson does not make this mistake). Nor does the existence of a traditional institution like the emperor which can be [illegible] and used in pursuit of modernization [illegible] that institution was a precondition [illegible] the origin of modernization. Just [illegible]

was in the kitchen while dinner was being cooked should not lead one to conclude that the plate was a precondition or a part of the origin of the dinner, even if the dinner is served on the plate! The problem, of course, is the lack of a theory of the modernization process—a lack which is hardly to be blamed upon these authors.

Finally, the authors raise by their implicit disagreements the question of whether modernization is a desirable thing or not. They are all very careful not to sneak ethnocentric assumptions about the goals of modernization into their analyses. So careful, in fact, that for some their "modernization" becomes a Frankenstein which I wonder whether they value or desire for their own country. (*Vide* Ward: " 'any perfectly modern political system' . . . would obviously be an intolerable monstrosity by any humane standards.") The treatment of popular political participation illustrates this tendency. Once scholars thought of popular participation through elections and self-government as a (or, even, the) basic component of political development. By now, participation has come to mean "involvement with the political system, though not necessarily with its decision-making aspects" (Ward, 7), and trends in bureaucratic control of the countryside and in the electoral fortunes of admittedly elitist parties, described by Steiner and Scalapino, are called "political participation." Development (which is not distinguished in this volume from modernization) has no theoretical link with democracy, nor with progress in any normative sense. In all this, the present volume seems to be in the main-stream of American social scientists' thinking about modernization. The lawyer Henderson is a refreshing and instructive change. His unabashedly normative conception of legal development permits him to identify clearly those concrete changes which are, and are not, "modernization." Social scientists could find a lesson here. It may be more fruitful to use admittedly *a priori*, i.e., normative, models of modernization or development than to continue with putatively empirically-derived definitions. (Such a switch in theory-building strategy might, for example, take care of some of the problems of analyzing "conservative modernization" and "preconditions" noted above.)

This is a stimulating and useful book. It should be particularly helpful for scholars who an up-to-date survey of knowledge about events in Japan in the Meiji period, dents of Japanese history and politi- ment, but it also contains several h should interest the specialist. In conjunction with the other volumes of the "Studies in the Modernization of Japan" series, it represents a massive contribution to our knowledge about Japan and about processes of historical change.

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Counterrevolution: How Revolutions Die, By JAMES H. MEISEL. (New York: Atherton Press, 1966. Pp. 237. $6.50) and *The Problem of Restoration.* By ROBERT A. KANN. (Berkeley: University of California Press, 1968. Pp. 441. $15.00.)

Revolution, some contend, is a term politics has borrowed from astronomy. Political life, like the recurring motions of the planets, seemed, to Renaissance man, to follow a determined cycle in which change was inevitably followed by a return, a restoration, to the starting point. This original connotation holds quite well for the English Revolution of 1688, but as we all know this all changes in the eighteenth century and the concept takes on a different meaning. Revolution comes to connote an abrupt break with continuity, indeed a dramatic failure to maintain inevitable patterns. In light of the earlier meaning, however, it is very fitting that the many recent books and articles on revolution and internal war should now be followed by two books on restoration and counterrevolution. Because of their appearance we now know that it is just as difficult to write comparative analysis of reaction as it is of revolution.

No two books on the same subject could differ more than James Meisel's *Counterrevolution* and Robert Kann's *The Problem of Restoration.* More at variance than their substantive positions are the styles of the two books. Professor Meisel's book is a series of rambling reflections, informal, intuitive, generally quite brilliant and self-consciously casual. Little attention is given to a rigorous argument developed and sustained throughout the book. In contrast to this Professor Kann's book is sober, formal, and elaborately reasoned. Anything but casual, his is the traditional method of researched inquiry. Still the books complement each other, and despite their different approaches they are both interesting and important.

Professor Meisel's book is in no sense an investigation of counterrevolution. Neither history nor hypothetical political science, it is simply a collection of brilliant essays (many of which have been published before, which may account for the unstructured quality of the book) on subjects which and people who fascinate the author. These essays are on diverse topics: the

Hebrew prophets, Cicero, General Monk, L'Abbé Sieyès, Louis Bonaparte, Freud, Weber, Durkheim, Pareto, Lenin, Trotsky, Mussolini, Hitler, T. E. Lawrence, Franco and de Gaulle. Behind the innovative features of the book's style (dialogues, memoranda, testimonials, journalistic narratives, personal reminiscences) there are subtle evaluations and speculations about many important events or fascinating individuals. There is, however, little if any detailed investigation of the title subject, counterrevolution. But the reader should not really expect this, for he is told early on that it is to be a book about people, second-raters, who tend to be around when revolutions end.

Despite the risk of giving too much unity to this diverse collection of essays, I think a case for calling this a book about counterrevolutions can be made, since the essays tend to be about one or more of five possible forms of counterrevolution. The first is the familiar variety which Meisel labels "devolution." General Monk, Napoleon, Louis Bonaparte, and Franco are the agents of "devolution." Meisel goes one better than Brinton for whom the end of revolution was the decline of fever and the return of the body politic to reasonable health. For Meisel, revolutions like "tautly wound cable" must literally devolute, roll back from "strains which have become unendurable." A second form of counterrevolution he labels "intercessions," which are "preventive counterrevolutions." There are two varieties of intercessions, bad ones—Mussolini and Hitler—and less bad ones —General de Gaulle. A third species of counterrevolution found in the book is intellectual, that of Freud, Weber, Durkheim and Pareto against Marx. A fourth is existential and personal, thus an essay on T. E. Lawrence. The final variety of counterrevolution one can find is doctrinal and thus the rationale for discussing Stalin and Lenin, and the Old Testament prophets and the modernizing Hebrew Kings.

This unity is imposed solely at the discretion of the reader, for little care is given by the author to structured argumentation. A fascinating book, full of meaty chunks of contentious speculation, *Counterrevolution* is by no means, however, a contribution to the comparative study of either revolution or counterrevolution. Indeed, it was not intended as such.

This cannot be said of Professor Kann's book. Its purpose is to generalize about and find uniformities in the recurring historical phenomenon of restoration. The scope of his inquiry is staggering, revealing a writer learned, judicious and wise. After some one hundred introductory pages of general methodological and theoretical speculation on such interesting topics as time, change, determinism, tradition, continuity, legitimacy, force and violence, he turns to a detailed study of what he calls "the cases." From antiquity he discusses the restoration of the Jewish homeland after the Babylonian exile, the restoration of Greek "democracy" after the rule of the Peisistratidae and the restoration under Marius to pre-Gracchan conditions. A second set of "cases" includes investigation of Justinian's reconquest of Italy, Otto the Great's restoration of Charlemagne's empire, Henry II of England's restoration of the Norman centralized monarchy, Rudolph of Hapsburg's restoration of lost Hohenstaufen imperial authority, and finally the Counter Reformation of late sixteenth century and early seventeenth century Austria. The final category of "cases" consists of the two "classical" restorations, Stuart England and Bourbon France. Each study is preceded by an historical chronology and periodization of these dates into three stages, "the Original System," which he shorthands as (A), "the Intermediate System," (B), and "the Restored System," (C). Each case studied is abstracted to fit this three-fold categorization.

In the theoretical introduction Kann introduces one major hypothesis which he quite successfully sustains in the case studies. "The reestablishment of a system must come about within the life span of a political generation and its endurance must be tested within another one." He also contends that the cases studied establish a "guideline" to help in predicting the success of restorations. They succeed only if they absorb a good part of the ideas and institutions of the intermediate system: (C) must thus be a synthesis of (A) and (B). Restorations reproduce anciens règimes only if along the way they accept some of the innovations of the intervening revolution. The usefulness of this "guideline," he holds, is seen in comparing the French effort at restoration in 1815 with the English efforts of 1660 and 1688. With this few can quibble. But can this be extended beyond the "classical" cases to all the others he has cited? In what real sense can one even compare these "cases"? Some, like the Jewish and the Justinian are territorial restorations; some, like the Roman, the French, and the English, are social and political; some, like the Austrian, are intellectual and theological restorations; some, like the Ottonian, the Henrican and the Hapsburg are restorations of strong central monarchs after crises of succession. Here is the heart of the problem. Is comparative analysis of restorations any easier than of revolutions? Is it not even clearer here that one is comparing apples, and pears,

and plums, and that no amount of abstraction by which all the differing fruits are seemingly reduced to some common parts (A), (B), and (C) will make the task any easier to realize?

But Professor Kann is too wise a man, and his book too learned and reasoned a production to be unaware of these problems. His search for hypothetical validation and "guidelines" for prediction is tempered by a modesty surprising in one pioneering a subject, a self-restraint which time and time again finds him cautioning the reader that no social or historical laws are being chartered, not even probabilities, but simply "the establishment of certain analogies common to a number of well-selected cases." Because this motivation operates and because Professor Kann's vast erudition is enlisted to serve that modest end, *The Problem of Restoration* is a truly important and valuable contribution to the study of comparative political history.

ISAAC KRAMNICK

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Inside Bureaucracy. By ANTHONY DOWNS. (Boston: Little, Brown, Inc., 1967. Pp. 292.)

The author set out to "develop a useful theory of bureaucratic decisionmaking"—useful in that it "should enable analysts to predict at least some aspects of bureau behavior accurately, and to incorporate bureaus into a more generalized theory of social decisionmaking. . . ." Bureau, as used by Downs, is a large organization, with full time employees comprising the majority of the membership. Bureaus operate in a non-market environment (output cannot be "objectively" evaluated) and their members are hired, fired, and promoted on the basis of their performance in the organization.

The "theory" is allegedly based on and the majority of conclusions are allegedly derivable from three basic "axioms" or "postulates": (1) bureaucratic officials are intendedly rational. "(A)ll agents in our theory are utility maximizers," given their limited computational abilities and the cost of information. (2) bureaucratic officials have a complex set of goals, some of which can be referred to as "self-interest," "(E)very official is significantly motivated by his own self-interest even when acting in a purely official capacity." (3) "Every organization's social functions strongly influence its internal structure and behavior, and vice versa."

The "theory" consists of the relationships between the following major types of variables: "ten internal dimensions of bureaus (communication system, information-gathering system, distribution of formal and informal authority, resource allocation system, purpose-and-function definition process, methods of and attitudes toward innovation, internal separation of functions, stability, methods of interacting with external environment, and mixture of types of officials), eight structural aspects of bureaus' social functions (clarity with which functions can be defined, ease of calculating bureau effectiveness and results, stability of internal technological environment, etc.), five specific types of bureaucrats ("purely self-interested employees": climbers and conservers, and "mixed-motive officials": zealots, advocates, and statesmen); and certain other normal charactersitics of organizations' and officials' behavior." The book is really about bureaucratic structure and contains relatively little that is unique about bureaucratic decisionmaking.

Downs has succeeded in cataloging much of the folklore of government bureaucracy (e.g., "*Law of Increasing Conservatism.* All organizations tend to become more conservative as they grow older, unless they experience periods of very rapid growth or internal turnover.") and in listing many of the tricks-of-the-trade utilized by Washington, D.C., bureaucrats (e.g., "Successful high-level officials develop whole informal networks of outside information sources through which they can verify the reports made to them by their subordinates.") Because of this, *Inside Bureaucracy* is a valuable contribution to the literature and its insights will be of substantial interest to scholars and students or organization theory.

The sections dealing with the life cycles and growth of bureaus are a mixtue of notions from Weberian sociologists (routinization-of-charisma accounts for birth of bureaus, organization structure reflects division of labor, etc.) and Parkinson ("Work—and bureaus—expands so as to fill the time available for its completion.") as are those parts of dealing with the internal structure of the bureau. The section on the inevitability and necessity of bureaus and other non-market organizations in society presents a lucid, welfare-economic discussion of the appropriate role of government in a market economy. Discussions of decision biases, communications, control processes, hierarchies, organizational boundaries, and bureaucrats' ideologies are a vague mixture of Downs' three basic "axioms" loosely woven in with notions found elsewhere in the literature. The interesting description of organizational search and change is very similar (with explicit consideration of the costs of search and information) to those found in March and Simon's *Organizations* and Cyert and March's *Behavioral Theory of the Firm.*

The major shortcoming of the book lies in the

disparity between its theoretical "ambitions" and its actual accomplishments. Although it *might* be true that *some* of Downs' conclusions can be derived from his three "axioms," he does not even approximately demonstrate that. The author's condescending style and methods of reasoning are also sources of some irritation. When coupled with his penchant for coining phrases ("shrinking violet and superman syndromes," "straight-scoop by-pass," etc.) and for creating "Laws" out of whole cloth, a work that might have been described as "creative insight" comes dangerously close to being pompous. E.g.,

> The bureau "customers" of the central search agency will have an ambivalent attitude toward it. They will ask it to furnish all information of any positive value, regardless of cost, since they do not have to pay for it. This conclusion has the following implications. First, no matter how large a data gathering and handling capacity the central search agency possesses, its facilities will always be overloaded. This results from the Law of Free Goods: *Requests for free services always rise to meet the capacity of the producing agency.*
>
> Second, officials of the central search agency will develop nonpecuniary prices for their services. These are devices for imposing costs upon members of other bureaus who request information. They will be designed both to discourage requests and to provide rewards to central search agency members. Such "quasi prices" will include demands for reciprocal favors, long delays, and frustrating barriers of red tape. This illustrates the Law of Non-Money Pricing: *Organizations that cannot charge money for their services must develop non-monetary costs to impose on their clients as a means of rationing their outputs.* Hence, much of the irritating behavior of bureaucrats often represents a necessary means of rationing their limited resources....
>
> Third....

Throughout the book, the "Laws" appear almost magically, not following directly from the discussion, from the original "axioms," nor from any presented empirical evidence. *Inside Bureaucracy,* contrary to the author's claims, does not represent a theory in any sense of the word.

As can be seen in the above example, the book contains strong doses of C. Northcote Parkinson. Unfortunately, Downs' first-second-third-hence-therefore style prevents him from being as entertaining. We are forced to take both his conclusions and his methods of arriving at them seriously.

The author's previous book, *An Economic Theory of Democracy,* was a relatively tightly-reasoned, original, and frequently brilliant piece of work. In it, Downs demonstrated the efficacy of a few axioms and a relatively simple theory involving few variables. In this book he introduces a hopeless number of variables and fails to utilize his "axioms" in any meaningful way. At the end of the highly-successful *Economic Theory of Democracy,* Downs listed a set of "testable propositions derived from the theory." He saw fit to repeat the practice in this book. In the *Economic Theory of Democracy,* it took considerable effort to discover that the propositions were neither testable nor derived from the theory. In this book there is no theory and the non-testability of the propositions is immediately obvious. If nothing else Downs has demonstrated how essential it is to empirically test theory and to use operational concepts in theory construction.

In spite of the book's considerable shortcomings, it also possesses condierable virtues. Foremost among them is the catalytic effect *Inside Bureaucracy* will have on its readers and on their research. The nineteen pages of axioms, laws, and propositions are packed with interesting and significant research questions and bureaucratic folklore. The space devoted by the reviewer to the virtues is by no means a measure of their relative importance.

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Political Parties in Western Democracies. By LEON D. EPSTEIN. (New York: Frederick A. Praeger, 1967. Pp. 274. $8.50.)

Leon Epstein's *Political Parties in the Western Democracies* will almost certainly become a standard element of the literature on political parties. It is a book informed by long and very cautious thought, and by a very thorough canvass of the existing literature. It is a book which will revise the received wisdom at several points, though not a book which will altogether reorder the way we think about party politics. It is certainly a book all of us should own and read.

In many ways, Epstein's book stands Maurice Duverger's very well-known work on its head. For Duverger, politics turns on large determinicies; for Epstein, politics hinges on a collection of not so grand contingencies. For Duverger, parties develop toward an idealization of mid-Century continental working-class parties; for Epstein, they seem currently to be progressing (or regressing) irregularly toward the model offered by American middle-class parties. Perhaps the difference is between a European and an American scholar; perhaps it is the difference between a rationalist and an empiricist. In ei-

ther case, Epstein quite consciously takes aim on the Gospel according to Maurice.

A central example is offered by the question of changes in party organization. Duverger thought he saw a determinate progression from brackward cadre-based parties to advanced mass-based parties. He went on, of course, to coat this progression with a naturalistic sense of goodness, making the mass-party appear "advanced", the cadre-party "backward". It followed that middle-class cadre-parties would learn to imitate the working-class mass-parties, or face extinction—a process which he called "contagion from the left".

Epstein argues that Duverger mistook an admittedly important set of early 20th Century contingencies in European history—class-consciousness and socialist unionism, prebroadcast technology, and rapid extension of mass suffrage —for a universal determinacy. Since one cannot expect these contingencies to retain a single exact configuration, one cannot expect the alleged determinacy to persist.

Indeed, Epstein puts forth the thesis that a new series of contingencies—centering about television and its depressive effect on the importance of volunteered labor and mass dues financing—may have set an opposite (though perhaps temporary) trend in motion. Now, expertise in the use of media and big money are relatively more important than mass canvassing and masses of small contributions. The result of this reversal, according to Epstein's argument, is "contagion from the right." Parties which Duverger thought backward—especially the major American parties—have learned to use the medium earlier and more effectively than Duverger's advanced mass parties, and the latter are just beginning to imitate these backward cadre parties as they themselves are forced to rely increasingly on television, and not on activists, to reach the electorate. The upshot of the argument, of course, is that Duverger's linear theory of party evolution is reduced to a quite ordinary, partly incorrect hypothesis. It is not so much that Epstein has proposed a revolutionary new "theory," but that he has revised and scaled down an old hypothesis.

It would, indeed, be very wrong to think of Epstein's book as a fundamental revision of the field. It does, as we have seen, often arrive at new conclusions. And it does, as I have already said, demonstrate a special and attractive approach to party politics. But its new conclusions follow from established questions, and its approach is well within the confines of conventional political science. Epstein takes strong (and insightful) exception to much within the conventional literature, but does not take exception to the categories which organize it and the questions which move it.

A major portion of the essay, for example, is organized by a very informal, very eclectic version of functionalism. Functions, here, are both things parties do for political systems and payoffs parties provide for their clients. The former (macro-) strand of functionalism is not in Epstein's (or any other) hands a powerful explanatory device. The later (micro-) strand of functionalism is potentially more interesting, but would have profited from a closer consideration of the equilibrium concepts proposed by Chester Barnard and Herbert Simon some time ago. There is nothing theoretically new in the use Epstein makes of functionalism.

The other major set of organizing concepts are drawn from democratic theory, and these are to be recommended at least for their straight-forwardness. Epstein divides the democratic norms which scholars have brought to the study of parties into three groups—majoritarian, pluralist, and individualist versions of democracy in large communities. These are helpful, for they enable Epstein to avoid the fruitless question which plagues a large sector of this and related fields—"What does 'democracy' *really* mean?" If the book does not remake the place of parties in democratic theory, it is at least unusually clear about the place others have given them.

A major virtue of the book is its serious attempt at genuine comparison. Despite what Epstein quite frankly calls his "Americanist bias," the book is organized by the theoretical categories we have been discussing, not by the units of political geography. The U.S. (and Great Britain) may receive disproportionate attention, but this imbalance cannot be blamed on Epstein. And, even if the theoretical categories are not revolutionary, they are at least lucid. And they offer greater coherence by far than do the country-by-country patterns of organization so common in the literature on parties.

And Epstein must be praised for the unpretentious, lucid style in which he writes. Keeping in mind the many garbled definitions that have been offered for the term 'political party,' Epstein's cannot fail to refresh the reader:

> It is time to define what is meant by political party. I have no desire to depart from common understanding. . . . [A party is] any group, however loosely organized, seeking to elect governmental office-holders under a given label. Having a label (which may or may not be on the ballot) rather than an organization is the crucial defining element. (p. 8).

We may find this definition useful for some purposes and not for others, but at least we can un-

derstand it, and so can our students. This straight-forwardness is characteristic of the entire book.

This frank reliance on common prose does, however, associate itself in many books, including this one, with a failure to use statistical and mathematical approaches where they are needed. Many problems in party politics are most effectively (and intelligibly) met with mathematical or statistical thought. But Epstein's book uses very little beyond the presentation of tabular data. Discussions involving the character of inter-party competition, for example, are not at their best where no numerical (much less mathematical) analysis is offered. It is my impression that this omission does not so much misguide Epstein's analysis as it limits the range of questions open to his attention.

Seen in the larger perspective, then, this is a fine and often sophisticated book. It does not offer a sort of post-Duverger renaissance to the study of party politics, but it does offer important revisions within a continuing tradition. It is insightful and pointed. It is well-written and unpretentious. It is, in language as direct as Epsteins own, a good book.

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A Great Society? EDITED BY BERTRAM M. GROSS. (New York: Basic Books, 1968. Pp. 362. $8.50.)

Few Americans, it seems safe to say, regard seriously the tags which Presidents fashion for their administrations. Often these tags are as much the choice of the press as of their formulators and are notably inept, The New Deal being a prime example. Just what President Johnson had in mind in his choice of label is far from clear, but Bertram M. Gross has paid him a notable compliment in taking that choice seriously and making it the title of a book of essays by a distinguished group of scholars. Professor Gross and some of the contributors make efforts to define the phrase, but they are more successful in discovering a variety of antecedents than in reaching agreement on a single meaning. Understandably, some of the best essays simply ignore the problem.

There is something rather mournful about the appearance of such a book when the term's progenitor has withdrawn from the partisan battlefield and whatever dream he had has dissipated. But there are other ironies in the venture as well. While the title in one of its plain language meanings would suggest a *large* society, the apparent theme of the editor and some of his contributors is a call for decentralization and return to a world of small societies. This call is further associated with a doctrinal affirmation of the pluralism paying tribute to the name of Arthur Bentley. Certainly Bentley contributed to the breakout of political science from its one-time formalistic confines, but that battle ended long ago, and the critical movement of the present is a similar escape of the discipline from the narrow confines of the pluralism which has been the orthodoxy of the last few decades.

If the book as a whole suffers from the dilemma of being too late to give guidance to a one-time powerful administration and too early to be able to assess that administration objectively and if its intended theme looks backwards, it is yet a highly honest and useful volume. The editor is far from insensitive to the discriminatory effects of the pluralism he idealizes and some of the contributions in the book contain sharp criticisms of that doctrine's implications.

The most incisive of these criticisms is the article by Norton Long, "Local and Private Initiative in the Great Society." With his customary keen sense for the relevant, he looks to the problem of the cities in American life. Here, the idealization of small units is absurdly at odds with the goals of greater equality and decency of life in the settings in which most Americans now live. This is as true in the fragmented governmental units of suburbia with, in his wonderful term, its "toy governments," as it is in the inner city where the neighborhood is being told its salvation lies in its own hands. Autonomous "self-government" and "participatory democracy" are, as always, OK words and they evoke a pleasing picture of a New England town meeting, but neither suburban "village" nor ghetto neighborhood is an island and it is time that both realized they are part of the main. The cold hard facts of tax resources, if nothing else, should be proof of that reality. Although Professor Long only hints at it, the suspicion seems justified that some of the ready acceptance of these ideas of autonomy and self-help rests at least partly on an unacknowledged understanding that these are the keys to maintenance of discrimination and privilege.

Although the book opens with a sympathetic presentation of President Johnson's own reflections and desires for the Great Society, it is notable that it also contains statements by perhaps his angriest critics, Herbert Marcuse and Hans J. Morgenthau. These articles are highly convenient summations of their positions, familiar as these have become. To Professor Marcuse the Johnsonian program is ambiguous, but also evil, being built on such American values as "commercialization of the soul, togetherness, the sanctity of business, the science of human rela-

tions." In its warlike neo-colonialism and general crassness, the America of President Johnson gives no chance for individualism or any of the decencies of a *real* Great Society. Professor Morgenthau says briefly what he has repeated with marked effect elsewhere: that the President has mistakenly and dangerously sought consensus where he is only entitled to consent, and the latter presumably only for different policies than he has followed, particularly in foreign affairs.

Some of the most interesting passages in the book are the assessments of what changes are overtaking American government and politics. Thus Daniel Bell in a discussion deceptively presented as a collection of notes argues that the vision of American politics descending from Bentley through David Truman and others is "astonishingly out of date for an understanding of politics in the second half of the twentieth century," failing to take account of the decisive charactersitics of policy today: "the influence of foreign policy, the 'future orientation' of society, and the increasing role of 'technical decision making'." He notes the blurring of the line between public and private organizations and finds great significance in the cooperation exemplified by RAND and its imitators.

Peter Drucker also perceives a deep change in American politics. To him the old economic issues of politics and interest group contests and the consequent patterns of accommodation are becoming obsolete. Economic policy, he claims, has now become a technology rather than a philosophy and domestic politics seem "likely to shift to a concern with the basically political—that is, with constitutional, moral and esthetic issues," with a resulting new preoccupation with power. There follow, he says, a passing of Southern political power, an erosion of economic blocs, reduction of Congress to the status of critic and commentator, and trouble for the parties, especially the Republican. With Professor Bell he stresses the interlaced character of private and public organizations but goes a step further to see in the commingling of corporations, labor unions and governmental bureaus a "new federalism." In a broad sense this is not too different from the "military-industrial complex" so troubling to radicals. Professor Drucker, it may be said, is bravely undismayed by his discovery.

The book, then, is an accurate and able reflection of a considerable part of the spectrum of social and political discussion occurring in the contemporary intellectual and semi-popular press. Despite some editorial effort to eulogize Arthur Bentley and to press the case for the sort of federalism variously seen as "new" or "creative," there is ample statement of the difficulties in maintaining such retrospective visions. Despite the misfortune of its title, it will be a useful volume to have available through the uncertain years in which we seem to be midpassage.

GRANT MCCONNELL
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Law Without Sanctions: Order in Primitive Societies and the World Community. By MICHAEL BARKUN. (New Haven: Yale University Press, 1968. Pp. 179. $6.50.)

Michael Barkun's book is a welcome contribution to those scholars who distinguish between training in law and the study of law, and who are concerned with the worth of the latter in advancing legal scholarship. This is a suggestive book, pregnant with ideas and propositions, constituting a firm step toward the scientific study of a field that has for many years been the province of practitioners. Barkun undertakes an attempt to overthrow, or at least to lessen the inverse relationship between the existence of a developed profession and the development of pure scholarship in the substantive area of law. It is a fruitful undertaking even though it does not come up with all of the answers, or even perhaps with any of the ultimate answers to the long-standing problems of legal theory that are the legacy of John Austin. As a germinal book in legal theory it poses some very intriguing propositions and suggests some very seductive paths of inquiry in developing the scholarly area of the study of law as an important segment of human experience which crosses the disciplinary lines of jurisprudence, political science, sociology, and anthropology.

The social and political role of law is an area of research that can offer great insights into human behavior, especially in the areas of activity that interest political scientists. As Barkun points out, "Law provides a means for maintaining change within humanly acceptable limits, for allowing innovation by increments while keeping the general scheme of things within accustomed tracks—for perpetually altering the status quo while perpetually preserving it."

This book is a real contribution to the theory of law which uses as its examples the two main social contexts in which Austin's command theory of law fails to provide a rigorous, or even applicable theory: primitive societies and the world community. Barkun points out the paralyzing limitations of the Hobbesian-Austinian command theory of law in the these contexts, not the least of which are the research aims that are limited by thinking of law in terms of the command theory of law in these contexts, not the superfluous task of a major demolition job

on the command theory. Barkun's point of view is that of sociological jurisprudence as fathered by Eugen Ehrlich and restructured in twentieth century America by such men as Holmes, Cardozo, and Llewellyn. Barkun, however, goes beyond Ehrlich's generalities about the "facts of the Law" or Llewellyn's exhortations to examine judicial behavior, or F.S.C. Northrop's idea of the "living law", to suggest some general theoretical propositions about law which are applicable to societies with or without political or legal structure in the formal sense. He also offers a more useful theoretical approach to the empirical political scientist than H. L. A. Hart by basing his observations on general empirical observations. Moreover, Barkun's work has the admirable quality of suggesting substantive theory rather than remaining in the penumbra of a pre-theoretical or methodological contribution.

Any general theory of law (and at the present time there really is none fully adequate) must cut across cultures rather than being bound to a single arrangement of institutions, must encompass rather than exclude present deviant cases, must cut across disciplinary lines when there is reason to believe that these lines have become outmoded as research guides, and must be confirmed in observed patterns of behavior. Barkun suggests the way to this goal. The empirical referents for this theoretical undertaking are those legal systems which remain on the margin of the classical approach to jurisprudence: international legal systems and primitive legal systems.

The Nuer of the southern Sudan and the Tiv of western equatorial Africa are examples of primitive societies that are jural communities examined by Barkun, and the international counterpart of these primitive communities are the different international systems that have existed in the past several centuries. The function of law within these primitive societies and the world community is essentially mediative, wherein conflict situations are resolved by turning a dyadic conflict relationship into a triadic relationship through the mediative element that is law. In modern developed states legal conflict resolution takes place in an explicit mediative fashion through complex sets of normative rules which are applied by sophisticated administrative institutions, completing the triadic conflict relationship. In primitive societies and the world community this mediative function is also performed, but the law-mediator is only implicit, and invisibly present in the form of system preserving values which have for the most part developed out of precedent settlement of conflict situations.

In the latter two examples of jural communities the systems are characterized as being "horizontal" rather than "vertical" systems of law, where the law is "handed down" from the sovereign to his subjects, a distinction that has often been used to distinguish the international legal system from the systems of municipal law, particularly by Richard Falk. In horizontal systems, however, there is nonetheless a discernible structure through which the legal process takes place. Within the international community and within the primitive societies there are patterns of behavior and social structure which permit "legal" conflict resolution to take place in the triadic, mediative fashion. The segmented lineage systems of primitive societies and the present bipolar structure of the international system are examples of the types of structure which define and limit conflicts, and which provide the actual source of the invisible mediator. The illustrations used by Barkun in demonstrating this proposition for primitive societies are drawn from anthropological literature. His use of the literature is selective and will for the most part be unfamiliar to the political scientist. On the other hand his referents for propositions dealing with the world community may be found to be a little too general and lack rigor for the scholar of international politics and international law. This criticism is not devastating, however, for Barkun seeks less to weave a tight argument around these specific examples than to provide the broad outline of general legal theory.

Barkun convincingly and effectively develops the isomorphism of law in the primitive society and the world community based on the implicit mediatory role of law in conflict resolution, and in doing this he goes beyond the simple development of this isomorphism to treat other relevant and penetrating questions concerning the nature of law. It is perhaps in these corollary questions that the most suggestive parts of the book lie. Although it is sometimes difficult for the reader to tie all the thoughts together in a single thesis (and Barkun warns of this difficulty), there is sophisticated treatment of such questions as the social basis of law, law and perception, and the relationship between law and societal complexity. Political scientists, particularly those involved in international law or law in "non-modern" societies will find much to interest them in this treatment of normative sources and regulation of behavior in situations where the command theory of law can provide no useful analysis.

The usefulness of the type of theorizing that is done in this book lies in its treatment of law as a political and social phenomenon which has as its main purpose to permit human actions to conform to predictable patterns so that contemplated actions can go forward with some hope of

achieving a rational relationship between means and ends. In this context the specific nature and function of law is elaborated. Barkun lists a half dozen "legal univerals" that are found in all societies:

1. Law is a set of interrelated symbols.
2. Legal symbols have some empirical form.
3. The symbols can be manipulated and can be arranged in propositional form.
4. The legal process involves the correlation of fact-situations and normative outcomes.
5. Law as a symbol system is a means of conceptualizing and managing the social environment.
6. The legal process involves the transformation of dyadic interactions into triadic interactions.

The elaboration of these formal characteristics of law provides the basis of theory which can be applied to real-world situations in such a manner as to permit a fuller understanding of law in societies that are not characterized by the existence of a sovereign. Beyond these principles Barkun elaborates additional polymorphous features of primitive systems and international systems which provide much stimulation for thought and should provoke further study along these lines.

W. ANDREW AXLINE
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Voting Patterns of the United States Supreme Court: Cases in Federalism 1889–1959. BY JOHN D. SPRAGUE. (Indianapolis: Bobbs-Merrill, 1968. Pp. 164. $6.50.)

This is a well-written and sophisticated book which should be read closely by students of the judicial process as well as by specialists in research methods and techniques. The manuscript first appeared in the form of a doctoral dissertation, and received the Edward D. Corwin Award of the American Political Science Association for 1966. In its new and revised form it seems destined for additional—and merited—recognition.

The purpose of the study was to evaluate two different methods of ordering and interpreting voting patterns of Supreme Court justices. The methods chosen for evaluation were *bloc analysis* and *Guttman-Schubert scalogram analysis,* and the data consisted of all non-unanimous "federalism" cases decided by the Supreme Court from 1889 to 1960—a total of 816 "usable" non-unanimous decisions.

Sprague found that for this sample of cases the Justices exhibited several identifiable patterns of association. Rates of division varied so significantly, from only 9% in 1915 to 70% in 1952, that comparison was sometimes difficult. Nevertheless, he was able to identify voting blocs in 28 of the 30 "courts" (defined as the time between personnel changes) which sat during this period, and a total of 54 blocs. Of these, nearly half (25) were of the two-judge variety, and only seven consisted of as many as five justices. From these data, Sprague concluded that "blocs are not relatively permanent subgroups across all cases, but rather are shifting subgroups cued into existence by particular issues." (p. 144) This assertion varies substantially from previous observations, by Ulmer and others, which held that decision-making on the Supreme Court was largely the result of interaction by "cliques" or persistently cohering subgroups.

Sprague's dissatisfaction with current definitions extends beyond his empirical results to the methods normally employed for identifying such groups. He feels that there is no acceptable criterion for arranging justices in a bloc matrix. In part, the problem is one of definition. Sprague points out that one has no assurance that a "bloc" identified by current methods will exhibit "over-all transivity; e.g. we have no assurance that if A is highly associated with B, and B is highly associated with C, then A will also be highly associated with C." (p. 31) This is because bloc matrices are constructed by building *pairs,* but interpreted *across* pair lines. The result is that many three, four, and five man "blocs" are frequently mere illusions of the matrix rather than real-life voting combinations.

The author handles this problem in two ways. First, he devises a scheme of ordering the justices along a matrix which at least attempts to meet the problem of relating A to C. Second, he adopts a very restrictive definition of bloc as "only a subgroup of justices for a given inter-agreement table [which] meets the criteria. A bloc is a name for a pattern of scores in a summary voting record, no more." (p. 56) While I think that his new rules for ordering justices along a matrix are intelligently conceived, I am bothered by his redefinition of bloc, for two reasons. First, it seems to me that even this definition does not entirely satisfy the transivity problem. There is still no assurance that justices identified as belonging to the same bloc *vote together as a bloc.* All it assures us is that their voting patterns are related. Second, to describe a bloc as merely the label affixed to a pattern of votes devitalizes a concept whose strength must lie in its sensitivity to the *actual* interactions among justices. Furthermore, since Walter Murphy was undoubtedly correct when he observed that merely knowing accurately that judges voted together was a rather weak indication of their interaction or "bloc" tendencies, the vul-

nerability of bloc analysis becomes even more apparent in the light of Sprague's findings. For the weakness inherent in relying solely on votes to establish bloc identities is only compounded when the accuracy of identifying voting blocs in this way is also brought into question.

Sprague's second approach utilizes Guttman-Schubert scalogram analysis. Unlike his bloc analysis, which produced identifiable subgroups in virtually every "court", his attempts to scale these 816 cases was a failure. This could have resulted simply from a lack of unidimensionality in his subject, or it could have resulted from deficiencies in the techniques and theory of cumulative scaling. Since Sprague used the same definition of "court" for his scaling as for his bloc analysis, he reasons that his inability to achieve scales was due in part to the extended periods of time involved. Most cumulative scaling done by others has been for single terms of the Court, or for relatively short time periods, and Sprague suggests that while "cumulative scaling is descriptively useful in the short run, [it can be] theoretically misleading in the long run." (p. 150) If, as Sprague suggests, voting behavior of judges changes over time as a result of their on-the-court experiences, and that these changes are in the direction of a reduced simplicity of outlook, then he has described a major weakness of cumulative scaling of judicial decisions—the inability of the method to comprehend longitudinal or developmental dimensions.

If this book lacks anything it is a modicum of descriptive background. One might want to know something more about the 816 cases in the sample, or about the impact of these judicial voting patterns on differential policy outputs—not enough to turn the book into a narrative description or collection of anecdotes, but just enough to give greater life and vitality to the subjects of the analysis. But perhaps herein lies a different book—hopefully a sequel to this excellent study.

JOEL B. GROSSMAN
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Man and His Government: An Empirical Theory of Politics. By CARL J. FRIEDRICH. (New York: McGraw-Hill, 1963, Pp. 737.)

An Introduction to Political Theory: Twelve Lectures at Harvard. By CARL J. FRIEDRICH. (New York: Harper and Row, 1967, Pp. 182. $5.50.)

Reviewership has been defined by Stephen Potter as the art of conveying the impression that the reviewer knows more about a subject than the author who has just written a book about it. In the case of this author and this reviewer, this is impossible.

Political science, like many other fields of knowledge, has grown far beyond the point where all of it could be carried in the head of a single man. Even today, however, some scholars come closer than others to this now unattainable degree of comprehensive knowledge of their general subject; and a few of them also have the intellectual power and self-discipline to organize the very great amount they know.

To the best of my own limited knowledge, Carl Joachim Friedrich knows more political science than any other man now living. This is particularly true in the great field of classic political theory and philosophy, but also in the no less important field of political experience as embodied in the comparative study of governments and of political and legal institutions. In addition, he has made distinguished contributions to the study of history; to the theory and practice of federalism and of constitution-making; to the early study of public opinion and of political parties and movements; and to the hard-headed and penetrating analysis of foreign policy and international affairs.

For every topical specialty, and for every particular country or geographic area, there may well be by now some specialists with a greater knowledge of detail. But if the intellectual unity of political science continues to be an important asset of our profession, and a basic condition for its sustained growth as a coherent field of scholarship and social science, then the broad, comprehensive competence and contribution of men like Carl Friedrich, Quincy Wright, Harold Lasswell and a very few others are among the best resources we have.

In *Man and His Government,* Friedrich attempts to put a very substantial part of the essentials of his knowledge into a single book. In his much briefer *Introduction to Political Theory,* he uses his knowledge only as a background for his selection of theorists and topics, and for the questions and judgments he puts forward. Both books are very much the product of a single mind—a mind that has imposed unity and order upon a vast, lively and unruly subject. The *Introduction* is an excellent book for its purpose. It combines great teaching skill with deep thoughtfulness about abiding problems, and with an awareness of the realities of present-day politics. *Man and His Government* is a very different kind of book. It is an excellent instrument for teaching the more mature and thoughtful students from the junior and senior undergraduate level upward, but it is also apt to teach the faculty—all of us—for many years to come.

Man and His Government is the most impressive summary I know of the great tradition of

political science within the compass of a single volume. Almost every one of its 675 pages is rich in thought, in facts, and in references to some highly pertinent works of political science, history, philosophy, or the social sciences.

This does not mean that readers will always agree with Professor Friedrich's views. Thus his definition of science (p. 6) seems to me too heavily weighted on the side of the consensus of the current generation of specialists within each currently accepted special field, and it may not stress sufficiently the verification by reproducible evidence from nature or external reality, which may eventually override the objections of the earlier specialists, or which may convince their successor, so as to change some of the basic concepts and methods of a scientific field, or change the field itself, or add a new one—as has happened from Galileo's telescope to Planck's quantum concept and the emergence of such new fields as nuclear physics and radio astronomy.

Again, we may feel uneasy when we read in the chapter on "Political Representation and Responsibility" that "the more permanent minorities who have no chance of ever electing their own choices, because of . . . racial or other permanent differentiation . . . are nonetheless represented but by unasked for representatives who represent them because they exist and occupy the position they do." (p. 308) Or we may wonder whether indeed in any near future "the comprehensive cultural wholes seem to be emerging as the components of this future international system . . . as the term 'nation' is broadened to cover such vast cultural complexes as India, Europe or Africa." Later in the same paragraph" "Latin America" and "Arabia" are seen now as such "emerging" political units, while "much uncertainty surrounds the British Commonwealth." (pp. 673–74)

Even where one feels some reservations about such points as these, the arguments in which they are embedded in Professor Friedrich's are deep-probing, rich and fruitful. One can learn more from disagreeing with one of his arguments, and from trying to think it through in the process, than one could learn from nodding in agreement with some lesser writer.

Most of the time, however, readers will agree with judgments which will strike them as sound and deep at the same time, adding unexpected points and perspectives to what had seemed familiar problems, and opening new prospects upon them with subtlety and sophistication. Among the most valuable parts of the book are the discussions of political community, not only in Part IV, but also in many of the earlier chapters. Another valuable part, rare in books of this kind, is Chapter 21, on "Political Innovation and Invention." Still other strong points seem to me the rich and thorough discussions of "authority," "legitimacy," "leadership," "influence and the rule of anticipated reaction," "justice," "order"—all matters that too often remain vague, or else are forced into small Procrustes beds of one-sided attempts at operational definitions. To be sure, Carl Friedrich does not make many of these concepts fully operational—often he leaves the reader "merely" with a deepened sense of insight and of recognition—but anyone who could try to develop adequate operational definitions for such matters should take very careful note of his work in this volume.

Despite its comprehensive and systematic character, there are certain kinds of political science which are absent or underrepresented in both books. Both works are first and foremost books of thought. Neither of them contains much in the way of specific materials or explicit evidence from such modern methods of political research as survey research, content analysis, voting studies, or the quantitative analysis of aggregate data of other kinds of actual behavior. Sometimes mathematical formulations are offered which reach beyond the present stage of our knowledge, such as a "formula for community" (p. 145), whose particular shape cannot now be proved to be correct or necessary, and whose terms cannot as yet be given quantitative meaning.

If one contrasts this with the extremely rich and sophisticated treatment of the philosophic contributions of Aristotle, Plato, Hobbes, Kant, and Hegel to the tradition of political thought, one might get the impression that Carl Friedrich's thought in both books is primarily oriented towards intellectual history and to the past. The scant index provided by the publisher for *Man and His Government*—only 3 per cent of the pages of the volume—tends to enhance this impression: it lists 55 references to Aristotle, 48 to Plato, but at most two to any political theorist now living in the United States, while the names of such men as Gabriel Almond, Robert Dahl, David Easton, David Truman and Quincy Wright do not appear in the index at all.

This impression, however, would be quite misleading. It is precisely one of the strengths of *Man and His Government* that it does confront the traditional wisdom and experience of political science with the work of modern theorists. This confrontation and discussion is carried on, almost on every page, in the footnotes (which, it seems, were not included in the index). Here modern works and authors are cited, criticized, agreed with, or questioned, in a continuing scholarly discussion that alone would be worth

the price of the book. A better index in a future edition would make this valuable material more readily accessible to students and scholars. The present poor index is balanced, however, by a rich and uncommonly good bibliography, which again brings traditional and modern works together.

Altogether, *Man and His Government* is a monument to the enduring intellectual unity of political science. It offers a major contribution of the kind we need now and for many years to come. If it stresses in its substance the analysis of such concepts as "authority," "legitimacy," "hierarchy," "bureaucracy" and "order," it also deals extensively with problems of revolution and of change, and it is particularly well-informed and constructive on the problems of democracy, law and constitutional government. If its methods include prominently the approaches of philosophy and history, its discussions—both in the text and in the footnotes—confront these time and again with the findings of empirical research in the behavioral sciences. I know of no other book that has gone so far in laying the foundation for a genuine and sustained dialog between adherents of the great tradition of political thought and the empirically and quantitatively oriented students of political behavior. It is rare for one book to fulfill the tasks of a small special library. This one does; and no serious political scientist should be without it.

Compared to this massive contribution, the function of the slender *Introduction to Political Theory* is largely pedagogical. It is indeed an excellent introduction. Much of the material it offers is traditional. It stresses most heavily the ideas of Christianity, Plato, Aristotle, Hobbes, Socrates, Marx, Machiavelli, Kant, Hegel, Engels, Thomas Aquinas, Rousseau and John Stuart Mill. References to any recent or living theorists are rare. But the problems to which the resources of the great tradition are applied are modern, and both the ideas and their manner of presentation are thoroughly alive. (The Harvard undergraduates in their unofficial *Crimson Guide to Courses,* usually quite critical of their faculty, called a typical lecture by Carl Friedrich "a tightly organized gem.")

This slim book of twelve lectures lives up to this standard. At the same time, it sets a standard of our teaching of beginners. For it demonstrates the kind of background knowledge which beginning undergraduates should acquire as early as possible in order to approach modern problems of political research and analysis with some of the intellectual and historic depth which their adequate exploration and solution is likely to require.

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NOTES

POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

The Liberal Politics of John Locke. By MARTIN SELIGER. (London: George Allen & Unwin Ltd., Pp. 372. 55s.)

Perhaps the good deeds of great men are 'oft interred with their bones,' but such is rarely the fate of their ideas, which seem capable of an infinite number of resurrections. Lockean political philosophy is the current beneficiary—or victim—of this resuscitation ritual. That it is experiencing an 'identity crisis' is a mild understatement. We have discovered the 'conservative' (Strauss, Cox), 'bourgeois' (MacPherson)—even the 'radical' majoritarian Locke (Kendall). Professor Seliger's contribution is a modernized version of the 'liberal' Locke. His objective is to provide a "clarification of basic ideological issues by elucidating the components of a political tradition" (p. 26) i.e., liberalism.

Since part of our ideological confusion is traceable to its muddled source, any 'basic' clarification ought to present a reasonably coherent account of Lockean thought. But, even if we understood Locke's views, we would still want to know their relationship to the 'tradition' of liberalism, which, at a minimum, demands a defensible (historical) definition of liberalism. Neither requirement is met in this book.

Was Locke a liberal? Seliger offers no definition of liberalism, but it appears to include political guarantees of individual liberties (p. 21), representative institutions (p. 179), the sharing and limitation of political power (p. 293), and supremacy of law and the legislature (p. 369). Yet, we are told that Locke's concessions to "authoritarian leadership" are "fundamental to his political theory" (p. 24), that he justified imperialism and slavery (p. 116ff.), and the exercise of "absolute power in politics." (p. 264ff.) He legitimized 'forcibly seized' political power (p. 253), was not a laissez-faire liberal (pp. 165, 179), and certainly not a democrat since he opposed majoritarian suffrage (pp. 253, 288).

If this seems a jaded picture of a 'liberal,' it is

no less modern for that. For, "operative democratic theory" is "not as far removed from Locke as it pretends to be." (p. 301) For a 'liberal tradition' opposed to universal suffrage (p. 19), fundamental issues reduce themselves to "the procedure through which an elite attains its special rights." (p. 293) Hence, politics is "a continuously competitive process of maintaining . . . a tolerable balance between powers which represent individual rights and capacities." (p. 259) How reassuring to know that "economic proficiency determines in truly liberal fashion the extent of political rights" (p. 292), that 'liberal' regimes may dispense with the "constitutionally expressed consent" of the people (p. 293), that, in fact, the apathy of the masses is "a civic virtue." (p. 320) At last, we see that, for liberals, "the chief executive is at the centre of it all, playing a decisive role in the legislative process and possessing the constitutional authority to act . . . as the sole and authoritarian representative of the will and interest of the people." (p. 367) The true sons of liberalism are Dahl, Laswell, Kaplan, and Easton, who Seliger cites in support of the above statements. Since I have my own reasons for agreeing with this conclusion, which have nothing to do with Seliger's book, I will leave it to exponents of liberalism to consider whether this 'defense' advances their cause.

A few words, however, must be said on Locke's behalf. Whatever his reputation as the archtypally ambiguous thinker, nothing Locke wrote is as confusing as Seliger's 'elucidation' of it. Thus, the 'state of nature' is: "not a real situation" but a "hypothesis" (pp. 82, 90, 258), a "specific situation" within political society (p. 259), "both" a hypothesis and "a real situation" (p. 169), not to mention a "psycho-sociological model" and "socio-political-key-concept." (p. 261) Perhaps it is a self-projected image when Seliger portrays the political ruler as 'hovering' like some giant moth between the state of nature and political society. (p. 368) Under a barrage of misspent verbosity, the problem of 'consent' evaporates before our eyes. According to Seliger, "express" consent equals "tacit" consent (pp. 226, 275), equals "an attitude . . . embedded in a process" (pp. 224–5, 261, 298, 301), equals what men would give *if* they were asked (p. 224), which equals "the absence of active dissent." (pp. 253, 261, 293, 297)

Such leaps of logic are made with the ease of an olympic hurdler, though Seliger ungracefully attributes them to Locke. After drawing a distinction between 'rebellion' and 'revolution,' which Locke did not make, Seliger accuses him of using the two terms inconsistently! (p. 317) A similar point is made in his discussion of 'absolute' and 'arbitrary' power. (p. 245ff.) If, says Seliger, rebellion is opposition to authority, not persons, then "it follows" that revolution is opposition to persons, not authority. (p. 317) Or, "any regime must be regarded as sustained by consent until it is challenged by revolution." (p. 298) Defending his interpretation of Locke's advocacy of one-man rule, Seliger's comment on a seemingly democratic passage ("it was the Tribes and the Congregation, that debated, resolved, and directed all that was done . . ." *First Treatise,* par. 165) is that it does not exclude individual leadership. (p. 220) On that reasoning, rule by a Supreme Court or a Poohbah cannot be excluded either.

Although Seliger proposes to discuss only the "mature" Locke, he makes no use of the "mature" works on toleration and education (when will we learn that not even a political theorist says everything in one book?), and quotes unsystematically from two early works to confirm some of his arguments, ignoring or rejecting them when they disconfirm others. Given Seliger's emphasis on Locke's "revolutionary" views, his non-developmental approach is particularly ill-advised, since, for most of his life, Locke was not a revolutionary; and, in his *Journal,* only three years before beginning the *Two Treatises,* he reaffirms his non-revolutionary position. Thus, despite its importance, Seliger offers no insights whatsoever into the intellectual, social, and political reasons for Locke's change of heart.

This exposes another difficulty: viz., in addition to inattention to Locke's other published works, Seliger writes without a knowledge of the approximately thirty volumes of Locke's correspondence, journals, notes, and manuscripts, not to mention contemporaneous writings and other historical evidence one might have used as aids for understanding some of the problems of Lockean philosophy. The apparent excuse, that history provides little guidance because contemporaries are themselves embroiled in controversies (pp. 32–3), would embarass the worst historian. Seliger himself remarks that "an unhistorical attitude is at the root of much of the talk about Locke's inconsistency or ambiguity . . . the resulting contradictions or ambiguities are often of the interpreter's than of the philosopher's making." (p. 33) I agree. At least, applying that suicidal judgment to his own book, a bad affair is brought to an honorable conclusion. —RICHARD ASHCRAFT, *University of California, Los Angeles.*

Toward a Mathematics of Politics. By GORDON TULLOCK (Ann Arbor: University of Michigan Press, 1967. Pp. 172. $7.95.)

Gordon Tullock writes of a diversity of topics: communications, lying, political persuasion, monopolistic profits, the aggregation of political and economic preferences, and proportional representation. These distinct subjects are linked in two ways. Throughout, man is assumed rational in the

sense that he is engaged in maximizing behavior. Tullock is most concerned with specification of choices relevant to maximizing in various contexts. When it might "pay" to be politically ignorant, is an example of one context of interest. A second unifying characteristic of the book is that the discussion of almost all topics is accompanied by geometric illustrations and formulae of a sort. Still, though, we had best take Tullock at his word (p. iv) and not expect one theme to pervade the book.

A chapter of particular interest to many political scientists is that on the irrelevance of Arrow's impossibility theorem for large groups of individuals faced with a limited number of policy alternatives. As applied this theorem simply states that it *may* be impossible for a majority to agree on any proposal over all others, although majorities often form to favor one proposal over one other. Tullock's argument is based upon voting rules actually employed, particularly in legislative bodies. First, hair-splitting amendments are not allowed. Second, procedure usually consists of making paired comparisons among alternatives, but not all combinations are offered. Voting is not a ranking process *per se*. This, by itself, does not indicate the irrelevance of the theorem in a strict sense; institutional arrangements make it irrelevant, so long as any set of policies each of which commands majority support over any policy not in the set is reasonably compact. Once a point in this set is obtained the cost to an individual of attempting a change to another such point in the face of institutional and conventional obstacles is probably greater than benefits gained. Lack of endeavor to change policy, to retrace legislative action (p. 47), can not, then, be taken as evidence that circularities do not exist.

The consideration of costs missing in the discussion of the impossibility theorem is found in Tullock's discussion of monopolistic competition. Population density and the consumer's aversion to traveling to a good are at least two variables not usually found in the micro-economic or anti-trust literature. In this chapter and those on persuasion, information and lying, Tullock presents costs and calculations in an analytic style sure to reward political scientists interested in these topics. Additionally it is a joy to find someone who at least attempts to distinguish between hypotheses and guesses about fact, a distinction not made in most of our literature. For these reasons and because it is the most thoroughly argued chapter in the book, the discussion of monopolistic competition stands out.

There are hypotheses in this book. Some are confusing, some implicit, but that they are there at all is salutary. Confusion reigns in consideration of the direction of the effect of population density on store size, prices and profits in monopolistic competition. Density would increase store size and lower price, except that density creates congestion which raises transportation costs. Tullock really only indicates variables to be contained in hypotheses in this instance. However, in the chapters on information, propaganda and lying, there are several hypotheses on the comparative level that might easily be passed over (eg. pp. 86–87). These hypotheses have no numerical aspects.

Interestingly, none of the hypotheses in this book are mathematical; that is none involve rational functions of intervally measured variables. The formulae which occur are best taken to indicate the hypothesized direction and additive or multiplicative form of the relationship among variables, if intervally measure (eg. pp. 109, 124, 126, 137). Actual formulae employed are for illustrative purposes (eg. the equation and subsequent differentials on pp. 75–76). Sometimes only the fact that a hypothesis exists is indicated (eg. p. 94). Further, the geometrical illustrations are of arbitrary instances of the hypotheses or facts presented; no hypothesis is presented *per se* as a geometrical construction.

If functions of intervally measured properties are not found in this book, why the title? Part of the responsibility does not rest with Tullock. Among young or "current" political scientists mathematics is often equated with doing science. Nothing could be further from the truth. Mathematics is a tool in empirical research. Gimmicks such as "causal analysis" are misused tools. Certainly advanced sciences possess functional laws, but doesn't this indicate an appropriate emphasis on concept formation, on the definition of intervally measured properties so we can use the tool and not concentration on the gimmicks? Fortunately Tullock's text does not betray the misunderstanding of the title.

Tullock's introduction does betray such a misunderstanding. He talks in terms of a general (probably "mathematical") theory of political science and economics. Isn't this putting the cart before the horse at a time when one would be hard pressed to even state many testable (not vague) hypotheses? However, I will not take the introduction too seriously because the rest of the book does not depend upon it.

Tullock's first and last chapters are clearly the weakest. A principle reason for this is that he sets up straw men which are either oversimplified or nonexistent. When he argues his own brand of "utility theory" in chapter one, I simply do not recognize the extreme positions he argues against. Tullock's arguments against depreciators of the non-informative aspects of advertising are weak, because they also counter absurd positions. Much more subtle positions exist than those Tullock

presents for the opposition. What about the arguments concerning the effects of advertising on expenditures on private versus those on public goods *in the long run?* What about long run effects on taste? However, Tullock's real problem is simply that the subjects of chapter one deserves a book. So does the last chapter. We know very much more about the effects of P.R. and plurality representation than Tullock presents; he argues against what must be considered even an over-simplification of Duverger. Tullock clearly prefers P.R. and, unfortunately, unlike any other discussion in the book, the preference distorts the picture. However, most of the rest of the book is very objective, very thoughtful, very clearly presented, and very enjoyable reading.—E. W. KELLEY, *University of Chicago.*

Middle Class Radicalism: The Social Bases of the British Campaign for Nuclear Disarmament. BY FRANK PARKIN. (New York: Frederick A. Praeger, Publishers, 1968. Pp. 207. $6.50.)

Students of mass movements will find this study of the social bases of the British Campaign for Nuclear Disarmament (CND) of great interest. Sociologist Frank Parkin has approached his task effectively, both empirically and intuitively. His analysis is based on a questionnaire drawn from some 445 CND followers, the literature produced in the course of the campaign, as well as on selected interviews. He treats the youth, Labour Party, church and intellectual components of the CND as well as the political impact on the Left, including the British Communists.

Parkin suggests that his findings question some well-known formulations relating to mass movements. He asserts that the thesis suggesting that mass movements derive a good part of their following from the dispossesed classes, the alienated, the atomized and the futile is not supported by his data. All but a small portion of his sample were of middle or upper class status, were involved in other organizations, and were optimistic both about the remediability of the human condition and their own efficacy in this quest.

As Parkin unfolds his evidence significant modifications in his own challenge to the traditional theses must be made. For example, in his last chapter he points out that large numbers of his sample, though "middle class" were only marginally so—that is, of other class origin and/or involved in the "independent" professions (university lecturers, physicians, clergymen, freelance intellectuals, etc.), while relatively few CNDers were from Establishment or "interdependent" professions (such as the civil service or commerce) giving strong sense of class and social identity. In terms of Lenski's hypothesis, the typical CND follower's different statuses (e.g., ethnic, occupational, economic, educational) were out of line with one another, in that he ranked high on some but low on others. In light of this conflicted model Parkin's earlier statistic indicating typical participation by CNDers in other organizations could well suggest the attempt of the relatively isolated and marginal to *overcome* that isolation as stated in the usual formulations of Arendt, Hoffer *et al.,* rather than indicating lack of isolation. This is especially indicated since the organizations to which CND followers tended to belong ("activist" and "moral orientation") would give them more identity with their isolated fellows than it would provide "intermediate structures" connecting them with the solid middle class or society as a whole.

This example only illuminates a central problem of which Parkin seems very aware. In what senses was the typical CND supporter "middle class?" Income, occupation, family and even education are indelicate tools for this task. Parkin himself sees the concept of "middle class" as mainly useful "in making contrasts with other social strata." Accordingly, the author attempts to analyze the differing characteristics of various groups within the CND, while maintaining the middle class description for the movement as a whole. Consequently, he fails to really exploit the theoretical implications of his own finding that his respondents overwhelmingly rejected prevailing middle class values: monarchy, nationalism, religion and capitalism. The great majority (58%) were non-believers—agnostics, atheists and so forth. Parkin also suggests that the CNDer, though typically "middle class," has "moral" rather than material interests to promote, unlike lower class mass movements. This is the heart of the problem. If the CND follower substantially rejects both the material interests and attitudes of the middle class, is conflicted in his middle class attributes and is not a member of the middle class as a social or political actor, then of what use is this very broad category except to register what become really secondary and only loosely functioning characteristics? The problem is more than semantic because Parkin never sufficiently frees himself from his middle class focus to develop the theoretical implications of his own data. In short, Parkin is not really discussing middle class radicalism at all but *intellectual* radicalism.

It was the intellectual component of the CND that gave it its distinctive ethos; indeed, as Parkin points out, without the intellectuals it is doubtful that the CND would have been created at all. If either the terms "intellectuals" or "middle class'" is to have any real meaning for the analysis of modern society they should be defined and treated separately. For most purposes, "middle class intellectual" is a contradiction in terms. In contemporary, mass-literacy societies where

large portions of the population may be engaged in the manipulation of the symbolic environment as a vocation the term "intellectual" is very loosely applied. Parkin must have such a large and indistinct group in mind when he says that "political conformity, in the broad sense, is a more typical position of this stratum" than dissent. A far more useful approach would have been to sharply delineate the qualities of mind attendant upon that relatively small group of creative intellectuals which dissent and criticize in every society, albeit to different degrees depending on the conditions of political and social power. It was to this component that Parkin himself was pointing when revealing the remarkable consistency of many of his respondents in their socialism, internationalism and pacifism—in short, all those "apolitical" traits of that stratum which derives its sense of being and action from a shared world-view rooted in various degrees of alienation. Once one had detected this intellectual core for the Campaign for Nuclear Disarmament by giving the intellectual more discrete treatment the author's extensive evidence that the CND was an umbrella covering a wide range of anti-Establishment (i.e. anti-middle class) grievances becomes more meaningful. Parkin could have very usefully dispensed with the notion of middle class and treated the intellectual stratum which provided the core of the CND as a separate phenomenon.—G. ERIC HANSEN, *Massachusetts Institute of Technology.*

The Origins of American Politics. By BERNARD BAILYN. (New York: Alfred A. Knopf, 1968. Pp. 161. $4.95.)

Liberty and Authority: Early American Political Ideology, 1689–1763. By LAWRENCE H. LEDER. (Chicago: Quadrangle Books, 1968. Pp. 146. $5.50.)

These two volumes by historians shed considerable light on American political thought prior to the Revolution. Both books deal extensively with the concepts of power and liberty in the colonies during the eighteenth century and help to fill a gap in the literature.

In his work which is an expansion of the Culver Lectures at Brown University in 1965, Bernard Bailyn, presently Winthrop Professor of History at Harvard, examines the differences between the political processes in eighteenth-century America and eighteenth-century England that can aid in explaining "the significantly different receptions of the same political ideas." While noting the common British culture and belief in certain Lockean political principles in England and the American colonies, the author emphasizes the differences in the role of the executive in the two areas. Except for Rhode Island and Connecticut, the executive power was legally "far stronger" in the colonies than in the Mother Country. However, the actual power of the colonial governors was greatly reduced because of rigid instructions and an increasing concentration of patronage powers in England. Thus, the governors were denied important weapons which were available to the Crown and its ministers in asserting influence and stifling dissent in Parliament.

The last chapter of Bailyn's volume is devoted to a number of examples of how the colonial governors actually exercised little political influence despite their legally autocratic power. Although concentrating on the situations in the royal colonies, the author claims that the pattern was not substantially different in the proprietary colonies, for the "Quaker party" exercised strong control over the Pennsylvania House in the 1740's and '50's and the Maryland House of Delegates reacted vigorously to the use of strong proprietary power. Bailyn states that democracy in many ways was more advanced in the colonies than in England; thus, there was greater opportunity for radical opposition to overweaning power and authority to be expressed and organized. Playing down the importance of the Stamp Act as an incendiary action in itself, the author claims that there was already a colonial mind that was steeped in an ideology which emphasized the evil of despotic power; therefore, the actions of Parliament in the 1760's and 1770's were more suspect than they otherwise might have been.

Although Bailyn's argument and documentation are very convincing, he might have devoted more space to the lack of an American aristocracy in explaining the differences in the influence of the English and colonial radicals in the eighteenth century. The Tocquevillian insight, which has been developed so well by Louis Hartz, that Americans were "born equal" should certainly be taken into account in an examination of the power structure in the colonial period immediately prior to the Revolution.

Lawrence Leder, presently Professor of History at Louisiana State University in New Orleans, gives credit to Bernard Bailyn for producing the "one major effort to penetrate the ideas held by Americans in general" in the eighteenth century and then proceeds to examine the ideological conflict between liberty and authority in the colonies in that period. The development of freedom of expression and especially the role of the press as a critic of the arbitrary use of power are noted as important factors in the revolutionary movement. The author shares the conventional view of the importance of Lockean principles in mid-eighteenth century America, including religious toleration.

Leder states that a crucial consideration in the political discussions in the colonies was the actual

nature of the British constitution. He notes that two general approaches are found in eighteenth-century American writings on the subject—that the British constitution was fixed and rigid and that Parliament was supreme and could change the constitution. Although a couple of citations from colonial periodicals are used to indicate support for the position of Parliamentary supremacy, there is not much evidence to show that the colonists generally understood the implications of the Glorious Revolution. The author does state that from 1749 on, the "myth" of a fixed British constitution was not effectively challenged in colonial writings.

Leder devotes a chapter to the debates over the nature of colonial charters. He notes that the disputations often arose in connection with the respective powers of the executive and the popularly-elected legislature and very frequently concerned money matters. The disputes over powers in the royal and proprietary colonies were never settled, whereas the debates over prerogatives in the charter colonies did lead to "an increasingly well-defined concept of charter rights." The disagreement among the colonies on the interpretation of the colonial charters did not allow for a united front against the British on the grounds of local constitutional principles.

In the reviewer's opinion, Leder's most effective chapter discusses the importance of rights and liberties in the pre-Revolutionary argument. Noting that the colonists developed a "simplistic version" of the Magna Carta and the rights of Englishmen, the author points out that these eighteenth-century Americans did not define their rights or liberties except in broad generalities as a rule. Here Leder ties in with Daniel Boorstin's theory of the "giveness" of American political principles which are only vaguely defined if defined at all. The author points out that this broad concept of rights often forced the British to formulate a definition of the principle in question. However, the Lockean rights were absolute and thus the colonists could respond that any limitation on these liberties was an example of the improper use of power.

These small volumes should prove valuable to students of American political thought and colonial government. Of the two, the Bailyn book makes more of an original contribution, but Leder's study also contains an interesting analysis of American political ideology prior to 1763.—Don W. Driggs, *University of Nevada*

Information Retrieval: Applications to Political Science. By Kenneth Janda. Indianapolis: Bobbs-Merrill Co., Inc. 1968. Pp. 230. $4.50.)

Information Retrieval is primarily a collection of articles which the author has published previously. The volume is organized into three sections which emphasize, repsectively, retrieval techniques, their application to political research, and their application to administrative and legislative decision making.

As Professor Janda demonstrates in this work, the potential contribution of modern information processing technology to the science of politics includes a great deal more than the computerized analysis of quantitative data. In Janda's words, "techniques of information retrieval promise to assume an important place in the activities and concerns of political science, for they offer the political scientist new methods of handling the vast flood of written materials that constitute both the data and the findings of political research."

Of course, the application of computers to nonnumerical materials is by no means a new endeavor as the growing literature on content analysis will attest. What is significant about the studies described by Janda, and by most work on the automated retrieval of politically relevant information, is not simply that textual data can be treated, but that because it can, the value of retrieval systems to the political scientist can be realized during the earliest stages of research. For by making possible wider and more rapid access to original documentary data and to the published accounts of other scientists, information retrieval systems are able to contribute to the critical processes of problem conceptualization and the formulation of hypotheses.

Janda's discussion of the formidable intellectual problems associated with classifying the contents of a document so that it can be obtained by all retrieval system users interested in it is first rate. Illustrating how 'key word' methods have been employed, he describes the preparation of the *Cumulative Index to the APSR* (1906–1963) and how KWIC and KWOC programs were used to search through abstracts of literature in order to identify data sources for the International Study of Values Project.

Among the retrieval applications which Janda covers are: Northwestern University's system for automatically disseminating articles drawn from leading social science journals to faculty members who have previously designated their areas of interest, a hybrid computer-microfilm system permitting rapid and selective access to recent literature on political parties, and computer generated key word indices to roll call votes in the Kansas Senate, the U. S. House of Representatives, and the U. N. General Assembly.

The final chapters of *Information Retrieval* set forth a group of proposals; the most interesting of these is for providing extensive improvements in the process whereby information is handled by the Congress. Janda analyzes the kinds of information services that the Congress, its chambers, its committees, and individual Congressmen might wish

to utilize. Basically, he argues that the oft-alleged decrease of congressional *vis-a-vis* executive power can be most effectively redressed not by reorganization or political reform, but by 'retooling' the Congress with new information systems.

By choosing to focus upon a limited number of information retrieval projects, Janda has created a work that I find to be admirably specific and remarkable for the conceptual integrity of its contents. It conveys an awareness of the major theoretical and operational problems in this field by confronting the reader with the details of their resolution in each of the projects examined. Throughout the volume, Janda's discussions reveal a balanced sensitivity to the practical as well as the conceptual aspects of retrieval. However, the principal emphasis is always directed to those issues which most greatly affect the development of improved ways of securing for the political scientist rapid access to the information he seeks.

Janda's writing is lucid and his manner of presentation forthright. Nonetheless, this is not an easy book to read. Its subject matter is technical, and although the casual reader will acquire some understanding of the kinds of functions that information retrieval systems can perform, a full appreciation of the material covered will be accessible only to those who are either already familiar with retrieval techniques, or to those willing to invest the effort required for a close reading of this work. This caveat, however, sould not deter the uninitiated. On the contrary, Janda's book is an excellent starting point from which to learn the fundamentals of information retrieval. Indeed, when viewed as an introductory volume, even the defect of redundancy among portions of several of the articles emerges as a virtue, permitting the reader to review the details of a given technique and to understand its use in a variety of applications.

To summarize, Professor Janda's *Information Retrieval* is highly instructive, and, in my opinion, represents a valuable contribution to the discipline.—EDWARD FRIEDLAND, *State University of New York at Stony Brook.*

Social Theory and African Tribal Organization: The Development of Socio-Legal Theory. By KENNETH S. CARLSTON. (Urbana: University of Illinois Press, 1968. Pp. 462. $10.00.)

The dictum that form follows function applies as much to works of scholarship as to skyscrapers or armchairs. Thus the only fundamental difficulty with Kenneth Carlston's most recent book is a question of manner rather than matter. This is particularly unfortunate, for in many respects this work contains much of great interest and importance.

The bulk of the book is given over to brief descriptions of twenty tribal societies, drawn from East and West Africa. Despite their brevity (seven appear merely as abstracts of the literature), an immense quantity of the ethnographic literature appears here systematically and lucidly presented. Of necessity, many of the tribes Carlston has chosen are those most thoroughly studied, such as the Tiv and the Nuer. In any case, these miniature portraits show once again that we know more (or at least can *say* more) about societies culturally and physically remote from us than about those in which we normally function and which occupy the major energies of the social sciences. In reading these chapters we can only wish someday to see comparable ethnographies of American society and of international relations.

Carlston has chosen quite properly to emphasize some elements of the ethnographic data to the exclusion of others, notably the rich material on order and conflict. The works he draws upon, by British social anthropologists and their students, are peculiarly laden with such data. Why this should be so is itself an intriguing question in the sociology of knowledge.

But unlike other who have written on law in tribal societies, such as E. Adamson Hoebel, Carlston gives substantial weight to the role of religion and the supernatural as part of the sanctioning process. This continual counterpoint of religion and social conflict has three major implications: First, we often arbitrarily divide "reality" into tangible and intangible parts; by concentrating on the former alone we miss the vital connection between beliefs and actions. Second, if religion is to be considered part of the sanctioning process, legal theorists must drastically overhaul their concept of the nature of sanctions, and we might well ask whether the term should not be dropped entirely. Third, Carlston implies that there are levels of both order and conflict, and that law regarding each should be separately considered. To this end, he places great stress on a distinction between conflicts within an accepted social order and "constitutive conflict," which challenges the validity of that order. This in turn suggests that law is most accurately pictured as a multiple, "layered" phenomenon, ranging from the most basic social values (close to Hoebel's "jural postulates") to subordinate, related values, disputes about which need not produce major fissures in the society.

If there is a major flaw in the work, it lies in its peculiar architecture, for the ethnographic material is sandwiched between two large sets of propositions. The first is a deductively arranged summary of the social theory that may be found in Carlston's earlier books, *Law and the Structures of Social Action* (1956) and *Law and Organization in World Society* (1962). While this is useful to have in itself it is intended to somehow link up

with the second set of propositions at the conclusion of the volume. These, however, have been inductively generated out of the ethnographies. If they are meant to serve as a test of the first set of statements, this has not been made clear. The first set may not in fact consist wholly of testable statements, and in any case the subheadings of the two chapters are not always parallel. Carlston is at pains to point out that the concluding observations are valid only insofar as they apply to the tribes he has studied and to emphasize this has appended after each proposition the numbers of the chapters that furnished the evidence. But it is difficult to know how to make use of any two statements, one of which may be true of twenty tribes, the other of only one. Yet the broadly general and the uniquely particular coexist within the same purportedly logical framework.—MICHAEL BARKUN, *Syracuse University.*

The First Bolshevik: A Political Biography of Peter Tkachev. By ALBERT L. WEEKS. (New York: New York University Press, 1968. Pp. 221. $7.50.)

Scholarly eyebrows will no doubt rise in response to Professor Weeks' seemingly presumptuous contention that an oft-mentioned but rarely studied nineteenth century Russian Jacobin socialist, Peter Tkachev, was, in reality, "the First Bolshevik." Whatever the reaction, the man Lenin called "the magnificent Tkachev" has been a victim of neglect. Albert Weeks has attempted to rectify this by locating the roots of Tkachev's philosophy within the tradition of nineteenth century Russian radicalism and by arguing that Tkachev's writings influenced V. I. Lenin who, if not the first, remains the most important Bolshevik.

Born of "landowning peasants of modest means," Tkachev drifted toward radicalism while a university student. Subsequently arrested, tried and exiled for his conspiratorial activities, he escaped to Switzerland and joined the circle of radical Russian emigrees. Until his death in 1886, at the age of forty-one, Tkachev published the revolutionary journal *Nabat* (*Alarm*) and participated in the activities of secret societies dedicated to the destruction of the Tsarist regime. Professor Weeks contends that Tkachev's ideology was a form of Jacobin radicalism which contributed to the philosophical transition from Populism to Bolshevism among Russia's modernizing intellectuals in the final quarter of the century. Therefore, he has devoted the major portion of his book to clarifying the principles and identifying the problems of Tkachevism.

Weeks focuses first upon Herzen's Populist movement of the 1840's which eventually yielded to the more extreme *raznochinstvo* philosophy of men like Belinsky, Chernyshevsky, Dobrolyubov and Pisarev. He maintains that Tkachev followed in this tradition and, in doing so, made certain essential contributions to Bolshevism. The most important of these were his concept of party organization and his belief that Russia was a special revolutionary case.

Tkachev had no faith in the masses as a revolutionary force. He believed that a strictly organized party of intellectuals, oriented toward the seizure of political power, was an absolute prerequisite to a successful revolution. Tkachev recognized the importance of propaganda and agitation in the post-revolutionary stage, and he envisioned a party dictatorship with no limits upon the means which it could employ to impose the new socialist order. Contrary to the strict Marxist revolutionary formula, Tkachev contended that revolution was possible in Russia because it was the "weak link" in the chain of capitalist nations. The source of Russia's uniqueness was, in Tkachev's mind, her instinctively and traditionally communistic peasantry.

The thesis that Tkachev was the "first Bolshevik" is more difficult to defend, however, with respect to other important aspects of his thought. The principles of Tkachev's economics, for example, are confusing. He wrote, ". . . all phenomena of a political, moral or intellectual nature . . . are the result of causal phenomena in the economic sphere" (p. 134). Notwithstanding such strains of Marxism, Tkachev's economic outlook was much closer to classical utilitarianism than it was to economic determinism, and Professor Weeks writes that, "Tkachev places the strongest emphasis on . . . the commanding position of politics over economics, over society . . ." (p. 112). Additional departures from Bolshevism's Marxist heritage which deserve mention include Tkachev's rejection of the notion of historical materialism and his low level of class consciousness manifested in his belief that the state could function above the interests of any particular class.

But doubt concerning Tkachev's adherence to Marxist principles would be secondary if his philosophical influence upon Lenin could be firmly established. With the exception of the title of his book, Professor Weeks is rather conservative in his conclusions on this point. There is simply no concrete evidence to prove that Lenin consciously or unconsciously drew upon Tkachev's writings for his ideas and, as Weeks notes, "to gauge the influence on one man of the reading of another's books is a most delicate task" (p. 74). Having admitted this, Weeks can only make the conditional statement that, "in the writings of Tkachev there may be found certain significant concepts which may be described as proto-Bolshevik" (p. 74). The task of measuring Tkachev's contribution to the development of Bolshevism has been further frustrated

by the refusal of Lenin's Soviet heirs to recognize any ideological connection with Tkachevism. In his efforts to surmount these difficulties, Professor Weeks has overemphasized the existing similarities between the ideas of Tkachev and Lenin, thereby jeopardizing the balance of an otherwise thoughtful analysis.

Even so, this study of Tkachev remains an extremely cogent contribution to the literature on Bolshevik history. Professor Weeks' heavy emphasis upon Tkachev's own words, most of which he has translated for the first time, along with the complete listing of Tkachev's works which he has appended are sufficient justification for considering this an important volume. The author's insightful analysis of the subject makes it essential reading for all students of Communism.—PETER R. ZWICK, *Louisiana State University.*

The Emergence of the Eastern World: Seven Essays on Political Ideas. By G. L. SEIDLER. (Oxford: Pergamon Press, 1968. Pp. 260. $10.00.)

Grzegorz Leopold Seidler is Rector of Marie Curie Sklodowska University at Lublin, Poland. His book is an attempt to remedy the neglect of Eastern political ideas that has long characterized the study of political thought in Western Europe and the United States. To that end, Seidler has written seven essays dealing with various aspects of Eastern political thought in the period between 3000 B.C. (in the case of Egypt and India) and the sixteenth century A.D.

Rather than undertaking a philosophical analysis of certain selected ideas, the author uses a genetic approach—political ideas are placed in a general socio-economic context and the origins and historical impact of the ideas are traced. In this respect, what Seidler says about one of his essays might well be applied to the whole book, namely, that it "will not bring to light any new facts, nor will it attempt to throw new light on the facts already known" (p. 227). When one considers the relative lack of familiarity most readers will have with the contents of this work, the kind of historical discussion it offers is probably what is initially needed to focus further attention on Eastern political thought.

The topics considered in the individual chapters extend from the very broad matter of ancient Eastern political ideas to the much narrower subject of the Jagiellonian monarchy in Poland. The first essay discusses the beginnings of Eastern political thought (and, indeed, of political thought itself) in ancient China, India, Babylon, Israel, and Egypt. Here emphasis is placed on the religious sources of the political doctrines of the ancient Eastern states, with most attention given to China and India. The period between the fall of Rome and the Hundred Year's War is represented by three essays dealing with "Islam as a Political Doctrine," "Byzantine Political Thought," and "The Political Doctrine of the Mongols." In the fifth chapter, "Political Thought in the Period of Transition (1350–1450)," the decline of European universalism provides the theme for discussions of the Conciliar Movement and the rise of religio-national opposition stemming from John Wyclif in England and John Hus in Bohemia; the collapse of the Islamic universal Empire sets the stage for a portrayal of Ibn Khaldun as the proponent of a new political realism. Two more specialized essays conclude the book: "The Muscovite Autocracy" and "The Jagiellonion Dynasty."

To take the book as a whole, certain peculiarities of interpretation stand out in what is for the most part an objective, if pedestrian, account of Eastern political ideas. Throughout the work, Seidler places special emphasis on class conflict and on the class character of political ideas. The author often lapses into what appear to be Marxian phraseology and formulae in explaining political ideas in terms of the correspondence or non-correspondence of social relations and structural forms to economic factors. This same line of thought is also suggested by the author's response to Sir Ernest Barker's observation that Byzantine political thought displays little originality or creativity. Seidler comments: "The value of political doctrines does not depend on the degree of their originality but on the social function they fulfilled or fulfil. The strength and importance of a given political idea is decided first of all by the degree in which it expresses class interests and by its effect on social relations and legal institutions." (p. 140).

Given this interpretive bias, which stresses "objective material factors" in understanding the origin and significance of political ideas, it is not surprising that the author also eschews what he calls subjective or idealistic interpretations of politico-religious ideas. In the case of Mohammed, for instance, Seidler dismisses as "typically idealistic" (p. 90) Tor Andrae's thesis that the personality of the Prophet was the dominant factor in the rise of Islam and asserts that Mohammed, though unaware of it himself, "was the man in whom the objective development of Arabia was mirrored" (p. 69). In a similar vein is the comment that the visions experienced by Mohammed at the outset of his career as religious leader were the product of "his nervous and sickly imagination" (p. 61). In sum, the various essays exhibit a tendency to deny any significance to politico-religious ideas and doctrines aside from the extent to which they manifest class interests or economic forces and serve as ideological weapons in the class struggle.

G. L. Seidler has written other works on jurisprudence and the history of political thought, but this is his first book to appear in English. Frankly,

it is not well-written. There are numerous failures of style and syntax. The essays are developed in disjointed fashion and tend to be repetitive.

Despite its defects, however, this book does help to fill the gap in our knowledge of Eastern political thought. The work is noteworthy because it brings together under one cover brief surveys of the main schools of Eastern political ideas. The essays, particularly those on Byzantine thought, the Muscovite autocracy, and the Jagiellonian dynasty, draw heavily on Eastern European and Russian sources which may be unknown or less accessible to students of Eastern political thought in the United States.—WILLIAM J. KIRSCH, *Arizona State University.*

AMERICAN GOVERNMENT AND POLITICS

Democracy and the Public Service. By FREDERICK C. MOSHER (New York: Oxford University Press, 1968. Pp. 219, $4.75.)

Reviewing one of the numerous Brookings Institution studies of our public bureaucrats a few years ago, the author of the book presently under review said: "One concludes this work like other studies of this kind, with a sense of vague disquiet: that he has added significantly to his knowledge; but that he needs a conceptual framework about American society and its government within which, or against which, to fit the empirical findings." (This REVIEW, September, 1966, p. 700.)

One suspects that this was the goal of Mosher's present study, the first in a new series on Public Administration and Democracy under the general editorship of Roscoe C. Martin of Syracuse. And it must be said that the author brings to his quest a distinguished reputation in the field, both as an extensively published scholar and as a past holder of several national administration positions. On the record, then, Frederick Mosher merits an immediate audience when he speaks to the subject of democracy and the public service.

Eschewing at the outset an extensive definitional analysis of the meanings of democracy, Mosher rather constructs a syndrome of social and political characteristics, the nature of which, taken together, is designed to give us a reading on the democratic health of the public service. Most of these characteristics are familiar: Responsibility, Representativeness, Mobility, Participation, Non-Elitism, and the Rights of Public Servants. In summary fashion, the author proceeds to comment on each. "Responsibility" is ambiguous as to authority (legal and political accountability vs. subjective allegiances). "Representativeness" is complex (active vs. passive; legal vs. political interest). "Mobility" is misleading (the classical rise in class vs. horizontal-field, public-private, national-state-local, or other possible planes of movement). "Participation" is popular today, but dilemma-ridden as it applies to administration (the imperatives of rational, institutional logic vs. pluralistic, democratic involvement). "Elitism" is *a priori* undemocratic. (A single, cohesive national elite is intolerable in theory and unverifiable in practice, but many specialized sub-elities appear active. Mills and Hunter went too far.) Finally, public servants should have citizens' rights, but these are infringed in many ways (restrictions imposed by loyalty-security programs, narrowly limited rights of collective bargaining, and so on.)

These then are the major problem areas. The remainder of the book is devoted largely to a description of the appointive public bureaucracy in the United States, relating characteristics of the service to the categories of concern for democracy. Mosher makes ample use of recent research literature on bureaucracy dealing with social backgrounds, social characteristics, image, etc. In addition, he supplies personal evaluation and reasoned assumptions based upon his own experience. About one-third of the book is devoted to the history of the relationship between the public service and education and to the evolution of civil service concepts from 1789 to the present ("Government by Gentlemen" to "Government by Administrators").

While what has gone before its informative (the degree depending, of course, on one's background) it is at page 99, when the author embarks on an analysis of "The Professional State" (Chapter IV), that interest really livens. At this point we are led through an examination of that characteristic of the higher bureaucracy that has most recently emerged and that is most portentous both for the bureaucracy itself and for democracy. In numbers, over one-third of all (U.S. Census defined) professionals are employed by American governments and these governments employ more than three times as many professionals as private organizations. Indeed government even stimulates the creation of new ("emergent") professions—some of which have little or no employment market but government, e.g.—penologists and air pollution specialists.

This phenomenon raises a host of questions about its impact and only tentative answers can be offered while many intriguing hypotheses are prompted. The prodigious specialization that one

finds in the public service—specialized expertise based upon narrow and deep training—has resulted in a proliferation of varying experiential backgrounds, approaches, methodologies, mentalities, self-perceptions, and desires. Given the proportionate size and pluralistic make-up of professionals in the public service, what use can one expect to make of traditional ideas about hierarchy, authority, efficiency, merit and conflict in administration? Perhaps something like the collegiality of professors has largely replaced hierarchical authority among professionals in government. Perhaps the professionalization of the public service throws new light on the old Friedrich-Finer debate about the nature of administrative responsibility, light incidentally, that seems to support Friedrich's side if we assume a more pervasive commitment to one's professional orientation. Perhaps, as Mosher's modestly stated "unproved observation" suggests, key conflict areas are those between different professions or segments of professions in different agencies, rather than between management and the workers.

In thinking about the future, Mosher in the end offers a key: higher education in America. Thus, at bottom, is is implied (and this reviewer agrees) that if we are to shape the future of the public service along democratic lines, our options are probably more dependent upon what comes to pass on the campuses of the nation than on institutional modifications or "reforms" within the bureaucracy. There are very real congruencies between what Jencks and Riesman are talking about in *The Academic Revolution* and what Mosher considers here. It may not be a novel idea that the future of the public bureaucracy is inextricably tied up with the ongoing organizational revolution in our society, but it is an idea which finds additional evidence and dimension in this book.

In sum, *Democracy and the Public Service* is a useful and informative synthesis of what we need to know about the public service now and to some extent, a suggestive one about where we might forage for intelligence concerning the future of the relationship considered. In fairness, it must be said that what we do not have here is a rigorous and comprehensive new model blending theory and data, but one could have a far worse organizing principle than the qualities of democracy for presenting facts about and trends in American bureaucracy. For students (especially since it is available in paperback edition) of administration and government, it offers much more of interest and importance than the traditional three or four textbook chapters on personnel administration.—CHARLES E. JACOB, *Rutgers University.*

The Information Machine: The United States Information Agency and American Foreign Policy. BY ROBERT E. ELDER. (Syracuse, New York: Syracuse University Press, 1968. Pp. 356. $10.00).

If the title and Table of Contents of a book faithfully predicted what its covers contained, this study of the United States Information Agency should be one of compelling interest to students of American foreign policy and administrative decision-making. In his Preface, professor Elder promises to reveal "how USIA presently functions, how it receives policy guidance or influences the formulation of foreign policy, how it translates foreign policy into information policy, and how it transforms policy into programs and products." His analysis, he announces and further whets the appetite of a potential reader, will focus on a complex web of "inter- and intra-agency contacts, of lateral and vertical relationships, with emphasis upon interactions affecting policy, program, or product." To a considerable extent, the author delivers on his promise. At great length and at times in exasperating detail Professor Elder describes the evolution of USIA, its changing missions and its somewhat awkward role in the confusing network of foreign policy-making and maps the various streams of decision and action within that network through which the agency attempts (a) to explain America to the world, (b) to support positively and pointedly America's foreign policy objectives, and (c) to establish credibility for both to a skeptical global audience. This is an ambitious assignment for USIA and if Professor Elder's study does not completely meet the requirements he set for himself, the explanation may lie as much in the agency's gropings as in the author's conceptual and methodological problems.

An information function serving foreign policy needs existed off and on long before the United States Agency was created in 1953, although it tended by be "psychwar" in purpose and was designed to meet the strategic needs of military operations. It was not until 1948, and then largely in response to intensifying Soviet propaganda efforts in Europe and the crystallization of the Cold War, that the Congress specifically authorized within the Department of State a "worldwide information and cultural exchange program." Therefore, when an independent information agency was created by President Eisenhower's Reorganization Plan No. 8 in 1953, the basic components of what became USIA were already in operation. Several of those components—the Voice of America, the USIS libraries, the binational centers, the Russian language periodical *America Illustrated*—became familiar to Americans at home even though none of USIA's activities are directed to or consumed by the domestic market.

Professor Elder's emphasis is on USIA as a complex, flexible, far-flung organization, responding over the years to an array of influences and

changes in mission. His analytical framework develops around the agency's structure (the Office of the Director, the Office of Policy and Research, the Area Offices, and the Media Services) and its functional responsibilities. The evolution of each organizational and functional component is described, frequently at some length, and each component's relationship internally to other USIA units and functions and externally to counterpart or complementary units and functions elsewhere in the Federal government, particularly in State and the Executive Office of the President, is mapped. Of considerable interest is Elder's rather skillful presentation of the impact of the organization environment—policy values, leadership styles, technological change both in a mechanical (advances in communications technology) and a cultural sense (new kinds of professionalism and expertise flowing from these advances)—on agency operations. His frequent discursive and detailed explorations of intra-agency relationships are both frustrating and tantalizing; it is difficult to decide whether they are an attempt to capture the "reality" of USIA existence through the "typical day in the life of an administrator" technique or an effort to *narrate* functional flow diagrams with words rather than through graphic presentations. The net effect, however tiresome the detours may seem at times, is a multi-dimensional grasp of the total organization.

USIA has had a turbulent, brief existence,one characterized by dramatic shifts in mission and alterations in organizational behavior. As Cold War tensions eased in mid-'fifties, the strident anti-communist propaganda themes that permeated agency operations gave way to softer tones designed to establish and broden USIA credibility with its overseas audiences. The late 'fifties found the agency primarily concerned with presenting the "full and fair picture" of America. The policy values guiding program emphasis shifted dramatically with the advent of the Kennedy Administration as programming increasingly was directed to selected audiences and designed "to achieve maximum influence" in support of specific foreign policy objectives. Under the Johnson Administration the "politicization of information policy" has been balanced by a renewed concern for cultural affairs and a new effort to tailor "media products" to suit individual country or regional requirements.

Each new turn in agency mission brought with it organizational adjustments that Professor Elder describes and analyzes in fine detail. The shifting functional roles in country planning, media services, and research and the fluid headquarters-field and USIA-State-White House relationships are exhaustively and at times exhaustingly documented. Since the book traces these adjustments and shifts for each organizational and functional component the reader soon is aware of a growing narrative redundancy which, given the conceptual and methodological structure used by the author, is perhaps unavoidable. Thus, the potential that PPBS has for USIA effectiveness is chewed over at least a dozen times, and the tensions between the "civil service" media specialists and the Foreign Service Career Reservists are analysed at least as many times.

Despite these caveats, I felt the volume a worthy investment of time for serious students of administrative behavior and American foreign policy. It treats with skill the persistent and still unresolved difficulty that any information-propaganda-cultural exchange function encounters in serving changing policy objectives which are pursued by a government committed, at least on the home front, to a free press and an open society. The continuous efforts to assure that policy shifts and shadings are accurately and functionally reflected in media output, particularly the so-called "fast media" of press and radio services, echo themes familiar to students of administrative planning and control. Finally, USIA's efforts to implement PPB through its comprehensive country programming system also strike a current note and reveal in a way that Elder is perhaps not aware of a major defect in the Federal PPB effort that purports to plan and program within departmental and agency program structures without the necessary "cross-walks" for inter-agency and inter-program coordination and decision that a government-wide program structure would provide. Without some such "common denominator" for program analysis USIA's efforts to work effectively with State, AID, and other members of the foreign policy team will be fraught with frustration.

This comprehensive study of USIA and the information function it contributes to the making and implementing of American foreign policy could have been a much more tightly organized volume. Both Professor Elder's study and USIA itself would benefit by a more deliberate cultivation of the applications of general systems theory and, perhaps symbolically, information theory. In time PPB may so inform the nation's "information machine." In the meantime, USIA will probably remain in a state of role uncertainty and mission ambiguity.—WILLIAM R. MONAT, *The Pennsylvania State University.*

Progressive Politics and Conservation: The Ballinger-Pinchot Affair. By JAMES PENICK, JR. (Chicago, Illinois: University of Chicago Press, 1968. Pp. 207. $7.50.)

Penick applies techniques of manuscript analysis to provide additional understanding of one of the most famous fights in the administration of American natural resources. Drawing upon the Balinger,

Pinchot, and Taft papers as well as the collections of several lesser figures of the time, Penick has identified and tried to set in fair perspective many facets of the controversy. The personalities, and the ideological conceptions, of the chief antagonists are woven into his story along with the perceptions the men had of the era, the issues, each other, and the various actions each took. A balanced presentation, from the standpoint of the warring factions, Penick's analysis neither vindicates Ballinger nor champions Pinchot, but rather sees each as acting righteously within his own convictions and within his conception of the public interest. Such judgment as he does levy, Penick reserves for Taft and Ballinger's mishandling of their side of the story, especially during the Congressional investigation of 1910.

Penick also tries to interpret the broader implications of the conservation movement and the meaning of the controversy in his opening and closing chapters. Both chapters, however, depend heavily upon the personal attitudes reflected in the papers the author reviewed and miss the type of insightful perspectives about the principal—and conflicting—elements within the conservation movement which Henry P. Caulfield, Jr. noted a decade ago. Both the preservationist and regulationist "thrusts" are too narrowly conceived, with, for example, the public power movement being entirely ignored. Thus Penick's conservation analysis centers mainly upon selected conflicts among development elements of the movement, chiefly those affecting the Land Office, the Forest Service, and the Reclamation Service. This focus leads him to conclude that the lesser issues ultimately revolved around the question of whether there was to be overall resource planning, and thus centralized management, or whether the various interests which wished to exploit public resources would be encouraged to continue their efforts. Ballinger, in these terms, is viewed as fighting centralized "bureaucracy" and applying business efficiences to resource management while Pinchot becomes the champion of scientific planning for resource management and thus of centralized, professionalized administration by qualified "non-political" specialists.

Although it contains many useful insights, Penick's book seems sure to win but few readers and to have but little application. An author's first obligation to his readers is to tell them clearly what he is writing about and to provide enough essential information so the reader can understand the issues and the action without having to conduct separate historical research. Penick fails this essential obligation. He assumes instead that all his readers will have a detailed knowledge of the issues and events which, in fact, but a handful of specialists possess. Thus he leaves out most of the key facts essential to understanding his story.

To compound his central failure, Penick never provides a consistent framework for his story. Instead he skips the reader back and forth from broad social concepts to individual personalities to public policy issues and then to the personal reactions of the participants in the sequence of bureaucratic infighting. Were the tale even chronological, the reader would have a fighting chance to follow him. But Penick prefers, as though he were adding a series of isolated historical footnotes to a story already told, to skip from topic to topic and back and forth in time without any seeming consistency. Thus his book degenerates into a series of enlightening footnotes about the reactions of individual participants to key events which he never presents or explains. For example, in the chapter on the Glavis charges, one never finds out what Glavis charged and in the chapter on the Congressional Investigation, one never learns what the investigating committee did or what it found!

Compounding this confusion is his extraordinarily difficult style of paragraph and sentence construction. He changes topics not only within paragraphs but within sentences as well and rarely bothers to open a paragraph with a topic sentence to aid the reader on his way.

Perhaps, however, public servants, politicians, professional conservationists, the interested public, and other scholars should not be too hasty to pass by such a work, even when its form of presentation submerges its valuable insights. The nation sorely needs more historians and other social scientists who can help us understand and interpret the issues and public policies which have affected our natural resources. Another recruit to the thin line of interested scholars is most welcome.—DANIEL M. OGDEN, JR., *U.S. Department of the Interior.*

Federalism in the Southern Confederacy. By CURTIS ARTHUR AMLUND. (Washington, D.C.: Public Affairs Press, 1966. Pp. 140. $4.50.)

With all the attention Americans have devoted to the Civil War in the past century, it is surprising how few political scientists have studied it as a phenomenon of politcal behavior. In fact, between the time of Burgess and Wilson, when studies of the constitutional issues surrounding secession and the rebellion itself were fashionable in the discipline, and our own the lack of interest in the War can be seen as a bench mark of political scientists' general lack of interest in the behavioral aspects of American political history. Recently we have seen signs of a change in this attitude and we are beginning to apply questions, methods and techniques developed for the study of contemporary political behavior to the past with increasing success.

Professor Amlund's small volume, apparently the revision of his doctoral dissertation at the University of Minnesota, represents a modest and useful effort to raise the kinds of questions political scientists have asked about intergovernmental relations in recent decades and apply them to the abortive Confederate States of America. He is particularly interested in the relationship between constitutional theory and practice within the Confederacy in light of the exigencies of war. He concludes that, despite nuances of constitutional differences, in some cases written into the fundamental law of the C.S.A., there was essentially little difference between federalism in the Southern republic and that of the old Union which the Southern states were trying to leave. Moreover, the necessities of national defense functioned to erase or lessen most of the differences that had been anticipated by the Confederacy's founding fathers. As a corollary, Professor Amlund points up the expansion of positive government, particularly at the state level, in an effort to meet wartime demands upon human and natural resources.

The book touches upon most aspects of intergovernmental relations in the Confederacy. After delineating the constitutional issues and the principles applied in the South to meet them, there follows a discussion of the presidency focusing on the exercise of presidential power by Jefferson Davis with his sometimes conflicting combination of rigid constitutionalism and Southern nationalism. The Confederate Congress and judiciary are treated, particularly in regard to the continuity of institutions, procedures and personnel from the "old Union." Separate chapters are devoted to finances, administration, and confederate-state relations. Amlund's data are derived from historical studies of the Confederate government and its leaders plus published documents of the C.S.A. By combining both kinds of sources, the author is able to add some sense of actual occurrences to the bare bones of legislation and court decisions, though the book's brevity and the lack of examination of primary sources preclude the development of supporting case studies, which would have been most useful in underscoring the points he makes.

This reviewer can hardly take exception to the book's thesis and conclusions. All the evidence he has examined points in the same direction. However, the author overlooks some complicating factors which need to be considered in assessing intergovernmental relations in the Confederacy. The division of opinion between Southern nationalists and orthodox states-righters, which outdated secession, continued as an important aspect of Confederate politics and was found at the root of most of the confederate-state conflicts that did occur, is virtually ignored. The problems of congressional and presidential weakness generated by the lack of a party system are not explored. Perhaps most significant, the problems of dual versus cooperative federalism are totally ignored, either as part of the intergovernmental setting out of which the Confederacy emerged or as an aspect of intergovernmental relations in the Civil War South. From reading this book, particularly in light of William B. Hesseltine's admirable *Lincoln and the War Governors*, one gets the strong impression that, while the North was capitalizing upon a *de facto* tradition of federal-state cooperation and deepening cooperative patterns as part of the war effort, the South was attempting to implement a theory of dual federalism against all practical reason, a fact which contributed in its own way to the downfall of the Confederacy. Finally, the very exciting question of bureaucratic persistence through political crisis is hinted at but not substantially raised.

Professor Amlund's book is a welcome addition to the small shelf of political studies of the Civil War. It does not attempt to exceed its self-imposed limitations. Accordingly, it raises many questions for further and deeper research into the problems of a federal system in crisis.—DANIEL J. ELAZAR, *Temple University.*

The Faces of Power: Constancy and Change in United States Foreign Policy From Truman to Johnson. By SEYOM BROWN (New York: Columbia University Press, 1968. Pp. 397. $8.95.)

The need for another book that reviews the major decisions of United States foreign policy in the post-World War II period might be questioned. But this book is an analysis with a difference—an important difference. Seyom Brown discusses United States policy from the perspective of how decision-makers in the United States viewed their adversaries and the alternatives as those decision-makers saw them. The author does not attempt to write a series of case studies, thus moving from one crisis to another, as is so often the case with foreign policy reviews.

The reader who is looking for more detailed and factual information about the many crises in which the United States has been involved since World War II will be disappointed. But if the reader is looking for an intelligent, well-reasoned interpretation of the patterns involved in foreign policy-making in the United States, this book is well worth the effort of a careful reading.

Brown recognizes that the task of interpreting how decision-makers perceive a situation is a difficult one. He admits that "any such analysis of the judgments of the nation's responsible policymakers is tentative and speculative. . . . Nor is there any logical formula by which one may infer, with a high degree of confidence, the basic assumptions

underlying observed official behavior" (p. 3). But Brown feels that "the public record" provides enough information that inferences about behavior can be made. Brown carefully points out that his only source of information is "the public record." Since the author wrote this book while on leave from his position on the staff of the RAND Corporation, his reference to the public record as his source of information is undoubtedly necessary. However, it is a fair assumption that the classified information Brown had available to him as a staff member of the Rand Corporation does not contradict "the public record" that he employs.

Brown begins his review of U.S. foreign policy by attempting to identify the "irreducible national interests" and the major premises on which U.S. policy is developed. The author recognizes the conceptual problem in using a term such as "irreducible national interest," but defends his use of the term as being necessary since policymakers debate at such length what this term encompasses and act as though such a no-retreat position exists. In effect, Brown defines "the irreducible national interest" situationally as whatever decision-makers feel the term means in a particular crisis. In his discussion of the various crises of American foreign policy, Brown, however, makes very limited reference to what is regarded as the "irreducible national interest" in those particular situations.

While some reference by Brown to the "irreducible national interest" is understandable, certain other terms he uses with little or no explanation are more difficult to understand. He uses the term "balance of power" apparently to mean any distribution of power existing in the international system and particularly the one existing between the Cold War blocs. After the lecturing Professor Claude (*Power and International Relations*) gave political scientists over their slip-shod use of various terms including balance of power, more attention to the manner in which Brown uses "balance of power" is appropriate. The same criticism could also be leveled at Brown's use of "bloc politics." He uses the term without explaining the sort of international behavior a reader should associate with this sort of politics. Potentially, however, Brown's analysis of United States policy makes for an interesting comparison with the "rules" of bipolarity that several political scientists have attempted to identify. In light of the objectives of this book, however, a rigorous conceptual framework is not necessary and the imprecise use of certain terms is not unduly distracting.

One of the most interesting aspects of this book is Brown's development of the assumptions, particularly about the Soviet Union, held by major policymakers. His discussion of Averell Harriman, John Foster Dulles, Dean Acheson, and President Eisenhower in this regard is particularly well developed, as is his account of the relationship between Eisenhower and Dulles.

What seems to be a major shortcoming in Brown's book is the lack of attention to domestic as opposed to international considerations in the development of policy. It may indeed be the case that "the public record" in regard to foreign policy decisions is generally stated in terms of international considerations, but the influence of domestic considerations cannot be ignored even if the public record does not place the proper emphasis on domestic influences. The almost total absence of any mention of the influence of McCarthyism in the early 1950's or the domestic pressures that the United States not allow the Dominican Republic to become another Cuba are examples. Brown might have said more about the policymakers' assumptions about the domestic system than he did.

One of the "constancies" that Brown observes in United States policy is that "each Administration [relied] heavily on the tools of physical coercion —sometimes as a visible and active ingredient in a particular situation—in order to control external threats to our national interests" (p. 365). This, Brown concludes, is constant during the past four presidential administrations. Since his analysis throughout the book has been to deal with crises such as Berlin, Laos, Vietnam, Cuban missiles, Dominican Republic and such, what might be a more appropriate conclusion is that the United States is most apt to express its physical power during a crisis than at other times and the more serious the crisis the greater the reliance on physical power. Brown has not, as he suggests, provided evidence that the United States relies so extensively on "physical coercion" in more routine diplomatic exchange.

It would not be difficult to challenge Brown's interpretation of the public record in drawing conclusions about the assumptions and alternatives in regard to various policy choices, but his analysis, even when debatable, is always well-thoughtout and carefully presented.—David N. Farnsworth, *Wichita State University.*

Government and Slum Housing: A Century of Frustration. By Lawrence M. Friedman. (Chicago, Illinois: Rand McNally and Company, 1968. Pp. 216. $8.00 cloth; $2.95 paper.)

Lawrence M. Friedman, a professor of law, has written a scholarly book dealing with the "authoritative promulgation of housing legislation" by Congress. His concern is with only a part of the housing policy process, and he points to the "stain of failure" which covers the way in which legal institutions have been brought to bear on this social problem.

He begins with the "negative" public programs —the path taken by tenement house laws, housing codes, etc. In the discussion of "positive" programs he emphasizes public housing. Next Friedman considers urban redevelopment and renewal, "uniting recent movements of mighty proposition in many elements of the past." He assesses proposals and programs which "constitute the opening wedge of the future," before he concludes with the lessons of housing history.

Defining slum housing in terms of its physical standards and in terms of the social conditions of slum neighborhoods, Friedman incorporates the two elements—physical and social—in his theoretical approach to an analysis of the problem. The social-cost approach and the welfare approach are challenged by people who believe that the slums are beneficial not only to society but to the people who live in slums. Their argument ends in strict residential segregation or apartheid. It is obvious, says Friedman, that race and income segregation have played powerful roles in preventing the passage of adequate kinds of housing legislation.

Public housing, the only alternative, is the government's answer to slum housing according to the author. The government can build decent, respectable houses and apartments and rent or give these to the poor. Since public housing has not worked magic, Friedman claims, it has been deserted by those who care less for the welfare of the inhabitants than for the assumed social costs of bad urban planning. If public housing needs to be reformed—and he assures us that it does—his suggestion is that one might ask tenants and potential tenants what should be done.

Working within a "limited" group theory of politics, Friedman says, that diverse "interests" are responsible for the making of housing law. They are direct pocket-book interests (downtown merchants), institutional, material, emotional, status, as well as reform interests. Direct materials interests are more powerful than reform or ideological, which are insufficient to pass major housing legislation. Since people's perception of their interests are important factors in determining their behavior, the reformers must define or redefine the situation—the need for public housing—in such a manner that major economic and social interest will not oppose it.

Friedman also assures us that governmental institutions affect political behavior and play a large role in maintaining housing laws. Through federalism administration can be local and give free play to the operation of experts, professionals, and civil servants, somewhat independently of municipal politicians. The complex, mixed federal-state-local structure of housing law presents reformers and pressure groups with problems and opportunities. Urban renewal is not easily diverted or destroyed. It has many ways to disarm the opposition; promises, beautification, humanitarianism, abolish poverty, and money.

Despite the setbacks of the past, Friedman assures us there are several reasons for guarded optimism. The militancy of the civil rights movement has spilled over into slum housing area, and through militant action the poor Negro may persuade the white, middle-class majority that its own self-interest requires concessions. The reform forces, as well as the academic community, the churches, H.U.D., and the model cities program are engaged in mobilizing resources toward a solution of the problem.

Friedman believes that genuine reform through legislation is politically possible; the housing problems of the poor can be solved. The government, however, must use carrots not sticks. The country must redefine its goals: it must decide to house the poor. The level of public housing construction programs has never been high enough to fill the need.

He claims that the boldest way is also the simplest; vastly to increase the subsidy and radically to diminish the punitive element in housing legislation. We ought to "swallow our distaste and look for ways to make it profitable to own houses in the slums, to make it as profitable as selling bread to the poor or showing them movies, which are activities carried on by respectable people." Rent subsidy is a timid step toward encouraging construction of housing for the poor. It could be improved, emboldened, and expanded.

The dominant motifs of Friedman's book on slum housing become: concern for the quality of life, the need for getting the people into the act, and confidence in the efficacy of the private sector in social endeavors. The Johnson housing bill of August 1, 1968, includes the same ideas; interest-rate subsidy for the home ownership program, a subsidy-rental program, a new Federal-private insurance program, an accelerated public housing program, more urban renewal, a "new towns" program, and a "housing partnership" program which will attract wealthy individual and corporate investors.

Because of the focus of his inquiry, Professor Friedman necessarily had to give a limited analysis of the politics of public housing policy process. I would suggest that Friedman's historical survey leaves less room for optimism than he indicates. Policy and process are inseparable; to change the policy, the process must be altered. This is true when change is incremental, much less fundamental. A group theory of politics almost by definition leaves little confidence that our housing goals will be clearly "redefined" as Professor Friedman asks and even less confidence that the public housing

policy process will produce the "simple" incremental solutions which he proposes. If the public housing policy process is a "group process" as he suggests and it has produced legislation which has "failed" to solve the problem of slum housing to date, I see no reason why we should have confidence that the traditional ways of doing things will redefine the situation. The "private-sector" approach has proven to be a deadend for solving the problem. More of the same seems inadequate.—MYRON Q. HALE, *Purdue University*.

Aid, Influence, and Foreign Policy. By JOAN M. NELSON. (New York: The Macmillan Company, 1968. Pp. 149. $1.75, paper.)

The Strategy of Foreign Aid. By LLOYD D. BLACK. (Princeton: D. Van Nostrand Company, Inc., 1968. Pp. 176. $5.95.)

The titles of these two books do not indicate the significant difference in subject matter, approach and purpose of the two authors. Joan Nelson's *Aid, Influence, and Foreign Policy* is a perceptive, conceptual analysis by an "insider" of aid policy-making in the Agency for International Development. After a brief historical introduction, the first half of her book considers the Agency's goals, programs, and program planning processes; the second half is an analysis of the influence of A.I.D. programs in improving recipients' development policies, achieving short-run U.S. political purposes, and promoting long-run political development in recipient countries. Lloyd Black's *The Strategy of Foreign Aid* discusses aid on a much broader scale, outlining United States, Free World, Communist bloc, and multilateral aid programs as well as the problems and needs of recipient countries. With this scope, the book becomes a useful overall compendium of facts and figures on aid programs, and a summary review of concepts, processes, and problems of economic development.

Miss Nelson views herself as a "rapporteur of ideas developed from 1962 to 1966 by past and present members of the Program Coordination Staff of the Agency for International Development." Dr. Black hopes to provide "a comprehensive factual tableau of the economic aid situation which will make the wheels of intellectual curiosity turn a bit faster." The first book is essentially conceptual, would serve as an excellent supplemental text, contains a single chart. The second book, though primarily factual, raises many of the problems and policy issues of development. It would be particularly useful for reference, contains 15 tables, 8 maps, and two graphs, and is footnoted and indexed.

In introduction, *Aid, Influence and Foreign Policy* highlights the changing geographic allocation of U.S. aid funds over time from Europe to the Far East, to the Near East and South Asia, and finally to growth of assistance to Latin America, citing U.S. aid levels of $3.41 billion for 1949–52; $1.82 billion for 1953–57; $1.85 billion for 1958–61; and $2.3 billion for 1962–66. Thus, the 1968 cuts in aid tend to restore the 1953–61 level. Roughly two thirds of A.I.D.'s effort in recent years has gone for economic, political, and social development, about one fifth for immediate defense or security problems, and a lesser amount for "humanitarian" purposes. Miss Nelson notes a tendency within the Executive Branch to downgrade the importance of the inevitable conflicts between these A.I.D. objectives and the negative side-effects of their interaction on the success of programs.

Then the author discusses the criteria for allocation of A.I.D funds, the degree of dispersion of funds, program planning principles, and major limits on the effectiveness of A.I.D programs. The three basic criteria considered in allocating funds are said to be the political importance of the country's development to the United States, the ability of the country to use aid effectively, and the availability of resources from other sources. Aid went to 74 countries in FY 1966, but the author argues that it was not so dispersed that it lost effectiveness, pointing out that half of all funds went to 9 countries, that 35 to 40 others received a total of only two per cent of A.I.D. funds for limited objective programs. Concerning program planning principles, Miss Nelson notes that three are accepted and practiced: Programming by country or region, concentration on a few high-priority goals, and identification of self-help measures to be taken by the recipient country. Accepted as principles but practiced less effectively, she says, are long-run planning and systematic analysis of alternatives. She sees the major limits upon achievement of A.I.D.'s program objectives as being the Agency's multiple and conflicting objectives, the necessity of working with independent governments, and the relatively small size of the U.S. fund imput compared to the recipient nations' own development efforts.

The second half of the book discusses the use of economic aid in each of its three forms (technical, capital, and commodity assistance) for improved development policies; short-run political purposes, and long-term political development. This discussion of the use of aid to gain influence or leverage is a valuable contribution to the knowledge of "outsiders," because it gives specific illustrations of the impact of aid upon recipients' domestic and foreign policies. In conclusion, the author calls lack of attention to political development problems the "missing dimension in U.S. foreign policy." She recommends that A.I.D strengthen its capacity to assess these problems. Her approach throughout this carefully written and well-orga-

nized book is moderate, flexible, and objective.

The two most useful chapters in *The Strategy of Foreign Aid* are those on "The Recipients—Problems and Needs" and "The Donors—Dimensions, Scope, and Volume of Aid." Together they comprise 50 per cent of the book. Discussing recipients, Dr. Black raises "the obstacles, bottlenecks, and roadblocks" to development. As a geographer, he naturally starts with the physical environment, then presents problems of health, population, food, illiteracy, infrastructure, one-crop economics, trade gap, poverty, debt-servicing, management, and absorptive capacity. Emphasizing U.S. programs in treating donors. Dr. Black summarizes the programs of twenty-four other "Free World" bilateral aid programs before discussing "Sino-Soviet Bloc" and multilateral aid. Somehow, the use of the old bipolar dichotomy seems out of data in a more polycentric period. Discussing the "real reasons" for foreign aid in an earlier chapter, the author appears to accept as valid the idea that the leaders of the Soviet Union and Communist China are "uninhibited by moral or humanitarian considerations." This may be a misreading of what was intended, because similar strictures upon perception of the environment in which foreign aid functions are not apparent elsewhere in the book. Aid policies need to adjust not only to changes within recipient countries but also to those in the broader climate of international relations.

Dr. Black includes in his book a useful listing of what he calls "weaknesses, especially relating to the utilization of personnel in A.I.D." Drawing upon the views of A.I.D. personnel, he says that A.I.D decisions involve the representatives of too many agencies; field personnel are burdened by having to respond to too many requests for reports; mission directors do not have enough independence of action; field personnel must spend too much time with VIP visitors; personnel receive insufficient orientation before going overseas; officers on Washington assignment are rotated through too many jobs in a single tour; the Agency lacks a process for retrieving information from its past studies; the Agency's economists don't recognize the value of insights from other disciplines; foreign personnel brought to the U.S. by A.I.D. training can't find jobs when they return home or want to remain in the United States. In fact, many of these or very similar problems also concern the other U.S. foreign affairs agencies and some of the U.S. domestic affairs agencies with overseas interests.

Although *The Strategy of Foreign Aid* tends to be a rather comprehensive dictionary of foreign aid agencies and programs, it also raises many issues concerning aid policy which the student with "intellectual curiosity" will wish to pursue further after reading Dr. Black's helpful collation of facts and ideas concerning the "economic aid situation." —ROBERT E. ELDER, *Colgate University.*

Class, Race, and Labor: Working-Class Consciousness in Detroit. By JOHN C. LEGGETT. (New York: Oxford University Press, 1968. Pp. 252. $7.50.)

This study of working-class consciousness is based on a sample of three hundred seventy-five workers interviewed in 1960 in seven Detroit neighborhoods selected to provide similar numbers of Negro, East European, and Northwest European workers. The operational definition of class consciousness is a Guttman scale which classified without error 75 per cent of the respondents. Instead of relying on the individual's self-perception of his class identity, the usual indicator of class consciousness, Leggett's index consists of spontaneous use of class terms, a belief that profits go to businessmen when the economy prospers, stated willingness to engage in direct action, and advocacy of economic equality. This index apparently differs from simple class identification, for Leggett says at one point that "a high correlation" between his index and class self-identification "does not occur" (p. 216). Yet the author neither justifies his measure of class consciousness nor does he compare the differing correlates of the alternative indices.

This is significant only because Leggett implies a critique of the "many social scientists" who, "during the 1950's . . . abandoned their Marxian assumptions and turned to an examination of why U.S. workers lacked militant class attitudes" (p. 27). Leggett does little to make his critique more explicit. Did they use wrong measures of class consciousness? Did they code responses to open-ended questions incorrectly? Leggett seems willing to code as evidence of class verbalization responses that Campbell *et al.* in *The American Voter* (1960) would have coded relatively low in their index of capacity to conceptualize in ideological categories. Or did they study the wrong cities? Leggett seems to believe that Detroit, where social and political life is particularly conducive to the development of class consciousness, is the ideal location for such an investigation. Perhaps Leggett remains vague on these points simply because his data cannot demonstrate how important class consciousness is for working class, social and political life in general. He is limited to comparisons among subgroups in his working class population; any general interpretation inevitably rest upon the observer's judgment and his own definition of a *significant* level of class consciousness.

Even though Leggett is not able to resolve this question, his concern with the topic did spur him to explore the sources of whatever consciousness

does exist in Detroit. Of particular interest, he finds greater consciousness among recent migrants to the city than among those born in an industrial center, even when ethnicity is held constant. This finding not only contradicts one of Marx's major contentions but also, as Leggett notes, helps to explain the greater consciousness in Nineteenth than Twentieth Century European societies. In addition Leggett finds that unemployment correlates negatively with consciousness among Negroes except when the black worker is unionized. He also demonstrates that consciousness and union membership is related to voting for such liberal Democrats as Governor G. Mennen Williams. He makes the reasonable argument that such a relationship is contingent upon a liberal candidate backed by labor support running against a conservative. Had Leggett asked questions about other elections—particularly, about Detroit's local, non-partisan elections—more evidence for this assertion might have been possible.

Finally, he notes that consciousness is much greater among Negroes than among whites. Here it is worth noting the decisive effect that the recent racial crisis has had on social scientists. Even though Leggett began this study as a critic of the consensus school, his own interpretation of American society changed in the course of writing the book. This is revealed by the differences in the tone of the body of the book, which is largely based on previously published papers, and the recently written epilogue. In the analysis itself, the Negro community is treated as another ethnic group a step behind other groups in its integration into American society. In the epilogue however, the problems of blacks are so severe, so unique, that it is possible to imagine that Negroes, unlike other ethnic groups, may come "to prefer *insurrection* to voting" (p. 152). Similarly, whereas in the body of the book industrial unions are complimented for their contributions to class consciousness, a major subsection of the epilogue is entitled "Is Labor Failing the Worker?" (p. 148). It is no longer the business community but "the C.I.O., . . . the Democratic party, and like established organizations" which are held responsible for the conditions of Negroes (p. 153). This new mood is symbolized by the abrupt transition in terminology from "Negroes" to the "black community" as one begins reading the epilogue. Leggett probably differs but little from many social scientists in his changing view of the country's social and political institutions; it was his misfortune to have reported research which spanned the rapidly changing seventh decade.

Nonetheless, focus on these limitations of the book should not obscure the fact that it remains a solid empirical contribution to our understanding of the sources and consequences of class consciousness.—PAUL E. PETERSON, *University of Chicago.*

The Public Library and its Community: A Study of the Impact of Library Services in Five Pennsylvania Cities. By WILLIAM R. MONAT, with the assistance of Lawrence K. Pettit and Philip M. Clark. (State College, Pa.: Himes Printing Co., 1967. Pp. 162.)

This little volume is the seventh published as part of the Pennsylvania state library monograph series, but is the first in this line to focus explicitly on the relationship between certain public libraries and their politico-socio-economic environment. This study's major objectives were ambitious in nature and five in number—(1) to determine who uses these libraries, with what frequency and for what purposes, (2) to determine the attitude of library users and non-users alike toward that institution which many people who have never set a foot inside regard as "a good thing to have around," (3) to ascertain the amount and sources of financial support for library services, along with the attitudes and knowledge of concerned citizens about these financial arrangements, (4) to determine, in some rough way at least, how well these libraries meet the "needs" of both their users and the communities they purport to serve, and (5) the determine where these libraries fit into the overall governmental scheme of things in their respective areas. In order to carry out this task in a very thorough and conscientious manner, the authors of this study concentrated their efforts on five medium-sized cities in Pennsylvania.

Unfortunately, for a variety of reasons, the results of their analysis can only be described as rather mixed. The findings as to the public's use of library facilities, for example, are probably the most reliable in this particular survey, though they differ relatively little really from those arrived at by Bernard Berelson (*The Library's Public*) back in the late 1940's—namely, that only about 20 to 25 percent of the adult population use the library much at all and most of these tend to be young, comparatively well-educated, female, and middle-class. Similarly, the vast majority of the respondents in this survey expressed general satisfaction with their library facilities and services. Just what interpretation should be placed on this finding, however, is a matter of doubt or opinion, for as the authors themselves observe, "the consumer of library services is not well-equipped to evaluate those services if he lacks standards or measures for rendering judgment. Not knowing what might or could be, the client is likely to conclude that whatever is, is good."

Perhaps the most significant set of data generated by this study has to do with the library users' (cardholders') knowledge of the level and sources of financial support for these five public library systems in Pennsylvania, together with an

analysis of user attitudes toward increased taxation and the question of whether libraries should be financed through general funds or a special library tax. These are important topics rarely treated at any length in the professional literature. As one might almost predict, the authors found that there was a great deal of ignorance, even among concerned citizens, as to existing financial arrangements for library services. That the majority of those queried stated that they did not know whether the libraries in their communities were adequately financed or not, the authors construed as indicating the possible existence of a sizable bloc of latent support for library improvement or expansion programs, an assumption some might care to challenge. Nevertheless, one point was made very clear by the respondents. They did not choose to pay for better library services through any kind of special taxes; all such funds must come out of the general treasury or be raised by other levels of government.

The question of how well the "needs" of both the users and non-users have been met by these libraries is, of course, a tricky one, and one not dealt with very satisfactorily in this volume. Nowhere is this problem treated at any length. Perhaps the authors should not be judged too harshly in this regard, considering the paucity of productive effort of other researchers in the field. As it stands, the closest the authors come to providing any sort of answer here is by equating need with public and consumer satisfaction.

The authors' attempt to deal with public libraries as governmental units left something to be desired too. On the basis of their data, they concluded that because of the nature of library financing and their pattern of historical development, public libraries are not regarded or treated as agencies of local government, a point which is probably true. They also claim that the two most important factors in determining the actions of library boards were the confidence the board had in the professional competence of the chief librarian or library administrator and, secondly, in the extent to which the library depended on local public funds to finance its operations and the nature of that relationship. However, inasmuch as this study is based on an analysis of five public library systems, it is difficult to tell just how much stock should be put in such generalizations; perhaps very little. Moreover, this Pennsylvania survey contains virtually no information about the web of library group relationships in contrast, for instance, to Oliver Garceau's *The Public Library in the Political Process*, a work which is in many ways still clearly superior. Thus from a political science standpoint in particular, this book is of only limited value.—PHILIP H. BURCH, JR., *Rutgers, The State University*

Illinois Municipal Finance: A Political and Economic Analysis. By GLENN W. FISHER AND ROBERT P. FAIRBANKS. (Urbana: University of Illinois Press, 1968. Pp. 242. $7.50.)

To date there have been very few studies focusing on municipal finance within only one state. The value of this approach is that it eliminates the disturbing effects of variations in functional responsibilities of municipal fiscal systems found in other states. Therefore, *Illinois Municipal Finance* is a welcome addition to the literature.

The main effort of the book concentrates on a detailed discussion of Illinois municipal property taxes, tax limits, non property revenue, and indebtedness using 1962 data in the main. Two major criticisms of the Illinois municipal finance system presented in the study concern assessment practices and tax limits.

In the case of assessment the authors point out that even though the Illinois State Department of Revenue was instructed in 1945 to assess property at full value, "Revenue Department Figures show that the ratio has declined from about 75 per cent in 1945 to 48 per cent in 1957" (p. 77).

In terms of tax limits the authors express concern over the usefulness of this legal constraint. "Evidence of what the rate limits happen to be at a given time is not easy to find, and once found, it is difficult to evaluate" (p. 93).

In addition, the authors reveal that the confusion over tax limits works for the benefit of the municipalities which regularly extend these limits causing railroads and other industries to pay their taxes under protest as a matter of police. "Industries other than railroads often pay their taxes under protest too" (p. 94).

The major weakness of this study lies in its methodology. First of all the study would have been considerably enhanced if it took a more longitudinal approach in an effort to trace the development of the system. In the few attempts that were made to use longitudinal data, such as Table 36 illustrating gross debt outstanding from 1902-62, no effort was made to put the dollar amounts in constant dollars which would have given greater meaning to the purpose of the tabulation to say the least.

The third chapter, "Municipal Expenditures," is by far the weakest section of the study. Although a great deal of information is presented in this chapter, it is very descriptive with little effort being made in the direction of meaningful analysis.

Many statements are made which have little documentation. For example the observation that "analysis of data for individual cities reveal that there are substantial differences in expenditures per capita among Illinois cities" (p. 59) is made

despite the fact that neither the standard deviation nor the coefficient of variation is ever presented to substantiate the claim.

The discussions of the various individual expenditures are really rather trifling, *e.g.*, three quarters of a page is devoted to the discussion on parks and recreation expenditures not including a one page table accompanying the discussion which rated only one paragraph of comment. The authors conclude the third chapter with the observation that economic and political characteristics of the cities may be related to expenditure decisions (p. 63).

Why did the authors completely overlook the regression equations in Appendix A when discussing expenditures? On the whole the regression analyses showed that per capita assessed valuation was the most important determinant of the expenditures analyzed with various demographic measures and tax limits being secondary but significant predictors. It seems as if the authors are more concerned about straddling both the traditional and more empirical methodologies in the interests of harmony than they are in solid research.

The unusual presentation of the regression analysis in the appendix is defended on the ground that the authors wanted ". . . to avoid the impression of overemphasis on the importance of multiple correlation analysis" (p. 211). In addition, the discussion is prefaced by seven criticisms of multiple correlation analysis.

It is, of course, true that regression shows only correlation and not causation. Based on correlation, however, the analysis does show relative importance of variables as well as the direction of association including level of confidence that this is not a random association. In addition the percent of the variance explained by the equation is given plus a wide range of other relevant information which space precludes from discussing.

The problem of intercorrelating variables can be handled in the stepwise regression. Basically the researcher must establish what level of intercorrelation is unacceptable and eliminate all variables interacting above this level which are least important to the equation stepwise. While this method does not solve all the effects of interaction, it does make the analysis more manageable and meaningful.

The seven objections made to regression analysis do raise pertinent issues. But too much can also be made of the shortcomings of the technique. There has been a host of literature in recent years employing multiple correlation analysis to good advantage in reaching greater understanding of the dynamics of the municipal fiscal system. Research methods cannot be rejected because they are complex and do not answer all questions with absolute certainty.

While this study will not have great interest to the political scientist being more economics in tone, there will be interest for the specialist in the Illinois municipal fiscal system. However, a more systematic methodological approach could have resulted in a significantly more valuable study in terms of theoretical interest and positive findings. —STEPHEN W. HARTMAN, *University of Connecticut.*

Toward Equal Opportunity: A Study of State and Local Antidiscrimination Laws. By DUANE LOCKARD. (New York: The Macmillan Company, 1968. Pp. 150. $1.75 paper.)

The racial crisis lies at the heart of America's urban problems today. Negro-white antagonism and conflict over housing, public schools, and community order prevent the development of comprehensive solutions to the problems of our cities. State and local governments are unable to alleviate these conflicts either because of long-standing institutional barriers to change or simply because of a defense of the white community to preserve the status quo at the expense of Negro demands.

We are living through a time of confrontation politics in our cities: whites and blacks are polarized over crucial matters of public policy. This is especially true in transitional lower middle class white neighborhoods bordering expanding Black ghetto areas. During a time of confrontation, it is difficult to maintain communications between the races. While Black leaders are employing a variety of direct action techniques to dramatize the need for change, proponents of "white backlash" argue that Negroes are trying to change things too fast by means of integration. What Black leaders really want is community control over ghetto institutions. As the communications gap widens, there is a real possibility that the confrontation politics of today may yield to the politics of repression of tomorrow. Black ghettos may become permanent armed garrisons patrolled by the National Guard. (Wilmington, Delaware is already experiencing this.)

Duane Lockard's book illustrates the inadequacies of the traditional, civil rights-oriented approach to Negro inequality in America. State and local antidiscrimination laws in employment, housing, and public accommodations did not benefit the mass of impoverished ghetto Negroes. The legal approach, however, was the only public response to racial discrimination that could be expected from state and local political systems. Such laws were an attempt to provide equal opportunity for individual complainants through a slow process of adjustments by means of administrative machinery.

What, then, are the values of studying state and

local antidiscrimination laws? Lockard does show that it is necessary to understand the complexities of the legislative process before analyzing the impact and effectiveness of such measures. In Chapter 2, he clearly identifies the actors in the legislative struggle, together with the strategies and tactics employed to promote or block action.

Secondly, the author is realistic in evaluating the administration and impact of employment, fair housing, and public accommodations laws in Chapters 3, 4, and 5. He notes that antidiscrimination laws rely mainly on individual complaint initiative rather than agency prosecution of discrimination; that agency procedures are complex, time-consuming, and involve much delay; that agencies often lack injunctive powers to retain jurisdiction over disputed jobs or property during proceedings; and that agencies are often timid in the investigation and prosecution of cases. Even more important, antidiscrimination measures are used primarily by middle class Negroes who oftentimes become discouraged since most employment and housing disputes are not settled satisfactorily.

Lockard recommends that the full potential of these laws should be tapped by improving administrative procedures. Agencies should attempt pattern-centered enforcement in entire industries and neighborhoods as well as informing ghetto residents of the availability of services. Furthermore, legislatures should provide adequate funding and staff services so that the agencies can carry out their legal mandates. While pessimistic about the ability of antidiscrimination laws to assist the entire Black community, Lockard claims that such measures can at least aid in reducing overall patterns of discrimination.

The book falls short, however, in not discussing the "new equality" presently sought by Black leaders in the ghetto. It seems that community control over ghetto institutions is the key to understanding Negro drives for equal opportunity today. Black inner-city residents must have the resources to attain meaningful political power before they can compete on equal terms with the majority white community. As a young Negro medical student recently told me, "Civil rights activity moved the Black community from hopelessness to hope; Black power is the means by which the Black poor will move from a position of powerlessness to power." If this is true, then it may be possible to test the effectiveness of democracy at the grass roots as a viable goal for the Black community. Such political power will perhaps enable new ghetto representatives to define the terms under which future legislative struggles will be fought to eradicate all discrimination in jobs, housing, and education.—ALAN SHANK, *Boston University.*

Crisis in Print: Desegregation and the Press in Tennessee. By HUGH DAVIS GRAHAM. (Nashville: Vanderbilt University Press, 1967. Pp. 338. $7.50.)

This is a straightforward and thoroughly readable description of the response of the press of Tennessee to matters racial from the momentous *Brown* decision to the mid-1960's. Graham's method is to take the major race-related developments originating in or intruding into Tennessee politics—including the school desegregation decisions, the Civil Rights Acts of 1957, 1960 and 1964, and election contests between candidates of different racial persuasions—and to describe the editorial reactions of the various Tennessee newspapers to these. Although he surveyed the 122 weekly and semi-weekly, and the 27 daily papers in the state, Graham devotes most of his attention to the eight big dailies, the *Journal* and the *News-Sentinel* of Knoxville, the *Times* and *News-Free Press* of Chattanooga, Nashville's *Banner* and *Tennessean,* and the *Commercial Appeal* and *Press-Scimitar* in Memphis.

Crisis in Print is not likely to startle even the casual observer of southern politics. What do we learn? (1) Just as Tennessee racially is one light year away from Alabama and Mississippi which it borders, so the response of its papers to the assault on the old biracial system in the decade after *Brown* was decidedly different from that of the press in its deep-South neighbors. Tennessee is a rim-South state. Its eastern third has few Negroes; the entire state was only 16.5% Negro in 1960 (in contrast to 42% in Mississippi); two of the seven southerners in the U.S. House of Representatives to vote for the Civil Rights Act of 1964 were Tennesseans (with Texas and Florida supplying the other five supporting votes). It is hardly surprising that the state's newspapers didn't display single-minded opposition to desegregation, and that some indeed gently applauded the big civil rights gains of the period. Four of the eight dailies listed above, for example, editorially supported the 1964 Civil Rights Act. (2) If Tennessee isn't Mississippi in its racial tradition, neither is it New York. The generally pro-civil rights papers of the state frequently saw fit to restrain their enthusiasm. The Chattanooga *Times,* the senior member of the Ochs family of papers, was far more guarded in its endorsement of the *Brown* decision than its more esteemed New York relative, quite likely out of some assessment of the tastes of its Chattanooga readers. (3) Graham reveals that the position of a Tennessee newspaper on civil rights controversies in the post-*Brown* decade could pretty much be predicted from its general position on a "progressive-conservative" continuum. That is, the four otherwise progressive

dailies among Tennessee's "big eight" gave their editorial blessings to the end of Jim Crow, and the conservative papers regularly voiced displeasure.

One of the reasons why *Crisis in Print* isn't of greater value to the political scientist is that it was written for a much more general readership. In the Introduction, the author voices the understandable dismay of a liberal-minded southerner that many northerners insist on seeing the South as an undifferentiated stomping ground of Tillmans and Bilboes in the past and now of Thurmonds and Wallaces. He thinks that the case of Tennessee and specifically of the Tennessee press demonstrates clearly that this just isn't so. Well, of course it does, but hopefully not too many students of southern politics needed to be freed from such a delusion.

I wish Professor Graham had paid at least some attention to two important questions. First, newspapers live on advertising. How much, and in what ways, were editors influenced in their commentary on civil rights developments by assessments of what advertisers would do? We know about the economic sanctions imposed through the withdrawal of advertising in deep-South states. The process would almost certainly have been more subtle in Tennessee, but did this occur at all? Had Professor Graham explored this, he would have given us a valuable note on the role of businessmen in southern desegregation.

I miss as well in *Crisis in Print* any attempt to assess the impact of the press on the public response to racial change in Tennessee. Graham assumes that newspapers reflect the pressures of the settings in which they are published, and at the same time that they have some influence on these settings. So do I. But the matter cannot be left here, even though it is not an easy one to handle. Graham does leave it as a casually stated assumption, and he in turn is left with routine reporting of what the newspapers said editorially. I am afraid most political scientists will feel that does not make very good copy.—Everett Carll Ladd, Jr., *University of Connecticut.*

COMPARATIVE GOVERNMENT AND POLITICS

Economic and Political Development: A Theoretical Approach and a Brazilian Case Study. By Helio Jaguaribe. (Cambridge: Harvard University Press, 1968. Pp. 202. $5.75.)

Though the two studies in this volume are presented as revised and updated English versions of essays written in 1960 and 1961, they are still best understood in terms of the intellectual mood of the beginning of the decade rather than that of the late 1960's. That mood, especially in Brazil, was one of buoyancy and mild euphoria in view of the apparent success economically and politically of an affirmative nationalist development policy that flew in the face of orthodox monetary doctrine and defied the established international and United States monitors of national growth plans. The opening essay on "National Development and Political models," though it strives for a sober and hard-headed approach to the definition of available development models and the options among them, was nevertheless written at a time when the range of models, if not unlimited, was at least seen as reasonably open and perhaps expanding. Clear thinking, firmness, and political acumen among a few visionary politicians, entrepreneurs, and social technicians might still prevail against the irrationalities of international power tensions as well as reaction, timidity, and political primitiveness at home. Interim events have justifiably dimmed confidence in the will or capacity of present elites to achieve more satisfying national futures through planning and the elaboration of highly rational ideologies.

It is therefore important in judging Jaguaribe's work to maintain some perspective on those things which were and remain lasting contributions to thinking about development in Latin America (the stress on purpose, foresight, and choice; the insistence on viewing development as a total process; the rejection of growth as against qualitative change as a standard for development gains; the redefinition of development in terms of the realization of maximum potentialities at a given historical moment rather than as a fixed ranking on some array of productive capabilities). Against these noteworthy contributions, which may seem less fresh today than a decade ago, a number of negative features bear mention. These include the pervasiveness of an elitist rationale (an over-reliance on social science expertise, charismatic politicians, and enlightened entrepreneurs), the emphasis on elite-mass cleavages and the spotty attention given to intra-elite dissensus, the cursory treatment of mass participation despite the central role such participation is given as a focal transition in political development, the absence of any concrete clues to action for planners despite the determined policy-oriented stance espoused by the writer. In addition political operations in the developed world that is implicitly taken as the ultimate goal are at more than one point ingenuously portrayed if not outrightly

idealized. The alleged workings of elite consensus on goals as well as the presumed effects of participation on mass loyalties and motivation suffer particularly from an uncritical depiction in which the very aspects that are most problematical are taken as given. On the other hand Jaguaribe's argument with respect to the viability of nations clearly prefigures one of Servan Schreiber's main theme in his recent *Le Defi Americain,* though the Brazilian volume emphasizes the quality of political leadership rather than managerial talent. Still the writer anticipates Servan Schreiber in calling attention to the need for social as against technological invention and innovation, particularly as regards the legitimation of strong political leadership.

Perhaps because Jaguaribe looks to the qualities of leaders and nationalist ideologies as the principal foundations of political legitimation, his review of the Brazilian case tends to find strong direction and national awareness where other analysts have found mainly drift and improvisation. Since the balance of new data as against novel interpretation is heavily tipped in favor of the latter in the compressed historical overview in the second essay, it is hard to say how strong a case can in fact be made for the existence of leadership elements with a well-formed national conscience reaching back into the colonial period. In any case this effort gives a curious slant to the historical portions of the country study. On the one hand Brazilians in the mass figure hardly at all in the analysis (slavery is mentioned for the first time when its abolition is noted). Secondly, qualities of foresight and nationalist purpose are attributed to early political leaders (especially, Dom João) that even noted apologists for these individuals have failed to claim for them. Finally, a mysterious nationalist conscience is alleged to have operated at critical moments among a select, enlightened elite. This conscience maintained "an unspoken constitutional pact" in the time of Dom João, allowed Dom Pedro to function as the "caretaker" of the unrealized possibilities of the nation during the 19th century, and is invoked as the critical underpinning for the new entente captained by the national bourgeoisie that is still depicted here as Brazil's main hope.

Though the author argues forcefully for an abandonment of irrational commitments of any order, there is a certain compulsiveness in his final insistence on a "Neo-Bismarckian" route to development despite his careful documentation of the abortive record of this line in Brazil itself. The political dilemma of development is clearly stated: the politician who is too moderate loses mass support; he who courts the mass will be toppled by panic and resentment on the right. Many, who unlike Jaguaribe now seriously doubt that national bourgeoisies can be awakened in time to save their own skins or appreciably change the life situation of the mass of Latin Americans, will want to take a longer look at the more revolutionary options that the author tends to dismiss as unrealizable.—FRANK BONILLA, *The Ford Foundation.*

Scandinavian Political Studies, Vol. II. EDITED BY PERTTI PESONEN. (Helsinki: The Academic Bookstore, and New York: Columbia University Press, 1967. Pp. 333. $8.00.)

This volume has broad scientific relevance; much of its emphasis is upon making theoretical and methodological contributions to the study of political parties, and this review will focus upon these concerns. There are two articles on each of four sub-topics: party formation and politicization; ecology; behavior; allegiance and images.

The first contributions are to our knowledge of the early structure and developmental sequence of national party organizations and to the causes of communal politicization and its effects on local political culture. Ulf Torgersen compares in detail the establishment of Norway's Liberal and Conservative organizations in 1882–85. He notes a poor fit with Duverger's typology. Additionally, his finding that the Conservatives were the first to organize locally both confirms the hypothesis that electoral defeat is the most important stimulus to improved organization and debunks the myth that the Liberals introduced the institution of party while the Conservatives disapproved of such organization. There is evidence that, the greater is the competitor's threat, the more likely is organizational response. The fact that both parties, in the course of four years, were able to draw two-thirds of their voters into formal organizations leads Torgersen to examine the conditions of rapid organizational growth. His analysis leaves one unsettling impression: were the new organizations really spontaneously formed, or should the conclusions about the process remain tentative until we gain knowledge of any role played by parliamentarians and other national leaders? In communal politics, politicization occurred two decades later at the initiative of the Labor Party and in the wake of economic and electoral change. Treating politicization next as the independent variable, Torstein Hjellum shows that it leads to changes in recruitment, increases in participation, and expansion of communal activity. He concludes by offering a scheme of causal chains.

The two ecological studies differ in purpose and method. Jan Stehouwer engages in longitudinal analysis of the post-war years to explain why national party support in Denmark has remained stable despite substantial change in economic and social structure. Examining communes which vary

in urbanization, occupation structure, and population growth, he offers conclusions about party stability both within and across ecological units. While there is great stability in the relationship of urbanization to the Social Democratic vote, this party's support decreases with very advanced urbanization but increases with new urbanization; Liberal strength is maintained by compensating for rural losses with urban gains. Stehouwer details the methodological alternatives and problems in the Danish context, showing awareness of the full range of possible analytic pitfalls. Refined political cartography with communal accuracy is the achievement of Onni Rantala, who focuses on Finnish regionalism. Rantala examines each party's internal regions, which bear classification by strength of support. He pinpoints each party's core areas and notes such structural characteristics as location of party offices and newspapers, as well as geographic features, which are suggestive in explicating the patterns of influence into surrounding areas.

Of the two chapters on party behavior, one is descriptive and empirical, the other purely analytic. To determine the level of agreement among parties and tendencies toward bloc formation in the post-war Danish parliament, Mogens Pedersen employs methods akin to those of roll call bloc analysis. His data are from the three-way voting divisions (Y, N, abstain) on 2,600 bills. An "index of distance" permits comparison across time, and changes of government permit analysis of three constellations of opposition. Pedersen finds agreement extraordinarily great and that consensus is greater with minority than with majority governments. He does not identify the issue bases of disagreement, nor does he directly identify what seems to be the most plausible explanation for both his findings, namely that the Radicals usually exploit their pivotal position to negotiate broad consensus toward their own policy preferences rather than to move their bargaining into the more limited arena of majority coalition government.

The purely analytic contribution of Gunnar Sjöblom begins with criticism of Björn Molin's method of explaining party decision-making in multiparty situations (Vol. I). Molin employed three variables: policy values, perceived opinion effects, and coalitional opportunities. Finding weaknesses in the operationalization of variables and the logic of explanation, Sjöblom concludes that Molin's work is nevertheless valuable as a paradigm if not as the purported "form of explanation". Sjöblom attempts to resolve some of his objections and refines the scheme of analysis.

The methodologically difficult task of establishing the impact of party allegiance upon opinion formation is the concern of Bo Särlvik. With data from four Swedish national interview surveys, he presents four tests of impact. Some of his evidence is quite striking. He finds that, in varying degrees, respondents adjust their evaluation of governing party performance and their attitude toward various issues to conform to the positions of the parties with which they affiliate. Differences in awareness of party position, attitude toward welfare state politics, and socio-economic status are Särlvik's keys to explaining variations in the impact of party allegiance.

The methodology for studying changes in party and candidate images induced by radio and television during a campaign is the focus of Kaarle Nordenstreng and Osmo Wiio's article reporting a method test study conducted during the 1966 Finnish election debates. They employ the semantic differential developed by Osgood, Suci and Tannenbaum, with bi-polar scales consisting of pairs of opposite adjectives which permit construction of profiles. Factor analysis, in the form of a new symmetric transformation technique, provides quantitative indication of semantic similarities and differences between two image structures and indicates varying meaning of individual scales with regard to these image structures. The authors find little media effect on politician images and that radio listening results in more dimensions of image change than does television exposure. It seems that party images are easier to change than politician images because they are vaguer and that radio, being the more "diffuse" medium, leaves more interpretation to personal experience. Mainly the media accentuate existing images.

What we can expect from Scandinavian researchers in the near future is set forth in country-by-country descriptions of current projects. The Swedes present a detailed and exciting prospectus for many-faceted research in local government by several universities. Their study is likely to have great impact, particularly given its $180,000 annual financing and the paucity of local level literature in nearly all countries. The Danes announce the establishment of a data archive on municipalities and describe their new ecological research on voting behavior. For Finland, where such research is longer established, there is a review of both the most recent ecological investigations and the 1966 election study based on survey research. Finally, Henry Valen and Stein Rokkan outline the various approaches and foci in Norwegian election research. As in Vol. I, there is a comprehensive bibliography of writings in political science by Scandinavian authors. Additionally, for those wishing to keep abreast of recent political developments within the four countries, there are short articles of description and interpretation.

In large part through these volumes, the Scandinavians are placing themselves in the mainstream

of modern political science with theoretical and empirical contributions. These volumes are communications of significance. They merit international attention.—SVEN GROENNINGS, *Indiana University.*

International Aid to Thailand: The New Colonialism? BY RONALD C. NAIRN. (New Haven: Yale University Press, 1966. Pp. 228. Price: $6.50)

Politics in Thailand. BY DAVID A. WILSON. (Ithaca: Cornell University Press, 1966. Pp. 307. $1.95)

Nairn's book is a valuable contribution to the literature on technical assistance and international development. It examines the experience of UNESCO in two of its main projects in Thailand: the Pilot Project at *Cha Cheong Sa* which was intended to demonstrate new educational methods for subsequent diffusion throughout the school system, and the Thai UNESCO Fundamental Education Center (TUFEC) at Ubon, designed as a training program for community development teams. A brief description, for comparative purposes, is added of training programs organized by automobile distributors, a German technical institute, and mission schools. Bi-national programs conducted by the United States, and projects of other international agencies, are not examined, although it is asserted that their records are no better than those of UNESCO.

Given the sharp limitations imposed by the scope of this book, despite its title, Nairn has provided a sharply etched case study of failure, failure to design and carry out an effective program which would in fact diffuse new technologies and attitudes or even establish a viable institution. The eventual demise of both projects testifies to their failure. This catalog of inadequacies in program conceptualization and implementation should be required reading in any training program for project leaders engaged in technical assistance activities.

For political scientists, the book holds less interest. On the administrative side, it points up some hard lessons on the conduct of field operations by an international agency. Reading between the lines, one gains insight into the nature of intra-bureaucratic politics in Thailand. One of the more successful endeavors sponsored by the Pilot Project, for example, involved setting up, in response to the stimulus of a resident agent from the Food and Agriculture Organization (FAO), of a demonstration village. The program lasted for only a year, however, and although Nairn says that the "reasons for its demise are obscure," he adds that "There was also friction between the Thai Ministry of Interior and the Ministry of Education. The former thought that the scheme was an unwarranted intrusion into its domain by another department which, administratively and functionally, should have had nothing to do with agriculture." (p. 54) The main lesson belatedly learned by UNESCO in Thailand appears to have been that it could only assure success by entrenching its projects under the protection of a powerful sponsoring arm of the bureaucracy, notably the Ministry of Interior and its field apparatus of governors and district officers.

A more far-reaching inference drawn by Nairn from his studies involves a challenge to the whole concept of foreign aid which he regards as "interference in a nation's lifeways. In reality," he continues, "it is as much a continuum of the technological confrontation, despite variations, as the implanting of Western techniques on Asian societies by colonial powers." (p. 190) For Nairn, it seems, the basic issue is the desire of people in a traditional society to maintain a culture and style of living suited to their needs, values, and habits as against the pressure of foreigners, whether under colonial rule or voluntary assistance programs, to introduce technological changes which are inherently disruptive.

In this perspective Nairn fails to raise more far-reaching questions. Is it feasible for countries with rapidly rising populations, spurred by new medical and public health programs to endure the decline in living standards which must assuredly result if traditional technologies persist unchanged? Can societies with urbanized cosmopolitan governing elites sustain traditional agricultural modes of life? If the answer is no, then the real issue is not whether to engage in developmental activities, but how to minimize costs while maximizing benefits of such operations.

Among the conditions which, politically speaking, obstruct development in Thailand there is one that Nairn mentions only tangentially. He writes, ". . . the general ecology of the nation has as yet not given rise to other [extra-bureaucratic] groups with sufficient strength to compete for the fruits of power or indeed greatly influence policy or decision-making." (p. 95) Yet, in my judgement, it is precisely the concentration of political power and decision-making in the bureaucracy, military and political, that makes the intra-bureaucratic struggle for power so acute and tends to stifle projects which require inter-agency cooperation.

This point is well understood by David Wilson who writes, "The tendency of the bureaucracy to break up into competing groups because it is under no pressure from articulate expressions of interest from constituencies outside the government results in a dispersal of power to make policy." (p. 163). Wilson's book on Thai politics remains today the most convenient survey of the subject. The 1966 reprint in paper-back form makes it more accessible, but not more up-to-date. A new preface adds this useful comment: "the

problem of assimilating a parliament to the system of bureaucratic dominance is the salient, ideological, and institutional question of Thai politics. While it has been possible to postpone a confrontation of this issue, the problem remains." (p. vi) Unfortunately, however, Wilson passes up the opportunity to up-date the contents of his volume or to revise its conceptual scheme in the light of materials published since 1962.

For example, an important point on which further clarification might be provided involves the structure of cliques, ruling groups, and *coups d'etat*. In a polity where neither the elected assembly nor the hereditary monarch exercise much power, the decisive political arena is intra-bureaucratic. Power is held by those bureaucrats capable of seizing it, and rival cliques consisting chiefly of military officers with their civil servant supporters, play the main role in this arena.

Wilson treats the Thai clique as a relatively autonomous informal organization, oriented primarily to its leader. He then gives biographical information about half a dozen of the chief Thai leaders thought to be the heads of cliques. Later, under the head of political organization, he describes the "khana" as a cluster of cliques, though it is not clear from his discussion whether a khana is the conspiratorial group that plots to seize power or the ruling group which dominates a government after the coup. Elsewhere he refers to a "ruling clique" as one that has seized power. (p. 158) He then refers to a "shift of power" or *coup d'etat* as equivalent concepts, thereby equating the transfer of power from one clique or khana to another with the use of violence. Undoubtedly violence has typically been used, or threatened, when one ruling group has replaced another, but not always, as Wilson's discussion of the 1944 shift of power demonstrates. Moreover, there have been military *coups* which caused minor readjustments and realignments in the ruling group, but not a major shift of power. In other words, the shift of power and a military act are normally associated, but not synonymous events.

A better analysis of the clique structure would have shed light on these phenomena. I believe clique structures are best analyzed as anthropologists do segmentary societies, not primarily in terms of their leaders. In these terms a khana is a cluster of cliques formed to seize power. Those khana members who subsequently hold cabinet positions constitute a ruling circle or coalition. The prime minister or coalition leader may, but is typically not, a clique leader—more often he is chosen by clique leaders to serve as a front man for the coalition. Hence although some, but not all, of the leaders described by Wilson headed cliques, the leaders of other cliques are neglected by Wilson because they did not become well-known or take the post of prime minister. In these terms, also, the relativity of clique structure can best be understood, for cliques are segments of a ruling coalition, and if one clique succeeds in displacing its rivals, the component factions of which it is composed tend, in their turn, to become cliques.

Failure to appreciate the dynamics of this process leads Wilson, for example, to exaggerate the importance of Phahon, who was a front man for the first ruling circle, and to neglect Song, leader of the senior clique in that circle. The result is a misunderstanding of the events of 1933 in which Phahon is described as leading the group which seized power from Mano. (p. 119) More accurately, both Mano and Phahon were "fronts" during an inter-clique conflict in which the senior clique, under Song, first tried to undermine the junior clique by driving Pridi out of the cabinet, with Mano's connivance (April 1933), and then Phibun drove Song and the senior clique out of the government, with the connivance of Phahon (June 1933).

These events are not, of course, intrinsically significant, but they established a pattern of inter-clique conflict and a political style which has, with variations, persisted until now. It is important not only for the study of Thai politics, but because similar forms of bureaucratic politics are to be found in many—though not all—of the non-Western States, and because they help explain many of the problems so clearly described in Nairn's book. Since Wilson's book contains much valuable and perceptive information about the history and dynamics of Thai politics, it is to be regretted that he failed to make use of the opportunity, in bringing out a new edition, to revise his discussion so as to make it an even more accurate reflection of the political system. Perhaps this is a service he can yet perform in a future, revised, edition of his important book.—Fred W. Riggs, (Riggs) *University of Hawaii.*

Reminiscences of the Cuban Revolutionary War. By Che Guevara. (New York: Grove Press, 1968. Pp. 287. $6.95.)

Venceremos! The Speeches and Writings of Che Guevara. Edited by John Gerassi. (New York: The Macmillan Company, 1968. Pp. 442. $7.95.)

In eulogizing Che Guevara, Fidel Castro called him "an extraordinary man," and urged all Cubans "to be like Che." To students of revolutions and of the people who make them, Guevara must also have a special place; for Che not only completed the revolutionary cycle—moving from the hills of the Oriente to the power of Havana—but like the winning gambler who dares to throw the dice one more time, Che went to Bolivia. Only this time the hills led not to the capital, but to his death.

Although these two books do not provide us with much insight into Che's decision to enter Bolivia, they do span critical phases in the Cuban Revolution and perhaps in Guevara's own development as a revolutionary leader. The first, *Reminiscences of the Cuban Revolutionary War,* is written by Guevara and tells of his experiences in the Oriente from the landing of the small guerrilla force up to Castro's final drive on Havana. Based on notes he took in the field, it contains terse descriptions of the small battles and difficult marches undertaken by the column he led. Che's style is often matter-of-fact and the thoughts and emotions of Che and his comrades are usually left to the imagination of the reader. Yet Che does provide some good vignettes of his fellow guerrillas, and his description of the march to Santa Clara and the ensuing battle is quite exciting, as is his account of the trial of a guerrilla who had killed a comrade while holding him under guard pending disciplinary action. To be sure, Che is no Hemingway, but he does give us some idea of what it must have beem like to be there, and we do learn something of the preoccupations of the guerrilla leader—of the need for salt, the concern for safety, the problems of morale and discipline and the sheer physical ordeal of initially untrained and inexperienced men fighting and living in rough country.

Venceremos!, a collection of the speeches and writings Che produced after the fall of Batista, shows a Che with a much broader perspective than that of a guerrilla. Here we find him interpreting the events and the meaning of the revolution; lecturing and writing on Marxism, Socialism, Capitalism and economic theory; and above all, calling for a new socialist morality—one in which human fulfillment is realized through the identification with a new, prideful Cuba. While these selections are often repetitive, polemic, and heavy with crude propaganda, they do reveal a good deal about the appeals used by the regime and could be an important source for research on the operational code of Che and his colleagues.

The introduction provided by Gerassi, who also edited and compiled the volume, presents a biographical sketch of Che which is based in part on interviews with those who knew Che as well as on Gerassi's own talks with Guevara. Although a bit too admiring, it does offer useful background material. It does not, however, give us much of an explanation for Che's revolutionary mentality other than to say that he was tough and courageous, and had become concerned about the poor and the oppressed in Latin America as a result of a trip he took throughout Latin America as a young man. Perhaps other students of Che will one day provide us with a more penetrating analysis—on which might, for example, examine the possible effects on Che of such factors as the severe asthma he contracted as a child, the separation of his parents, and his business failure in Argentina. It would also be interesting to know more about Che's relationship with Castro—a relationship which has long puzzled students of the Cuban Revolution.

In any case, Guevara emerges from both of these books as a complicated and many-sided man. Like his revolution, he would seem to be grappling with a series of conflicts. When he speaks of the privations of the peasant, of malnutrition and unemployment, and of industrialization as the way to a better standard of living, his stance is that of a humanist. Yet the same man also reports ordering the execution of comrades who had attempted to withdraw from the guerrilla band, or who had otherwise strayed from the proper revolutionary path. The justification? That the revolution at that point was not strong enough to imprison them, only to kill them.

These selections also show Guevara's implicit handling of other issues, such as Cuban nationalism vs. Cuban dependency on the U.S.S.R.; the need for a mass movement vs. the need for a revolutionary elite; and the need for a bureaucracy to organize and administer the revolution vs. Che's fear that it engendered red tape and inefficiency, and threatened to separate the people from the revolution. And finally, there is Che's attempt to mate Communist ideology with the Cuban experience—an issue he chose to resolve by arguing that Marx bears the same relationship to the Cuban Revolution that Newton does to physics. Marxism, in short, is a basic scientific finding as well as an orienting concept—one which must be creatively applied to a particular revolutionary setting.

Throughout Che's speeches and writings he stresses the need for pragmatism, yet he spends a great deal of time trying to interpret the Cuban Revolution—to build a theory, it would seem, from events whose meaning he may only partially have understood. And here, of course, is the irony: that the principal theoretician of the Cuban revolutionary experience, the one who professed to know its lessons, should be the one to blunder into Bolivia. Perhaps for all his analysis, Che finally yielded to his barely repressed romanticism, and like Garibaldi felt compelled to charge forth once again to do battle—rejecting, as did Garibaldi, hard-won participation in government. Or perhaps poor Che felt the need to be purged of his success—for "revolution," said Che, "purifies man."

In any event, while these two books tell us much about the man, they fail to tell us why Che missed an essential fact of the Cuban Revolution: that it did not succeed because it was communist

but because Castro appeared to be a non-communist revolutionary—a position that made the Cuban army reluctant to fight and resulted in the defection of the middle-class from Batista. In short, Che not only misunderstood the political environment in Bolivia, but he failed to understand that the ideology which had been used to consolidate the revolution in Cuba would be a liability when it came to exporting revolution in Latin America.—WILLIAM H. BRILL, *Georgetown University.*

Communications and Change in the Developing Countries. EDITED BY DANIEL LERNER AND WILBUR SCHRAMM. (Honolulu: East-West Center Press, 1967. Pp. 333 $9.50.)

This book stems from an international conference on communications and the developing areas held at the East-West Center in Honlulu during the summer of 1964. It contains theoretical papers and case studies about India, China, and the Philippines. The results suggest that communications theory is lagging behind the fresh insights being gained in field experiences. Lerner and Schramm attempt to provide an overview, and they are quite perceptive about the role of the communications specialist: he must gain a warm intuitive insight into traditional society and then present the villager with modernizing innovations in an attractive light. They are rather unhelpful about situations of deep-set conflicts between the two "worlds" of tradition and modernity, and the danger that enhanced communications can contribute to that conflict.

Wilbur Schramm provides the keynote to the book in his introductory chapter, which asks how communications can be a tool of economic development programs. He emphasizes the need for a specialist to have a "fit to the culture" (p. 21), but is unclear on what this implies for the role of communications in the larger society. If as he contends, "the really basic strategies of developmental communication . . . are economic and political" (p. 27), who is to decide the political orientation of the specialist? Won't his contact with peasants change his approach? How can he lobby for his new-found insights in the seats of power? A Pakistani fellow at the Center, Inayatullah, hints at these questions in a *cri de coeur* against the ethnocentrism of foreigners and the Westernized native elites. He contends that they ignore the historical innovations and emotional supports of traditional culture (chap. 7). Daniel Lerner simply responds with an outspoken defense of the "Western model." He deplores the false dichotomy of "spiritual" East and "material" West, denounces Asian ethnocentricity against minority groups, and apparently contrasts it with the "self-denying ordinance" of the U.S. not to use its long-term development aid for short-term political purposes (p. 107).

He is even more direct in the conclusion. Traditional society is closed, inertial, and undercuts a man's ability to empathize with the unknown and venturesome. His recommendations follow quite logically: communications can help to reverse the "revolution of rising frustrations" by teaching men to be empathetic. The "policy sciences of democratic development," focusing on "system sustenance" (p. 310), can give firm advice to the policy-maker, and he praises the Socratic philosopher-king ideal.

Lerner's model is quite attractive if one assumes that the villagers can accept incremental amounts of information without perceiving them as attacks upon a rewarding and coherent style of life. He apparently assumes so, and this helps to explain his strange insensitivity to the advantages of "ineffectual" ideology (p. 311) in offering the villager a new sense of life's coherence, however wicked the consequences for his later acceptance of democratic values. His model also seems to assume that the larger political order has resolved its major conflicts and is devoted to development activities.

The other articles in the book do not confirm the optimism about the straightforward virtues of enhanced communications levels, and suggest that the traditional order can teach us a thing or two about empathy. Lucian Pye sensitively analyzes the role of the press in the developing areas (chap. 3). He suggests that the journalist's norms demand that he play inconsistent roles, that of being "inspector-general" of the government's activities, but also an agent of the nation's development program. Better communications can help weak social institutions to build consensus, but effective guidelines still must be worked out through painful experience. The economist Harry Oshima persuasively argues for the central importance of media in publicizing the results of selective growth investments (chap. 5), but Myron Weiner implies that the effectiveness of communications is largely dependent on economic and social variables and cannot be examined in isolation; for example, even as India's press became more influential it helped to exacerbate communal tensions through its sensationalism. India's S. C. Dube further suggests that almost nothing of assurance is known about the impact of communications on traditional society (pp. 95–96). His pessimism is reinforced by Schramm and Ruggels (chap. 4). Their use of 1961 multinational statistics fail to confirm Lerner's 1958 hypotheses on the importance of urbanization for literacy and media growth (in his *The Passing of Traditional Society*). They discern different patterns of media growth in various areas of the world, rather than single world-wide similarities.

Another article by S. C. Dube uses a number of survey studies to stress the importance of mouth-to-mouth communication in India, the most impressive being the amazingly fast spread of oral news about the 1962 Chinese invasion. C. R. Nair then praises the integrity and freedom of the Indian press, but also criticizes its small attention to development. Frederick Yu offers the startling contrast of the verve and intricate organization of Communist China's propaganda efforts to develop the "socialist man" through pageantry and indoctrination (chap. 12). Robert Worth follows with an historical account of the Chinese communists' skillful interweaving of modern and folk medicine in the villages after 1955.

Lawrence Fuchs recounts the experiences of early Peace Corp volunteers in the Philippines. They found that their "communications role" as village school teachers was largely a process of self-discovery about the limitations of American life (chap. 15). Any lasting progress required that they sacrifice a good deal of achievement-orientation to adapt to a more "personalistic," pleasure-centered Philippine attitude. The important point is not that they learned how to manipulate villagers out of their "unempathetic" narrowness, but had to recognize their own lack of empathy and pace their goals accordingly. G. D. Feliciano and T. G. Flores' perceptive and hilarious case study outlines the attempts to introduce sanitary toilets in the nation's rural areas (chap. 16). Development planners gradually had to adapt their designs to the villagers' own experiences and anxieties.

The volume as a whole suggests a disquieting future for the "policy science" of communications. Lyndon Johnson's Foreword and Lerner's idealistic model ring with the consensus attitudes of 1964, and thus have a certain nostalgic charm. However, they force us to ask if communications theory must inherently neglect a questioning of development goals on the one side and the virtues of tradition on the other. The central issue is not who is ethnocentric, but how to get cross-cultural feedback to the highest policy levels. We are still far from that goal, but the first step must be to recognize that the traditional man's defensiveness can stem out of his legitimate fear of losing something treasured and satisfying.—DAVID J. BUTLER, *The Johns Hopkins University.*

France in the Age of the Scientific State. By ROBERT GILPIN. (Princeton: Princeton University Press, 1968. Pp. 474. $12.50.)

With this book, Robert Gilpin has come very close to establishing the definitive model for a case study of science policy in an industrial nation. Whatever reservations one may hold about science policy *per se,* Gilpin's effort has begun to achieve both the scope and the precision that will ultimately provide firm intellectual foundations for the study of interaction among science, technology, and public affairs.

Its chief strength is that this is not a book about politics alone. Rather, it is a successful merger of economics and sociology with the study of policy and politics. The result is coherent and methodical, yet unmarred by superficiality or jargon. If there is a drawback to this approach, it is the blandness that creeps into summary remarks on the force of tradition, values, national character, and institutions. Yet there is little doubt that behaviour patterns and ideology inculcated during the nineteenth and early twentieth centuries have been (and continue to be) crucial barriers to the process of modernization. The difficulty is to delineate them sharply and with economy.

The structure of the book is reasonably clear, though there is some overlapping. Each of four groups of chapters focuses on a different aspect of the underlying issue: what is the role of the state in an era of rapid change, in which scientific technology provides the main motive force. The first three chapters elaborate this question in a framework conditioned (if not determined) by the dynamics of American development and its repercussions in trans-Atlantic economic, financial, and political relations. Chapters four through nine provide a detailed review of the historical forces in French education, politics, and public administration, leading up to the present situation. This view is thrown into relief by a short chapter interpreting the American experience as a kind of ideal type.

The next two chapters—ten and eleven—are devoted to a careful examination of French science policy in two key areas—institutional modernization, and the expansion and politization of the scientific and technical community. In the final two chapters the contemporary dilemma of France —and by extension of Western Europe and other middle-sized industrial states—is analyzed in relation to the centripetal attractions of the American model, the option of a common "European" solution, and the vague attractions of independence.

Because of the general excellence of this study, one may cavil only at small failings. On the conceptual side, it is not made clear whether the proposed "imperatives" for scientific and technological advance and exploitation are modeled on the United States as it is, or as it would like to be (or ought to be). In short, one senses that a number of normative assumptions are not adequately examined, on the theory that success provides its own justification. Does this suggest that the French, and other nations, should strive to emulate and reproduce, analyze and select, or absorb and surpass the American experience?

On the methodological side, there is some evidence that budgetary data has been rather uncritically reproduced from official French sources. While it is useful to have so much information available in one place, it would be helpful to have it presented in a more comprehensible fashion. For example, this reviewer would have preferred reading budget amounts in percentages as well as in gross sums. A dollar conversion has become a standard device in publications of international organizations, and might have been adopted here.

Obviously a great deal of translation was necessary, and most of it, while not elegant, is workmanlike. However, some readers may be uncomfortable in the presence of renderings such as "law-program" (legislative program), "in effect" (in short), and "supple" (flexible), which are less misleading than literal and awkward.

A study so ambitious and detailed partially satisfies every specialist craving, and excites new appetites as well. This reviewer would have liked to find a discussion of tax measures for the promotion of private industrial research, for example. The somewhat rhetorical treatment of the relation of economic planning to governmental research investment may leave students of economic policy hungering for more practical information. Likewise, the scientific community is not analyzed as an emergent interest group, though an adequate foundation is laid. In all these cases, however, the notes are copious and suggestive.

One especially intriguing gap is the influence of that generation of scientific adviser-administrators who admired the Soviet model and who were at one time members of the Communist party. Only Frédéric Joliot-Curie is mentioned more than briefly, yet the political activities of this scientist and his colleagues commenced between the wars and continued until the emergence of the Fifth Republic.

The internal and external politics of change has many aspects, of which the mobilization and application of science for social purposes is of great contemporary interest. Gilpin's study of one national society moves us away from the monographic, disciplinary, and culturally limited stage of analysis without sacrificing the flavor and importance of national patterns and traditional disciplines as sources of insight. What remains in question is the objectivity and utility of a scheme that is Atlantic in scope and tends to merge Europe's fears with America's dreams.—T. Dixon Long, *Case Western Reserve University.*

Korea and the Politics of Imperialism, 1876–1910. By C. I. Eugene Kim and Han-kyo Kim. (Berkeley: University of California Press, 1968. Pp. 260. $7.00.)

The study of the decline and fall of any kingdom or dynasty is always a fascinating subject to a serious scholar. Compared to the voluminous studies on the fall of the Ch'ing dynasty or the coming of the Meiji restoration, the demise of the Yi dynasty in Korea has seldom been studied by a disciplined scholar. There are some studies on the Japanese policy to seize Korea: H. F. Conroy's study in the United States and C. C. Mun's study in Korea. Studies on some phases of the fall have appeared recently: K. J. Shin's study done in Japan and published in Korea and Yi Na-yŏng's study in North Korea in 1965. But there has been no monograph, until this study, that covered the three decades of the intricate story of the opening and annexation of Korea.

This book, by two Associate Professors of Political Science at Western Michigan University and the University of Cincinnati, is valuable, not merely because the authors have done the difficult work of recounting complex forces that operated in the process of the Korean demise, but because they have shown a remarkable understanding of history and an amazing perception of the imperialistic designs of foreign powers that operated in the fall of the Yi dynasty. The result is a pithy and highly commendable inquiry into the circumstances leading up to the Japanese annexation of Korea.

The book in four parts covers the fall from the first treaties concluded to the final annexation treaty of 1910. It begins with succinct but excellent introductory notes on traditional Korean political life, and unfolds the events which attest to the manifest and growing Japanese influence at the expense of Chinese suzerainty over Korea. The Un'yō incident that resulted in the first treaties, the Im'o mutiny of soldiers, the political imbrioglio of Kapsin, and the Treaty of Tientsin all are examined. The precarious domination of Korea by China prior to the Sino-Japanese confrontation, the indigenous rebellion, and the Russo-Japanese war that successfully eliminated the last of the rivals for Japan are closely scrutinized. The cautious and consistent Japanese advance that ultimately incorporated Korea into Japanese empire is also evaluated. The authors concluded that Korea's inability to transform its society to meet change, persistence of factionalism, lack of popular participation, lack of resilience and adaptability in Korea's political system, and rapid growth of Japanese power, all contributed to its fall.

The study is basically sound and the accounts of intricate workings of the imperial forces are well presented with uncommon objectivity. But the study suffers from the lack in general of critical analysis of the events so well documented. Often it is merely an account of events without any meaningful evaluation of the evidence presented. At times it is only a summary rather than

analysis of events. There is analysis only of the reform movement of 1898 (pp. 112–14); such analysis was needed in each part at least, if not in each chapter.

Some five or more pages of data, for example, were introduced (p. 154) with little or no analysis of the Japanese control measures. A case was also made to stress that only three percent of the populace were participating in the decision-making process (several tables appended to the book), but how this fact and the tables are related to the fall of the dynasty was not explained. Particularly in Part IV, there are prolonged discussions on the role of Bethel, what seems to be the minor if not irrelevant role of missionaries, and the exaggerated role of the Ŭibyŏng; but there is little analysis of how these affected the Japanese use of coercion in Korea. A convincing argument has been advanced as to the lack of resilience and adaptability of the traditional Korean political system, but there is relatively little discussion on the Korean political system itself. Compared to a relatively detailed account of the workings of imperial powers, there is little account of the roles of Koreans. This is perhaps a reflection of predominantly Japanese sources used by the authors.

The authors presented well the passing of the old and coming of the new subjugated Korea. They have skillfully described many facets of the fall of Korea; some similar to that of China and others peculiar to Korea. The encroachment of imperialists to open a secluded and reluctant hermit nation that was incapable of reform and plagued with internal rebellion is very similar to the Chinese experience. The actual breakdown of the old tributary system to give way to the new treaty system was exemplified by the Korean demise. The authors correctly pointed out that it was indeed an Aisan imperialist that ended and engulfed the Korean kingdom. The defeat of Russia and China's desperate but unsuccessful clinging to the fiction of "the autonomous but dependent" Korea was also unique to the Korean demise.

The book provides a much needed study of the period, and is a valuable addition to the growing studies on Korean politics in the United States. The authors' research for the detailed facts is meticulous, and they adhered scrupulously to a comprehensive conceptual framework. They have written well and with erudition the account of the demise of the Korean kingdom.—Dae-Sook Suh, *University of Houston.*

The Whig Party and the French Revolution. By F. O'Gorman. (New York: St. Martin's Press, 1967. Pp. 270. $9.00.)

Donald Southgate has pointed out that while Dr. Johnson thought the Devil must have been the first Whig, Lord Acton favoured the claims to that honour of St. Thomas Aquinas. Certainly for much of the long period from 1688 to 1885 there was room to accommodate no less strange bedfellows beneath the protective cover of Whiggery. Yet there were all the while changes and recombinations of philosophical and political forces taking place which eventuated in the emergence of the 'new' Whigs as an identifiable force in the modern British party system. For most of the 18th century, of course, as the painstaking researches of the late Sir Louis Namier and his disciples established, there was no 'party' system at all, but rather an elaborate system of factional politics based on family and patronage, in which the Whigs were more or less all those who opposed the Crown and Administration group in the parliament. Since Namier, interest has been high in tracing the steps by which party grew up at Westminster. We now have the major work of Sir Keith Feiling on the Tories, while on the Whig side, there have appeared in addition to a number of excellent monographs on prominent Whig statesmen, important books by such writers as Brooke, Foord, Derry, Black and Pole. What is clear, the writer of the study under review believes, is that as our knowledge of party development deepens, simple structural interpretations of the growth of party, dependent on one dominant variable such as family tie or patronage system, will not suffice. "Belief in a principle for its own sake, prejudice, fear, ignorance and hesitation," O'Gorman suggests, must be taken into account alongside the more tangible factors. He has arrived at this conclusion after a close reconstruction of shifting political allegiances in 'old' Whig circles during the crisis years of 1789–1794, when the old alliance of Whig factions was shattered by the historic decision of the Portland Whigs and the 'Third Party' forces of Windham and Burke to join in supporting the war government of the younger Pitt. The Foxites were thereafter left virtually alone in opposition, and from there formed the vehicle through which the 'new' Whigs of the 19th century received the ideological legacy of "Peace, Economy and Reform."

The general features of Whig politics in these years are well-known, and need no elaboration here. O'Gorman finds structural, ideological, and chance elements which contributed to the breakdown of the old Whig machine. The appearance of political clubs, the growing reliance on party newspapers and party symbols for mobilizing support and extending party identification, the remarkable contributions to party financial organization of William Adam—all these posed a threat to the continued domination of the Whigs by family notables. While the Foxites supported such new vehicles of public opinion as the radical Association of the Friends of the People, and many of

them favoured parliamentary reform, the issue of the extent to which the party should adopt a popularizing stance became a major one separating the progressive and traditionalist element in the party.

So far as the ideological factor is concerned, it was the events following in the train of the French Revolution, as O'Gorman argues in the main section of his narrative, that after 1793 irreversibly tipped the scales away from the old balance of forces. From the beginning Fox saw in the Revolution "one of the most glorious events in the history of mankind," while first the government benches, then aristocratic Whigs, and finally public opinion swung behind Burke's vehement denunciations of French Jacobinism. In the end, the issue became a struggle over the definition of party that ended in disaster for the Whigs. The Foxites strained to portray their differences in foreign policy as a "particular issue," not fundamental to the Whig attachment itself. Yet the great theoretician of party government in 1770, Edmund Burke (of whom Goldsmith had jibed: ". . . born for the universe, narrowed his mind, / And to Party gave up what was meant for mankind."), was now in his *Observations on the Conduct of the Minority* (1793) above all else the advocate of "joint endeavours [for] the national interest." There was no inconsistency with the notion of coalition government in Burke's view of party, and as O'Gorman observes, a "delighted" Edmund Burke looked on as Portland accepted the seals of office under Pitt.

Finally, there were elements that accelerated the separation traceable simply to personality and lost opportunity. While the Duke of Portland, wishing to be all things to all men, stood as titular head of the party, it was the uncontrollable Fox, passionate defender of great causes but curiously lacking in political sensitivity, who held the hearts of the younger members. Burke was as high-minded as Windham was vacillating, and others like Loughborough were more concerned for "personal honour." And the lost opportunities! We are told that during the entire three weeks leading up to the fateful debate on the Quebec Bill, when Burke's intentions to break openly with Fox were widely known, little was done by any faction to reconcile the two.

This is a well researched and heavily documented study that will be welcomed by those interested in British party history. It is not always easy going, and assumes some acquaintance with the background events of the period. But it is reliable, and will be a required reference for more ambitious interpretations of the general development of party politics in the historical and comparative perspective. Dr. O'Gorman, in several appendices, provides important estimates of the relative strength of the Whig factions at Westminster during the period.—GEORGE FEAVER, *Emory University.*

Revolutionary Russia. ED. BY RICHARD PIPES. (Cambridge, Mass.: Harvard University Press, 1968. Pp. 365. $7.95.)

The fiftieth anniversary of the bolshevik revolution provided the stimulus for many conferences and books, of quite varied quality. The papers presented at the Harvard conference on this theme are of more than passing interest; together they form a useful statement on the present state of Western research on the Russian revolution. Although the conference organizers made no effort to provide a chronological history—these already exist in abundance—the inclusion of many of the West's most distinguished students of the revolution assured a broad representation of current research interests and interpretive efforts. These interests range from the old and continuing questions, such as the reasons for the breakdown of the tsarist autocracy, and the role of German intelligence in the pre-revolutionary period, to a restatement of the well-trod ground of the October events in Petrograd; to such relatively unexplored areas as the political thought of the First Provisional Government and the Petrograd Soviet, the aspirations of Russian society in the March days, and the conflicts between the town and country in the civil war. Three interpretive essays—by E. H. Carr, Maximilien Rubel, and Adam B. Ulam—are directed to the often raised but finally irresolvable question of the "meaning" of the revolution. In addition to topical variety the essays, and particularly the formal "comments" and informal discussion following each paper reflect the great ideological and methodological differences among students of the revolution. Far from detracting from the book's value this heterogeneity enormously enhances its utility. The evident conflicts force the reader to adopt a critical attitude to the premises, evidence and conclusions of each contributor.

While it would be presumptuous to attempt criticism of each article—the comments and discussion following each essay point well enough to their more obvious shortcomings—some general observations may be in order. In a penetrating, lucid essay Mr. Kennan reviews the evidence once again on the chances for the survival of the Russian monarchy had not the war intervened. Optimistic at the outset, Mr. Kennan finds that neither the slowness of the regime to grant political reforms, nor its excessive nationalism, nor the personal limitations of the Tsar—all critical factors in the final fall of the monarchy, in his view—could be blamed on the war. Neither were these factors substantially on the way to correction in 1914. Therefore, he concludes, there was no real hope

for the dynasty's survival. In my opinion, Mr. Kennan's essay is an example of the best in interpretive, analytic history. In raising broad questions, he never strays far from factual data in seeking their answers. This same can be said of Adam Ulam's highly insightful reflections on the gap between practice and ideology in the revolution and how both Lenin and Stalin justified this to themselves. Unfortunately, one does not get the same sense of a careful reverence for factual support behind the interpretive efforts of Maximilien Rubel. His thesis that the Russian revolution was not a socialist but actually a capitalist revolution led by the *de facto* Russian bourgeoise rests upon a thin semantic argument and requires alteration of long established and accepted notions of capitalism as having something to do with private ownership of capital and a market exchange system operating under more or less competitive conditions. This as well as other fundamental methodological shortcomings in Mr. Rubel's essay are well formulated in the comments by Shlomo Avineri.

Mr. Richard Pipes has presented us with the finest piece of straight historical research and expository writing in this collection. His thorough study of the sparce documentation on Lenin's early political activities and thought combine with his closely reasoned argument to make a most valuable contribution to "Leninology." Of particular interest is his discussion of Lenin's "conversion" to Social Democracy in the summer of 1895.

The question of the role of German intelligence in Russia prior to the revolution has always been the center of fierce ideological conflicts among Western scholars. Those who would like to believe that among the Bolsheviks were many "paid agents" of the Germans tend to be less critical in their evaluation of the existing scanty evidence of German-Bolshevik connections, while those opposed to this view for ideological reasons refuse to credit anything but "legal proof." George Katkov's paper represents the former position; the critique by Alexander Dallin the latter. If one evaluates Mr. Katkov's essay solely as a report of the available evidence of German political intervention, without considering the broader conclusions he draws from it, then it must be recognized as a solid piece of research. Yet, an important test of scholarship, in my view, is the care with which conclusions and assertions are related to evidence. In this sense, Mr. Dallin's acid remarks are well taken—Mr. Katkov's limited data permit only very limited conclusions. Together, Mr. Katkov's and Mr. Dallin's contributions show the extent to which ideological positions influence the research of Western scholars.

The analyses of the political ideology of the First Provisional Government and of the leaders of the Petrograd Soviet by Mr. Schapiro and Mr. Anweiler respectively deal quite competently with complex and important questions. Their arguments and conclusions elucidate effectively the reasons why these two bodies both failed to work together and failed to provide effective leadership in a revolutionary situation.

From the point of view of a political scientist, perhaps the most interesting article in this collection is Marc Ferro's study, "The Aspirations of Russian Society." Its significance lies in its methodology and the type of data utilized. Ferro attempts to assess popular attitudes through statistical and textual analysis of letters to the editor received by *Pravda* and *Izvestia* in the first few weeks following the fall of the Monarchy. His method is similar to that used in several studies of the American revolution. Yet, to my knowledge, this is a first in Soviet studies. While Mr. Ferro's results so far, presented in this collection, are rather sketchy and somewhat disappointing, the important point here is the possibilities for future research this method may open up. While both the author and Mr. Thompson in his comments on Ferro point out the extreme caution with which the results of such studies must be interpreted, the experiences gained by Mr. Ferro and the Ecole Practique des Hautes Etudes in gathering and processing this type of data may suggest and make possible more fruitful studies of the role of public opinion in Soviet politics.

On the whole this is a valuable collection of interpretive and analytic essays on the general subject of revolutionary Russia. Both the student and general reader will find it well worth reading.—PHILIP D. STEWART, *Ohio State University.*

Soviet Leaders. ED. BY GEORGE W. SIMMONDS. (New York: Thomas Y. Crowell, 1967. Pp. 405. $10.00.)

This collection of biographical sketches of 42 prominent figures in Soviet government, science, and the arts provides interesting biographical reading and a very useful reference work for both the interested layman and the political scientist. Since it was compiled primarily with the former audience in mind (p. v), the book contains a glossary of political and historical terms (e.g., collective farm, Council of Ministers of the USSR, Executive Committee, *kulak,* etc.). It is usefully indexed by name and subject. The 42 essays are written by 20 scholars, some of whom are well known to American students of Soviet politics John Hazard, Grey Hodnett, Robert Slusser, and Severyn Bialer) and others who are probably not as well known (R. Milner-Gulland, Maxim Mikulak, and Siegfried Müller-Markus). The essays vary in length considerably, from Kermit McKenzie's biography of Anastas Mikoyan (23 pp.) to

Severyn Bialer's biography of Konstantin Vershinin (3 pp.).

The editor's Introduction to the book presents a brief chronology (20 pp.) of events in the Soviet Union since the death of Stalin. In the first half of the essay Khrushchev's changes in domestic and foreign policy are contrasted to the earlier period of Stalinism; in the latter half Khrushchev's policies are compared to those of his successors. There is little of value here for the reader who has at least a general knowledge of Soviet politics, be he political scientist or layman.

The value of the 42 biographical sketches for the interested layman or the non-specialist is clearly more notable. Aside from official Soviet sources (almost all of which are in Russian), the only detailed sources of biographic data on Soviet personalities are the directories published from the biographic data in the files of the Institute for the Study of the USSR in Munich, Germany: *Biographic Directory of the USSR* (1958); *Who's Who in the USSR: 1960/1961* (1962); and *Who's Who in the USSR: 1965/1966* (1966). These three directories merely list demographic and career positions (with dates). The volume under review is in the form of readable prose and has the added advantage (for those who desire or require it) of placing this information in the perspective of the times and discussing, with the information that is available, the personalities and achievements of the subjects. The editor claims that the contributors "have avoided a partisan approach" in this endeavor (p. v), and that claim seems a wholly accurate representation. However, this more elaborated form of biography contains instances of interpretation (or embellishment) of data which one might question, such as the conclusion that "Semichastny is . . . probably less intelligent . . . than Shelepin . . ." (p. 86).

Because of the availability of the Institute biographical publications, it is rather strange that more use was not made of them by the contributors to this volume. Only five of the biographers (Slusser, McKenzie, Hodnett, Parry, and DeGeorge) included them in their bibliographies. Presumably, they were the only five of the 20 who made use of them in compiling biographic data for their essays. This is especially strange in that all but four of the 42 subjects (Liberman, Neizvestny, Solzhenitsyn, and Voznesensky) are listed in the second edition of the *Who's Who in the USSR*. In several cases where the Institute publications were not utilized, the biographic sketches in this book did not include some relevant material contained in the former. Especially is this so in the case of several of Bialer's essays. Of course, the validity of the Institute's data files is questioned by some. If there is good reason to question their validity, then the five contributors who utilized the Institute's publications should neither have used them nor have suggested that their readers consult them. On the other hand, if those files are considered reliable, the other contributors should have made use of them.

This prompts one to consider the many problems which confront the researcher in his quest for reliable biographic data on personalities in all Communist systems, especially the Soviet Union. Since the purpose of this volume is to present such data, the reader is entitled to some explanation concerning the methods employed and the sources utilized. In this case, it would appear to be the responsibility of the editor to inform the reader on these matters. Yet nowhere in this volume are questions related to the validity of data and the reliability of sources discussed. In a paper read at the 1963 Annual Meeting of the APSA, Severyn Bialer demonstrated his keen awareness of the problems of reliability in the collection of Soviet elite biographic data. One can assume, therefore, that he exercised extreme caution in the utilization of official Soviet sources. One would hope that the other contributors exercised similar caution in dealing with problems such as the systematic distortion of social background data so that it appears that most, if not all, prominent figures came from industrial proletarian or peasant families. Since there is no discussion of the question of source reliability in this work, we have no way of knowing what, if any, consistent standards were used in the compilation of the biographic data.

The 42 personalities selected for inclusion in this book are well-chosen, given the aim of presenting "a composite portrait of the current Soviet elite" (p. v). In the case of the party, government, military, and foreign service officials in the book, they clearly occupy the top positions on the various organizational charts. However, one must be aware of the fact that this selection places the most severe limitations on the generalizability of the "composite portrait." Unfortunately, no caveat to that effect appears in the book. That is a significant omission since the book is directed mainly at a lay audience and especially because the similarities in the lives of these elite members are emphasized. A caution against over-generalization or stereotyping is not merely appropriate, but mandatory, in this country where a good many people think that all Russian women look more or less like Nina Khrushchev.—FREDERIC J. FLERON, JR., *University of Kentucky*.

La socialisation politique des enfants. By CHARLES ROIG AND F. BILLON-GRAND. (Paris: Armand Colin, 1968. Pp. 186.)

La socialisation politique des enfants is the first published report which extensively discusses the

political socialization of French school children. The data are based upon questionnaires administered in 1963 to 413 children betwee the ages of ten and fourteen living in or near Grenoble. Although the authors could not study all areas of the city (most importantly, they could not gain access to the schools in the Red Belt), they did question students in both Roman Catholic and state schools. Thus they not only are able to present global findings, but also to discuss some interesting subcultural differences.

The most interesting findings concerned the absence of partisan attachment and the lack of positive affect for political authorities. Less than one-half answered yes when asked if there were any good political parties and only 13 per cent named a political party they thought was good. The U.N.R. was the most popular party, but it was supported by only 10 per cent of the children questioned. (Unfortunately, the authors did not gather data on children's perceptions of their parents' party identifications and do not comment on the well-known interpretation of lack of partisan support presented by Philip E. Converse and Georges Dupeux.)

The authors also find a relatively high level of distrust for public authorities. When asked to explain what the President of the Republic does, only eight per cent viewed him as a benevolent leader; less than one-half designated a politician or statesman as the man who has done the most for France; only one child out of five named a politician as a person he would like to resemble, whereas two out of five listed a politician as a person they would not like to resemble. Nearly all of the children could name De Gaulle as the President of the Republic, but when asked to list his good and bad qualities, there was a higher percentage of unfavorable than favorable responses. The authors argue that these unfavorable evaluations of the President reflect more of a resentment toward authority in general than a disapproval of De Gaulle and his policies.

The children do not appear anti-parliamentarian. Asked whether a country should be led by one man or by an assembly of deputies, three out of five preferred government by assembly. Only one out of five said a country should be governed by one man. Surprisingly, those who wanted a government by one man were more likely to believe there were good political parties than those who favored government by assembly. This is explained by the high percentage of U.N.R. supporters among those who thought there were good parties.

Roig and Billon-Grand are well informed about American social science and, in particular, by American findings about the political socialization process. David Easton, Robert Hess, and Fred Greenstein are among the persons most frequently cited. One of the most valuable contributions of this book—to French readers at least—will be an introduction to some recent American social science.

The American reader, however, is likely to be both disappointed and confused by the data analysis. Throughout, the authors present data in the form of two-way contingency tables. There are usually three breakdowns: by sex, by type of school (Catholic or state), and by class in school. Initially, the breakdown by class in school appears to present pseudolongitudinal patterns, but this classification involves more than age differences. The oldest children (in the end-of-studies class) are of heavily working class origins compared to the children in the middle two years. Also, children educated in church schools have higher social origins than those in state schools, and girls have higher social origins than boys. Thus to the extent that interesting subcultural differences do emerge, it is difficult to know whether they are related primarily to age, sex, type of school, or social class of origin. Also, and more serious, the authors sometimes select one item to measure an overall political orientation and then present two-way contingency tables showing the relationship between responses on that item and other attitudinal items. For example, they employ one item "Was the French Revolution of 1789 good or bad?", as a measure of Right-Left orientation and then show the relationship of responses about the Revolution to 24 other items. True, some interesting relationships emerge, but one cannot use such findings, as the authors do, to make broad generalizations about the French Right and Left.

But the American reader will learn much about French political culture from this book. One of the authors' most interesting arguments is that one should discard such preconceived notions as France being a conflictual society (as contrasted with an American consensual society). Rather, they argue, there are probably basic areas of consensus in France although, they admit, they do not find them in their data. Most importantly, Roig and Billon-Grand have demonstrated that socialization research can be conducted in French schools. Problems of access may be greater than in the United States, but they are not insurmountable—PAUL R. ABRAMSON, *Michigan State University.*

Thailand: Another Vietnam? BY DANIEL WIT. (New York: Charles Scribner's Sons, 1968. Pp. 205. $4.95 & $2.50.)

American involvement in Vietnam has caused many observers and statesmen to look at Thailand as a possible alternative base in the resistance against Communist expansion in Southeast Asia.

Thailand is the only relatively large unified non-Communist society located near Vietnam. It is the only nation in the region which did not fall under European colonial rule, and the absence of an anti-Western sentiment has encouraged it to cooperate actively with the American containment policy since 1950. This action has caused the Peking regime to mark Thailand as the next country for a "war of national liberation." Already several thousand Communist insurgents are waging guerrilla warfare in the northeastern provinces of the kingdom.

Thus the talk of "another Vietnam." The ominous title of Professor Wit's book is heightened on the cover of the paperback edition which shows a lush green map of Southeast Asia broken by a bright red undercoating beneath the torn fabric of Thailand. Yet only in brief sections at the beginning and near the end of the study does the author directly address the challenging issue of Thailand becoming "another Vietnam." It is very possible that the newsworthy title of this book and the sensationalized cover of the paperback edition were chosen by the publisher rather than the author.

The study provides much good data and analysis on which to judge the future of the Thai kingdom. It is a well written and scholarly piece of research. The book is well documented and includes a vast array of useful statistics on the Thai educational system, the economy, and the administration. It contains brief but good background information on the cultural and social forces that affect Thai politics.

The bulk of the study is an assessment of the Thai political system and the strengths and weaknesses of Thai society. The author's major thesis is that the Thai government for many centuries has provided stability and security to its people and it has gradually adjusted to new changes and new demands. Yet the ruling elite today must build a broader representative base if it is to cope with new economic and social challenges in the near future. More of the political system, Professor Wit claims, must be institutionalized and more politically oriented groups must be brought into the decision-making process. If these reforms are not forthcoming, Thailand will be subject to greater internal instability and the likelihood of more external infiltration.

The deficiencies in the study are relatively minor. In places the discourse is hampered by long outlines enumerating factual data and analytical commentary which the author feels is of special importance. This practice tends to make the discussion somewhat rigid, wooden, and oversimplified. Also, the author never resolves the dilemma posed by his assessment of the widespread devotion among the Thai people to Therevada Buddhism with its emphasis on suffering and detachment from worldly pursuits and the contagious and joyous enthusiasm of contemporary Thai society for rapid material progress.

Perhaps the most serious weakness to this reviewer is the heavy stress by the author on government planning and the need to create "pioneers" to guide the kingdom in its uncertain future. Professor Wit never elaborates on who these innovators will be and what role they will play in future *political* development. His proposal very likely involves a political contradiction. If the political reforms he desires are inaugurated, more groups and more leaders will be brought into the political life of the nation. Open debate and free political activity will cause more diverse opinions on economic, social, and international affairs. Some Thai will want to place much more of the national economy under public ownership. Many will propose more private enterprise. Some will advocate a neutral foreign policy. Many will want to retain close relations with the United States. This development will make it less rather than more likely that the government will be able to engage in any consistent and precise long-range economic and social planning. The price of democracy in Thailand as elsewhere will be a greater role for conflicting political groups and some political and administrative disorder.

In spite of these shortcomings this is an excellent book. The question posed in the title is never directly answered. Fortunately, the author provides much good information and analysis on which the reader can make up his own mind.—FRANK C. DARLING, *DePauw University.*

The Fate of the Revolution: Interpretations of Soviet History. By WALTER LAQUEUR. (New York: The Macmillan Company, 1967. Pp. 216. $5.95.)

Since the historians of the French Revolution are still debating the shape of its events and their meaning almost two centuries after the *revolte nobilaire* opened the period of revolutions, it is not surprising that the Russian Revolution a half century later is still the subject of passionate partisan and scholarly controversy. *The Fate of the Revolution: Interpretations of Soviet History* is Walter Laqueur's contribution to the flood of books on the fiftieth anniversary of the revolutions of 1917. It is a reasonably dispassionate and scholarly attempt to sum up the problems faced and the positions taken by the more important historians and political scientists, by apologists like Barbusse, by official Soviet historians, by loyal oppositionist historians like Leon Trotsky and Isaac Deutscher, by semi-critical analysts of the accounts of the victorious like E. H. Carr, and such varied writers as Billington, Ulam, Gurian, Feiler, Arthur Rosenberg, Fainsod, Brzezinski,

Borkenau, Leonard Schapiro, Bernard Pares, Bertrand Russell, Boris Souvarine, Prince Mirsky, George Kennan, and the present reviewer. Some of these he summarizes and analyzes in depth, others only tangentially. In the case of the former he attempts to establish the background of each historian's point of view, the problems he raises, and the level of his skill and scholarship. In each case he strives to present objectively the positions expounded before venturing or withholding his own comment. Though Mr. Laqueur has not read everything of significance he has come as near to it as any man writing on the occasion of the fiftieth anniversary of the fateful year 1917, including the major bulk of writings in Russian and English, and key works in German. Thus he has produced a combination of a critical bibliography with a good summary of major viewpoints raised during the entire half century of historiography, political analysis, and controversy, and of the problems debated and still subject to debate. Of particular interest today are his analyses of such questions as the applicability of the term totalitarian to the Russian one-party state aiming to embrace and control all the concern of Russian society; the uses and weaknesses of "Kremlinology"; the problem of whether Stalinism was historically "necessary"; and the soundness or unsoundness of the "convergence theory."

Mr. Laqueur's credentials for handling these difficult tasks include the former editorship of the British journal *Survey,* coeditorship of the *Journal of Contemporary History,* directorship of the British Institute of Contemporary History, and the post of Professor in the History of Ideas at Brandeis University in this country.

It might be well to close this review with the author's own statement of the scope and purposes of his book:

> I have not tried to break fresh ground as far as source material is concerned, nor have I attempted an historical synthesis. The aim was less ambitious. I wanted to provide a guide for the non-expert, to stimulate further study and critical thought rather than offer my own answers to the many questions posed. [The resultant book] is a study of the problems which have faced historians of the Soviet Union and a discussion of their interpretations . . . an introduction but neither a history nor a bibliography. . . . I have tried to reproduce or summarize fairly views and arguments that are not my own. . . .

The task he thus set himself is unique in historical writing on the Russian Revolution and is competently performed.—BERTRAM D. WOLFE, *Hoover Institution on War, Revolution, and Peace.*

The Rise and Fall of Kwame Nkrumah. BY HENRY L. BRETTON. (New York: Frederick A. Praeger, 1966. Pp. 232. $5.95.)

In this critical reappraisal of Ghanaian politics Professor Bretton argues that Kwame Nkrumah subsumed all Ghanaian political organizations within a tightly controlled personal machine. In Bretton's view, Nkrumah did not need to share decision-making or balance contending groups, as previously supposed, because Ghana lacked any organized social forces or ideas other than those dependent upon its leader. Scholars have neglected this system of personal rule, Bretton asserts, due to acceptance of the institutional label for the fact and a morally irresponsible tendency to condone autocracy. Betton strives to correct these failings, moral and intellectual, by getting behind the forms to the "real actors." The result is a curiously unreal apotheosis of Nkrumah consequent upon presenting his extensive powers abstracted from their setting and embellished with Cold War hyperbole.

This formalism does not arise from disregarding backstage manipulation but from excessive concentration on "top-level control of the power and influence structure." This viewpoint excludes the overall process of decision-making and focuses narrowly on Nkrumah's often unused final authority. Hence, Bretton does not treat institutional continuity as a reflection of decisions shared by organizational leadership, nor describe the common organizational practice of weighting decisional alternatives by pre-emptive acts, nor assess the political resistance to innovation manifest in unfulfilled commands. The omission of a decisional framework, descriptive or conceptual, facilitates these exclusions, as does the absence of a case study of Nkrumah's exercise of power.

Moreover, the view from on high reduces Ghana's actual cultural and social fragmentation to an atomistic void; Bretton treats the political machine as disassociated individuals linked only by Nkrumah and rejects even systems analysis as imposing false coherence on the data. To establish the "cultural wasteland" Bretton concentrates on the lack of a unitary national culture and coherent national doctrine. He ignores such influential belief systems as traditional attitudes towards authority and ethnicity, the English Liberal sub-culture of the educated sector, the emerging African Socialist sentiments of conflict avoidance and communalism, Christianity and Islam. In his assessment of political doctrine Bretton confines the significance of Nkrumah's ideas to the tactical flexibility of empty abstractions. Yet both Nkrumah's supporters and opponents readily identify him with such substantive ideas as African unity, the threat of neocolonialism, the economic role of the state and the centrality of politics.

Unlike Ghanaians, Bretton ascribes no political importance to economic, status, ethnic, or geographic conflicts. He regards the organizations which reflect these strains as cogs in the machine.

The role of the Central Committee of the Convention People's Party in decision-making has not been established; but it can not be assessed—as Bretton attempts—by accepting without qualification the post-coup pleas of Party leaders that they were only following orders. While the Party had only a minor role in mass mobilization after independence, Bretton excludes the Party's role in linking groups of stalwarts, assimilating opposition blocs and recruiting new partisans. Bretton also minimizes the significance of other organizations both as repositories of power and mechanisms of control. His extensive survey of machine building omits the political struggles required and mistakenly treats Nkrumah as the sole beneficiary of power shifts. He exaggerates the stifling of parliamentary debates and underestimates civil service resistance. He ignores the strongest Party Wing, the Farmer's Council, which effectively challenged Government policy.

The residual system of personal rule failed, Bretton contends, due to its incapacity in four functional areas: it lacked the ability to learn and correct, sacrificed stabilizing institutions to the machine, mis-directed the economy and compromised the aim of ideologically inspiring leadership. The regime actually possessed relatively good information and certainly corrected some of its serious errors respecting agriculture and political disruption of the civil service and local government. Nkrumah did not destroy the stablizing framework; the continuing state institutions—army, police and civil service—destroyed him. Nkrumah did contribute to economic disruption and failed to bring about an ideological transformation. A serious assessment of these weaknesses would, however, require comparison with both Nkrumah's military successors and other African regimes.

Bretton prefers a broader canvas: he seeks to assess the "age-old pattern of personal rule" against the "whole spectrum of alternatives developed by mankind over a period of 4,000 years." History, rather than establishing conditions or limits, offers a smorgasbord of possibilities. Unencumbered by the decisional process, systems repercussions, culture, society or history, Nkrumah emerges in unpredicated freedom to decide a nation's destiny and only himself to blame in defeat. The fable may be a salutary reminder of the responsibilities of leadership, though I am unpersuaded of the rectitude of a judgment which systematically depreciates situational factors. It does not accurately portray the Ghanaian political process.—ERNST BENJAMIN, *Wayne State University.*

Philippine Collaboration in World War II. By DAVID JOEL STEINBERG. (Ann Arbor: The University of Michigan Press, 1967. Pp. 235. $7.50.)

The military occupation of the Philippines by the Japanese during World War II resulted in a long-lasting collaboration crisis for the Filipinos in their relationships with the United States and Japan. The Philippines were approaching independence in 1941 and had been relatively successful in blending Western and Eastern cultures. After the Japanese invasion and occupation, the necessity for survival of the Filipino people forced a degree of compliance with the new military government. The author notes that ". . . not everyone can go into exile or join a guerrilla band, and yet those left in occupied territory have the greatest need to preserve the sanctity of their national ideals." David Steinberg, a University of Michigan history professor, has researched very ably the actions of the Philippine elite or the establishment as they struggled with the countless conflicts that arose between 1941 and 1948.

The Filipinos, led by President Quezon strongly backed the United States in 1941. Although the United States had planned full military defense of the Philippines, the Pearl Harbor disaster caused a major change in the defense perimeter of the allied forces and left no hope that the United States would retake the Philippines in the immediate future. A major objective of the Japanese after establishing military control of the islands was to create a pro-Japanese government and a pro-Oriental society. Steinberg traces the steps taken by the Japanese to woo the Filipinos. An executive commission was appointed, comprised of well-known Filipinos who had been known to be in varying degrees sympathetic to the Japanese. The author traces the reasons for the selection of Jorge Vargas, executive secretary for Quezon, as head of the commission rather than Begnigno Aquino or Jose Laurel.

The Japanese recognized the necessity of matching the American plan for independence for the Philippines. Japan set the date for 1943 and the first preference for President was Manuel Roxas, who had been second in command in the Quezon administration. After much reflection on the part of the Japanese, they named Laurel to head the new Republic. Laurel and the Japanese were unable to alleviate severe food shortages and inflation *was so rapid* that the country was faced with an economic collapse. Laurel attempted to accommodate the guerrillas by amnesty declarations but was never able to eliminate the vague excitement of MacArthur's promised return. The nadir or climax of the Laurel administration came with the declaration of war against the United States in September, 1944.

After the American invasion of Leyte in October, 1944, Sergio Osmeña was named by the allied forces to head a pro-American Philippine government. The author describes the influence of MacArthur in securing the freedom of Roxas and ex-

plains now MacArthur played a key role in Roxas' 1946 election victory over Osmeña. Osmeña had been urged by the Americans to purge all Filipinos who held office under the Japanese occupation but he held firm to a policy of judging each case on its merits.

Steinberg has a penetrating analysis of the fate of the Filipino leaders who collaborated with the Japanese. The Philippine Peoples Court filed 5,603 cases of political collaboration but only 156 were convicted. The author selected 165 of the most influential Filipinos arrested by the Americans immediately after reconquest and compared their cases with the People's Court records. Of those arrested by the Americans, 122 had no record in the Peoples Court. Eleven, including Laurel and Vargas, had their cases dismissed or were not prosecuted. Only Teofilo Sison was convicted and his was the initial case. Thus, the author concludes, ". . . the elite remained intact. . . . The war diminished neither the size nor the authority of the establishment."—H. ODELL WALDBY, *Florida State University.*

The Citizen and the Administrator in a Developing Democracy: An Empirical Study in Delhi State, India, 1964. BY SAMUEL J. ELDERSVELD, V. JAGANNADHAM, and A. P. BARNABAS. (Glenview, Ill.: Scott, Foresman and Company, 1968. Pp. 170. $6.00.)

Eldersveld, et al., argue that economic development in India requires that the public understand and support government officials. Thus, they attempt to study the network of interactions between a highly differentiated set of officials and the heterogeneous publics they serve, "with the particular goal of determining the relevance of bureaucratic contact for citizen perceptions, attitudes, and orientations." (p. 7) The book is a conscious replication of Janowitz's 1954 study of public perceptions of Detroit officials and illustrates the current effort to extend scientific analysis to the study of politics in non-Western areas. At the same time, the authors' mistakes and omissions are indicative of the problems which such attempts have encountered. I will comment only on some of the substantive questions raised by their findings.

Underlying the argument of the book are a set of assumptions about what constitutes "modern" administration, and thus what the goals of administrative development should be. For example, the authors assert that "the basic test, then, for political development in our research is the relative strength and direction of the societal environmental factors and individual predispositions, on one hand, and the stimulus resulting from bureaucratic contacts, on the other hand." (p. 13) Eldersveld and his associates never discuss why stimuli from the bureaucracy rather than social and personal factors within the electorate should be the distinguishing feature of "modern" political processes.

The most interesting and provocative findings in the book raise questions about the nature and effects of urban life in India (or Delhi). These findings are scattered (pp. 20, 24–8, 31–2, 40, 42–3, 46–7, 49, 136–7) and never discussed as a whole. The findings indicate that rural areas are more supportive (i.e., less critical or "hostile") of officials and government than are urban dwellers and that they also have more contact with officials than do people in Delhi. Though the mean level of contact is low, in the urban sample criticism of administrators increases with contact. Why is it that the rural citizens are better informed, score higher on sense of efficacy, manifest more self-confidence in dealing with officials, and have more contact with officials than do city dwellers? Given greater contact and sophistication, why should rural citizens be more supportive of officials whereas increasing contact is correlated with greater hostility in the urban sample? Considering the lower response rate in the city (64% vs. 85% for rural) and the unexpectedly high rate of "don't know" responses, can these anomalous findings be artifacts of a biased urban (and/or rural) sample? If they are indeed valid findings, must we then re-evaluate our assumptions about the integrating and modernizing effects of urbanization when dealing with India?

This apparent paradox may stem from the authors' failure to clarify their own interpretation of respondents' criticisms of administrators. Sometimes they equate criticism with cynicism or disaffection, as when they label it "hostility." Elsewhere they question whether non-critical responses among rural groups (especially illiterates) really do indicate a genuine support of the government, thereby suggesting that criticism may be a reflection of "realism" (their term) in dealing with administrators and even of integration into the modern community. Considering the high levels of contact, efficacy, self-confidence, and knowledge in rural areas, one might guess that the proportion of "realistic" and integrative criticism is higher there than in urban areas. But why should this be so? One can only regret that Eldersveld and his colleagues have not gone more deeply into this aspect of the analysis, but have contented themselves with presenting largely descriptive material.

The interpretability of the data is also diminished by a perpetual vagueness about procedures. There is a suggestion, for example, that only men were sampled, but some women were interviewed (response rate 96%). It is not clear whether these women were included in the analysis. Likewise, the exact scoring of indices is never indicated, though usually the items in each index are men-

tioned. Castes are ranked into high, middle, and low, but with no indication of the specific castes in each category. Since the tables report no middle castes in the city, the Vaisya castes (who as traders, businessmen, and artisans constitute a large proportion of all urban areas) must be included in either the high or low cells, but no justification for this decision is given. Furthermore, the number of respondents is rarely and irregularly included in tables. In some places the authors mention that a particular finding is or is not significant, but nowhere is it stated whether this evaluation refers to statistical significance or to theoretical and practical importance. Finally, one wonders how well respondents understood some of the questions and how willingly they responded. Administrators may have been both confused and threatened when asked, "What per cent of the public do you think would say that people in your position are efficient?"

This review has focused on the defects more than the strengths of Eldersveld's efforts because these defects prohibit a full assessment of the data (which are quite interesting), and because the value of a "pilot project" depends heavily on its explicitness as a guide for future research. While some of the defects are due to faulty research design, others may be due simply to carelessness or to poor judgment on the part of a copy editor. Whatever the causes, one hopes that Prof. Eldersveld and and his associates will remedy these deficiencies in the reporting of his current survey efforts in India. There is no reason why survey research in non-Western areas should not meet the standards of rigor and precision expected of such work in the United States.—DAVID J. ELKINS, *University of California, Berkeley.*

Politics and Change in a Traditional Society: Lebanon, 1711–1845. By ILIYA F. HARIK. (Princeton: Princeton University Press, 1968. Pp. 324. $9.00.)

Students of political culture and change, whether or not they have a professional interest in the Middle East, will find Iliya Harik's study of the decay of the feudal order in Mount Lebanon illuminating. Harik has provided us with a careful analysis based on scrupulous documentation, and his historical discussion is always pertinent to his model of political change. The political dynamics of Mount Lebanon's crumbling feudal order are complex, but Harik is admirably concise in his exposition, and the reader not familiar with the area will be grateful for the clarity of his interpretations, chapter summaries, and conclusions.

The Imarah of Mount Lebanon from the mid-eighteenth to mid-nineteenth centuries is described as an iqta', or feudalistic, political system ". . . in which authority is distributed among a number of hereditary aristocratic chiefs subordinate in certain respects to a common overlord." (p. 37) Harik describes the political dynamics of this system, which was headed by the Ma'ni and Shihabi dynasties, in terms of a systems model whose three components are the legitimacy principle, institutionalized practices, and actors. The appearance of new and contradictory principles, practices, and actors causes a disruption of the system. Harik uses the model to discover the political loads that arose in the Imarah, no small feat given the complexity of its traditional pluralism and the fragmentary documentation available. In the stable iqta' order the legitimacy of the Hakim (Ruler) arose primarily out of the ancient class divisions of the Mountain, and authority was institutionalized in a gradated yet reciprocal relationship between the Hakim and the muqati'jis (local hereditary chiefs and collectors of the iqta'). The common people did not constitute an independent political factor. Sectarianism, one of the main characteristics of contemporary Lebanon, had no place in the iqta' order.

Why did this sophisticated system finally break down in civil war, sectarian strife and foreign intervention? In an analysis which must be commended to all interested in processes of political change, Harik shows how the institutional conflict between authority and kinship fragmented the power and enhanced disintegrative tendencies among the muqati'jis, and how the conflict between hereditary and electoral practices aggravated the problem of succession of the Hakim. On the systemic level a more serious disintegrative force was the rise of the Maronite community to political significance. Harik skillfully traces the origins of communal feeling among the Maronites and describes how the Maronite clergy and peasantry arose to challenge the legitimacy, institutions, and main actors in the iqta' order. He identifies the clergy as a principal source of political organization, innovation, and disruption, and he shows how it infiltrated the ruling structures, for example, through the administrative office of the mudabbir. It also politicized the common people and precipitated the popular uprisings of 1820 and the 1840's. Harik's masterly portrayal of the rise and fall of the Maronite mudabbir, Jirjus Baz to mention one of several examples, focuses sharply upon the conflict between the established and the innovating forces. He shows why the innovating forces, represented by the Maronites, were unable to perpetuate their newly-established position, how the Druze muqati'jis unwittingly undermined their own power position through excessive reliance on the Ottoman government, and how the last of the great Shihab Hakims, Amir Bashir II, maneuvered in vain to maintain a broad basis of legitimacy.

Inevitably, some points might have been clari-

fied. It would be helpful to have further explanation as to why the Shihabis became Maronite. Further attention might have been devoted to the reasons why a feudal spirit still persists in Lebanon, even among Maronites, despite its attrition by the new sectarianism and its official abolition by foreign powers. Evidence of the Lebanese entity notwithstanding, one still might question the intensity of national feeling among the people: thus, speaking of the qa'immaqamiyyah, is it perhaps an exaggeration to say that "(t)he problem in this partition arrangement was that the Lebbanese were not really willing to divide their country"? (p. 35). Compared to the particularistic relationships of the iqta' system, te emergence of a communal Maronite feeling may be called universalistic, but the latter designation seems inappropriate when viewed in terms of the confessionalism in the modern Lebanese republic. In the early nineteenth century communalism was, for better or worse, a modernizing force: today, however, in Lebanon and elsewhere in the Middle East the communal order, along with the still important remnants of feudalism, it would seem to be an obstacle to satisfactory political performance.

This study is not only a valuable contribution to our understanding of political culture and integration in Lebanon and the Middle East, it is also an informative case study of the effects of differential political development within a political community. The model is shown to be workable, fruitful, and relevant to modern political theory; as such, it deserves application elsewhere.—MICHAEL C. HUDSON, *Brooklyn College of the City University of New York.*

African Opposition in South Africa: The Failure of Passive Resistance. By EDWARD FEIT. (Stanford: The Hoover Institution on War, Revolution, and Peace, 1967. Pp. 223. $7.50.)

This work by Edward Feit, currently Assistant Professor of Government at the University of Massachusetts, is a revision of the author's Ph.D. dissertation at the University of Michigan entitled *Conflict and Communication: An Analysis of the "Western Areas" and "Bantu-Education" Campaigns of the African National Congress of South Africa Based on Communication and Conflict Theories.* It also draws upon his earlier monograph, *South Africa: The Dynamics of the African National Congress,* which was issued under the auspices of the Institute of Race Relations in London and published by Oxford University Press in 1962.

African Opposition in South Africa . . . is an incisive and dispassionate analysis of the strengths, weaknesses, and strategies of the oldest African nationalist organization in South Africa, the African National Congress, which dates back to 1912. Although, as the title of the doctoral dissertation suggests, it draws heavily from communications and conflict theories, it also utilizes both structural-functional and decision-making modes of political analysis. Such a behavioral approach to the increasingly arcane and complex field of ethnic politics and polities in the Republic of South Africa is all the more refreshing due to the generally barren wasteland of South African studies which has far too long been inundated with a surfeit of descriptive and/or polemical writing. Professor Feit's book is, without question, a significant contribution to South African studies in particular and to African studies in general because its research design is germane both to South Africa and to other countries and regions of Africa, giving it both comparative and heuristic utility.

Professor Feit's study of the African National Congress draws heavily from the records of the Treason Trial in South Africa as well as from *The Bantu World,* an African newspaper published in Johannesburg, which chronicles the events of interest to the urban African population of South Africa and which, to a considerable degree, reflects the attitudes of the African Third Estate towards the programs and personalities of the African National Congress. The Treason Trial records, which have by no means been fully exhausted by those engaged in research on South Africa, are a case of serendipity in terms of source material on the structure and function of the African National Congress. By utilizing these records, which have been catalogued by Professor Thomas G. Karis (in *The Treason Trial in South Africa: A Guide to the Microfilm Record of the Trial* [Stanford, California: The Hoover Institution on War, Revolution, and Peace, 1965]), Professor Feit has limned an unusually revealing portrait of this preeminent African political organization.

In terms of the subtitle of his book, Dr. Feit suggests that the failure of passive resistance against the South African Government in the campaigns against the removal of African shantytowns in Johannesburg (the so-called Western Areas) to newer townships in the peri-urban areas where the Africans no longer enjoy freehold tenure and against an allegedly servile educational curriculum (the so-called Bantu education) stemmed not just from the power differential between the white body politic and Congress but also from the dysstructural organization of Congress. Moreover, the failure was attributable to strategic myopia on the part of the leadership cadre of Congress which, as the author demonstrates, was a byproduct of the asynchronous structure of the African National Congress. One of the most crippling maladies of Congress was the lack of feedback, owing to the sclerotic nature of

the communications and command channels, and this, in turn, meant that there was a debilitating polycentrism within the decision-making structure. The dialogue between the leadership and the functionaries, on the one hand, and the leadership and its African clientele, on the other, was confused, discontinuous, and disjointed, with the result that there was considerable "noise" in the communications network. Closely related to the failures in organizational feedback were complementary failures in monitoring the activities and utterances of the subalterns by the elected national leaders.

In terms of strategies, Professor Feit cogently contends that Congress dissipated its rather limited resources in waging too many campaigns simultaneously rather than canalizing its efforts into one well planned and ably executed foray against the Government. Despite its self-appointed role as a mobilizing agent for the African majority, Congress nevertheless appealed to only a tiny fraction of the African masses; and, in addition, its penchant for coalescing with other ethnic groups in anti-Government campaigns alienated no small number of its articulate and politicized members who espoused a uni-racial, not a multi-racial, doctrine of Africa for the Africans. The decision-making elite of Congress was, as Dr. Feit has suggested, attuned to bourgeois values which placed a high premium on respectability which, in turn, fettered the leadership and caused them to intone the moderate idiom of non-violence and racial cooperation and parity, an idiom which increasingly alienated the militants. Here Dr. Feit's findings closely parallel those of Michels with respect to the social distance between leaders and clients, on the one hand, and leaders and their protagonists in parliament, on the other, although the command structure of Congress was such that it seemed to invalidate his iron law of oligarchy thesis.

The main flaws in Professor Feit's work are essentially minor and do not detract from the integrity of his research design or the importance of his findings. First, the concluding chapter was much too brief (a mere four pages); second, he made far too little use of the South African Hansards (and on the one occasion when he did—on pp. 148–149—he quoted not from the primary source itself but from a secondary source) and treated the M.P.s and Senators in Parliament who represented Africans at that time in far too cursory a fashion; third, he neglected to utilize one of the standard works on Congress (Mary Benson, *The African Patriots: The Story of the African National Congress of South Africa* [London: Faber and Faber, 1963]); and finally, he could with profit have been less parsimonious in his use of footnotes.—RICHARD DALE, *Southern Illinois University.*

The Politics of Violence: Revolution in the Modern World. By CARL LEIBEN AND KARL M. SCHMITT. (Englewood Cliffs: Prentice-Hall, 1968. Pp. 244. $5.95.)

In recent years many competent case studies and a few comparative analyses of political violence have been written, along with a small flood of essays on conceptual and theoretical issues in revolutionary studies, hortatory texts on the making of revolution, and establishmentarian treatises on their prophylaxis. James C. Davies, Chalmers Johnson, Nicholas Timasheff, and others have proposed new theoretical interpretations of revolutionary causation and process. However not since the publication of Crane Brinton's *Anatomy of Revolution* and George Pettee's *The Process of Revolution* three decades ago has anyone attempted a general, descriptively-comparative summary of what we know, or think we know, about revolution. This study by Professors Leiden and Schmitt is such an attempt, a remarkably good one. The authors have written the book primarily for students. In doing so they have provided a lucid, jargon-free, and reasonably comprehensive summary that provides both introduction to the subject and a bench-mark for further inquiry.

The book does not break new ground. It does not offer a new conceptual framework, propose a synthesizing theory, or report new comparative studies. In the authors' words it offers "a summary of the literature on the theory and analysis of revolutionary causation and development," to which they contribute modestly with brief studies of the origins and consequences of the Mexican, Turkish, Egyptian, and Cuban revolutions. The definitional question is dealt with first. The authors' reluctance to innovate is demonstrated by their reliance on Calvert's definition of revolutions as "forcible interventions, either to replace governments, or to change the processes of government." Elsewhere they distinguish several forms of revolutionary action: gradually-intensifying conflicts among large and powerful groups that, if unresolved, lead to civil or revolutionary war; coups d'etat; and mass uprisings and guerrilla activity, whose participants are largely outside the power structure. Unsuccessful attempts at revolution as well as successful ones are considered. A wide range of illustrative and theoretical material is used throughout. Most major and many other revolutionary movements of the twentieth century receive at least passing discussion; the classic revolutions get somewhat more than equal time. Lesser acts of political violence like riots and assassinations are seldom considered except when they occur in the context of revolution. There is for example no reference to racial turmoil or advocacy of revolution in the United States.

Much of the recent and older literature on revolution is summarized under conventional rubrics: The Prerevolutionary Environment, Stages of Revolution, Revolutionary Leaders and Followers, the Ideology of Revolution. The past 30 years have provided many new illustrations of the conventional catalog of underlying conditions that dispose men to revolutionary violence: foreign control, economic privation, demands for political participation, deterioration of elites and crumbling regime authority. But the only conceptual improvements represented here are new labels like "dysfunction" for these conditions, and the recognition that it is the deterioration of circumstances relative to men's expectations rather than absolute deprivation that frustrates men to the point of revolutionary violence. Evidence on the stages of revolution suggests—implicitly, not explicitly in this study—the inadequacy of Brinton's fever metaphor for conceptualizing the revolutionary process. The potential impact of foreign support and opposition on the genesis and outcome of political violence, made strikingly evident by post-war events, are recognized.

A number of myths about the nature of revolutionary actors are examined in the light of much evidence and then reinterred. Revolutionary leaders tend to come from the middle levels of their societies. Though some are fanatical and perhaps a few psychopaths, most are frustrated men of conviction, ambition, and great energy, not deviants in a clinical or criminal sense. They seldom if ever create revolts by themselves. Rather they mobilize followers from among their frustrated fellow citizens. *Lumpenmenschen* and peasants are said to be seldom among those followers; the upper segments of the working classes, the middle classes, and students form the backbone of most revolutionary movements. It is unfortunate that the authors, in making these judgments, give little attention to the conditions under which there has been mass mobilization of rural people by revolutionary movements, for example in China, Vietnam, Kenya, and the Congo.

A principle contention of the authors about revolutionary and related nationalist ideology is that much of it is designed *post hoc* and *ad hoc* to provide justification for revolutionary action and guidance for social transformation once revolutionaries have attained power. Neither they nor recent research provide much insight into the less formal justificatory ideas about political violence that circulate in prerevolutionary periods, or the relative importance of cultural attitudes toward violence, foreign ideologies, and indigenous revolutionary appeals for transforming discontent into political violence.

The four case studies that comprise the last part of the book are especially useful because for three of them—Mexico, Turkey, and Egypt—extant studies are partial and specialized rather than comprehensive. The case studies are too summary for many scholarly purposes but provide good illustrations of some of the generalizations developed in preceding chapters. The 300-item bibliography is occasionally idiosyncratic but more comprehensive than any other that is generally available.

Despite the extensive speculative and empirical material examined in the study it has certain lacunae. One is the relative inattention, dictated by the authors' regional interests, to revolutionary movements in contemporary Africa and Asia. Another is an unaccounted failure to incorporate the results of the systematic quantative studies made of political violence, either recent ones like those of the Feierabends and Tanter or older ones like that of Sorokin. The study can also be criticized for its failure to contribute to analytic clarity or theoretical synthesis, but to so criticize it would be to misconstrue its purposes. It is a wide-ranging and intelligible summary of the current state of knowledge, no more and no less. One of its principal lessons may be that such clarity and synthesis are still badly needed.—Ted Gurr, *Princeton University.*

The Origins of Communism in Turkey. By George S. Harris. (Stanford: The Hoover Institution on War, Revolution and Peace, 1967. Pp. 215.)

It is surprising that the relations between the Kemalist regime of Turkey and the Bolsheviks in Russia during the early 1920's have not received more attention. In origin, context, and purpose, there were many parallels. Both regimes emerged from a long series of conspiracies dedicated to the overthrow of corrupt government and the reform of their respective societies. In both cases, success came in the wake of overwhelming defeat in the First World War. Both groups were faced with stiff opposition by the victorious World War I allies supported by local factions, including in the Turkish case the legitimate government in Istanbul. There was then a definite basis for a community of interest between the two revolutionary regimes. The means for implementing such a community of interest were also available, since the two governments had a common frontier in the Caucasus, well beyond the reach of Allied military or political power.

On the surface at least, Kemalists and Bolsheviks acted in accordance with their apparently common interests. They concluded a formal treaty of friendship early in 1921, marking the first full formal recognition of the Kemalist government by a foreign regime. The Kemalists, who were militarily hard pressed by Greek forces advancing across western Anatolia, also received substantial mate-

rial assistance from the Bolsheviks. Friendly relations continued throughout the inter-war period, although significantly Atatürk made certain that his tight grip on the Turkish political system was not undermined in any way by Moscow-oriented communist organizations.

Indeed, it has been generally assumed that there was no Communist activity worthy of note in Turkey from the 1920's at least to 1960. There were political controversies in the late 1940's which involved charges of crypto-Communism and fellow travelling. These charges resulted in the dismissal of a number of professors from the University of Ankara, the resignation of the Minister of Education, and the end of the rather unique and highly interesting Village Institute program which had been designed to train primary school teachers and community development workers for the villages. This episode bore all the marks of a type of McCarthyism, however, and was probably politically motivated.

More recently, and particularly since the military coup d'etat of May 27, 1960, Marxist and other leftist ideologies have openly emerged on the stage of Turkish politics. The most concrete expression of these new trends has been in the form of the Turkish Workers Party. This group advocates extensive nationalization of business and industry, opposes foreign investment in the economy and American influences in particular, and favors a neutralist foreign policy. It has attracted widespread support among the intelligentsia but very little among workers and peasants, for whom it purports to speak. The question of identifying the political and intellectual antecedents of these latter day socialists calls attention once again to the gap in the literature on Turkish political history.

George Harris's interesting book is an attempt to fill this gap. It is a workmanlike history of the largely unsuccessful attempts to launch a communist movement in the confused and rapidly changing Turkish political environment between roughly 1900 and 1925. Harris is a close student of Turkish affairs, having spent about seven years in the country, five of them as an official in the American Embassy during the critical period of the late 1950's and early 1960's. The book reflects his familiarity with the subject. It provides valuable new insights into the turbulent formative period of the Turkish Republic.

Unfortunately, Harris limits himself to a straightforward account of chronological developments in the shadowy left wing of Turkish politics during this period. He therefore has missed the opportunity to comment on perhaps the most interesting contrast between Kemalism and Bolshevism. The Bolsheviks clearly represented a social and political upheaval of the most profound degree. Kemal's movement, on the other hand, was far more instrumental in its approach both to ideology and action. He was prepared to advocate revolution only in the political sense and only for the purpose of reestablishing Turkish politics on a stable foundation. Although he spoke of making the peasant master of the country, he never embraced any doctrine of total social reform or revolution. Moreover, while the revolutionary character of Bolshevism was evident from the first, the true nature of Kemalism did not begin to emerge until the Ankara regime was firmly entrenched in power. Indeed, it has been argued by contemporary leftist writers in Turkey that the Kemalists were socially conservative, and that the effect of their rule was to further entrench the traditional Ottoman elite. In the light of this analysis, the change of regime from Sultanate to Republic and of dominant ideology from Islam to republican nationalism were mere formal changes which failed to touch the basic social structure. Harris's account supports this analysis insofar as he documents Atatürk's consistent resistance to the social doctrines of the Marxists, even when he pursued a policy of cooperation with both domestic Communists and the Kremlin.

On the other hand, Harris's account gives concrete illustration of the attractive power of the Bolshevik Revolution for the underdeveloped nations. Time and again he shows how some of Kemal's supporters were disposed to look upon the Soviet regime as a model for Turkish development, and how easily Marxist ideas could be blended with the nationalism so typical of Turkey at the time and other countries of Asia and Africa since.

The book is occasionally marred by apparent slips of the pen. For example, on p. 99 there is a reference to "the present Turkish Communist Party." Since communism has been outlawed since 1925, it is not clear what is being referred to. Harris's conclusion to the effect that contemporary Turkish leaders have not succeeded in "incorporating . . . restless radicals into the system with the sure touch displayed by Atatürk a generation ago" leaves a great deal unsaid. Atatürk had an open mind to new and even radical ideas so long as his political domination was assured. Contemporary Turkish leaders, on the other hand, represent a social stratum that has traditionally been subordinate to the social and intellectual elite which spawned both Atatürk and the radicals who espouse Marxist ideas. To expect these new leaders to adopt the same expansive strategy towards ideological dissidents as the great national founder is therefore asking for a great deal. Indeed, this very fact speaks volumes about the social and political changes that have come about in Turkey and the grave problems that confront that country

today.—FRANK TACHAU, *University of Illinois at Chicago Circle.*

The Congress Party of India: The Dynamics of One-Party Democracy. By STANLEY A. KOCHANEK. (Princeton: Princeton University Press, 1968. Pp. 516. $13.50.)

Stanley Kochanek's study of the Indian National Congress is the third book-length study of the Congress to appear in the past four years. The other two, my own study of Congress party organization in Uttar Pradesh and Myron Weiner's book, *Party-Building in a New Nation,* were primarily concerned with the local, district organizations of the Congress and with the problems of building and maintaining strong party institutions in a modernizing society. The book under review has a different focus. It is a study of the relationships between party and government at the center and between the central and state units of the party in the period from 1946 to 1967, with an added long section on some aspects of leadership recruitment into the Congress.

The book is organized into three long sections —the first on the roles of and the relationships between the prime minister and the Congress president; the second on the executive institutions of the Congress, the Working Committee and its powerful sub-committees, the Parliamentary Board and the Central Election Committee; the third on Congress leadership. Also included is a postscript on the implications for the Congress organization of its loss of all-India hegemony in the aftermath of the 1967 elections. The book is based upon newspaper reports, Congress publications, personal interviews and questionnaires administered during two field trips to India, the private papers of three Congress leaders, and the minutes of the meetings of the Congress party in parliament. Some of this material is original and has not been used before by other scholars.

The organization of the study is largely historical. The history of the post-independence Congress organization is divided into three periods—a period of transition from 1946 to 1951, marked by conflict between the parliamentary and organizational wings of the party; a period of "centralization and convergence" from 1951 to 1963, marked by parliamentary dominance over the party organization and central dominance over the states; and a period of "divergence" from 1963 to 1967, marked by "equilibrium of power between party and government, center and states" (p. xxiv). The periodization is really nothing more than a statement of Nehru's personal role in party and government during his prime ministership, which, between 1951 and 1963, masked the great changes that were taking place in the Congress organizations in the states and which began to affect the functioning of central party institutions after 1963 It is unfortunate that this periodization is so frequently reiterated throughout the book and is used as very nearly the only organizational framework for the whole study, for it tends to obscure many of Kochanek's insights into the way the Congress system functions. For example, Kochanek does demonstrate that, through all three periods, the supremacy of the parliamentary wing on policy matters has been retained, but that party-government relations on matters of party and parliamentary power can become strained during periods of transition, both at the center and in the states. These points are worth making and they remain valid generally, irrespective of the periodization.

The periodization is also of limited use in analyzing central-state relationships in the Congress since the middle period is the longest and the one during which the most fundamental changes occurred in the character of the party organization in the provinces and in the relations between the provincial and central party organizations. Yet, in chapter ix, which deals with the Parliamentary Board and the issue of party-government relations in the states, two pages are devoted to the entire period from 1951 to 1963. More attention is devoted to this second period in other chapters, such as in chapter xi on the Central Election Committee, but here the applications of the periodization is not entirely convincing. Kochanek's argument is that the ability of the central leadership to intervene decisively in the candidate selection process has declined significantly since 1963 as a consequence of reduced unity among the national leaders of the party. This may be true, but Kochanek provides examples of effective central intervention from both periods—in the Punjab in 1962 and in Andhra in 1967.

Kochanek's analysis of party-government relations is also not satisfying. On p. 434, he points out that "the issue of party-government relations is more often than not a matter of intra-party factionalism in which the dialogue of competition for ultimate power in the parliamentary wing disguises itself as the problem of coordinating parliamentary and organizational wings." Yet, his history of party-government relations at the center in the first four chapters takes the dialogue for the reality. Thus, the great struggle of 1950–51, in which Purushottamdas Tandon became Congress president and was then forced to resign, to be replaced by Pandit Nehru himself, is interpreted as a grand conflict between party organization and the parliamentary party. In fact, it was probably less a conflict between Nehru and Tandon over party-government relations than a conflict between Tandon and Rafi Ahmad Kidwai, which

had its roots in factional politics in Uttar Pradesh. The fact is that we have not yet had a full account of this turning-point in Congress history, which has elsewhere been interpreted as a conflict of ideologies, a conflict between Sardar Patel and Nehru, and is in Kochanek's study a conflict between party and government. All these versions are the top of the iceberg, the rhetoric and inside newspaper dope of Indian politics which cover complex interrelationships of personal ambitions and factional alliances extending throughout a subcontinent.

Kochanek has also failed to relate his analysis of party-government relations in the Congress to the vast theoretical and comparative literature on this issue. How do party-government relations in India in fact compare with party-government relations in Great Britain, from which the Indian theory has been drawn? Kochanek argues that, as in Great Britain, the parliamentary wing of the party has emerged supreme. Yet, the dynamics of the system are entirely different. For, the party organization is the road to parliamentary power in India and the parliamentary wing is supreme only when a single group or coalition in the Congress is united and controls both wings of the party. In other words, it is not tradition and precedent or a clear constitutional definition of functions which determines the pattern of party-government relations in India, but the relative balance of political forces in both wings of the party at a given time. Moreover, the issue of party-government relations has never been primarily an issue of policy responsibility, but rather it has been an issue of power in the party and in the government.

I believe that Kochanek understands these things and his book shows evidence of considerable insight into the workings of Congress politics in India. He has also provided some useful data on the changing and broadening social bases of the Congress at different levels of the party. What is lacking is a theoretical or analytical perspective sharper than a superficial periodization or a conventional analysis of party-government relations in an unconventional context. The book is also far too long and repetitious. The sub-title of the book is misleading and is especially inappropriate after the 1967 elections. India has never had a one-party system.—PAUL R. BRASS, *University of Washington.*

Party and People: A Study of Political Change in Tunisia. By LARS RUDEBECK, (Stockholm: Almqvist & Wiksell, 1967. Pp. 275. SW. Kr. 25.)

Lars Rudebeck's *Party and People: A Study of Political Change in Tunisia,* is the latest of four major books on that country in English over the past four years, all of which are written by Western scholars. So far as I know, none of them has been translated into Arabic, the native language of Tunisia. This reviewer remembers with some concern how he searched in vain in Tunis' bookstores, during a visit in the summer of 1967, for books on Tunisia by Tunisians and finally settled for the only thing he could find—a biography of President Bourguiba put out by a government information office.

Faced with the disparity in scholarly contributions on development between the Western intellectual enterprise and that of the developing countries themselves, one's first impulse is to congratulate ourselves on our impressive record. However, before we rush to do so, it may be worth our while to ask if we are missing something of value by a failure to establish some measure of community of scholarship with the people we study. Lack of funds and pressing practical concerns may prevent intellectuals in developing countries from matching our record, but this should not make us believe that they do not exist. In studying developing areas, it is often felt, we listen to natives and talk to colleagues. It is going to take some time and thought, before we start learning how to talk to natives; and for a start we might cherish the hope of being treated some time to a review by a scholar from the same country we write about.

It perhaps adds to the interest in Rudebeck's book on Tunisia that it not only provides us with an excellent and informative account on the politics of that country but also illustrates important points about the state of our discipline. Consider the fact that Rudebeck is a Swedish scholar writing in English, with an American theoretical approach, about a North African country! Rudebeck has been influenced by the writings of David Easton and Gabriel Almond; indeed we may say that he has in this book followed Almond's scheme faithfully as it has been elaborated in *The Politics of Developing Areas.* The Almond scheme is very attractive and a too ready an acceptance of it may lead the researcher to gloss over the underpinnings of structural-functional theory upon which it has been based. Indeed, Rudebeck's book confirms the view sometimes held of Almond's version that it being so succinctly stated lends itself most easily to descriptive treatment of a political system. The author makes it clear, though, that not only is he concerned with describing how the party system functions in Tunisia but that underlying this is the thought that "an underdeveloped country today can be systematically developed only if its political system touches and mobilizes the masses of the people. I have investigated the case of Tunisia during the nineteen-sixties," he adds, "in order to illustrate and analyze this connection empirically."

There is a great need to establish empirically

the connection between development and political mobilization, especially since the sympathetic attitude of development scholars toward this thesis of Mr. Rudebeck's has so far not produced the evidence necessary for us to accept or reject it. To show that political mobilization of the masses in one or two cases has been associated with political development is by no means a proof of the generalization as it has been stated by Mr. Rudebeck. Instances just as good could be cited in which development takes place in non-mobilization regimes, too. It is perhaps because the proposition in its present form is not subject to demonstration that Mr. Rudebeck does not substantiate it and devotes his efforts instead to the reconstruction of the process of party activity in Tunisia.

In 1961, the Tunisian government made the decision to take a more active part in directing the economy and christened its new policy with the name of "Neo-Destourian socialism." The event marks a major step in the gradual but decisive process of greater governmental controls in the manifold aspects of society. The author finds in this new policy the fledging of a mobilization regime under one charismatic leader with a mass political party in the leader's service. Of particular interest in Mr. Rudebeck's monitoring of this transition is his extensive and interesting account of the recruitment function of party leaders at the regional level. The reader will also find his analysis of the party relations to provincial government and the involvement of President Bourguiba's Destourian Socialist Party in the socio-economic spheres very enlightening.

Arstide Zolberg in his essay, *Creating Political Order: The Party States of West Africa,* has judiciously warned against making too much of the dichotomy between political party and state in the single party regimes. The evidence on party-state relationship in Tunisia provided by Mr. Rudebeck could just as easily be construed in favor of Zolberg's observation as of the author's contention that the party has supremacy over the official agencies of government. More attention should be given to the presentation and evidence for this thesis by Mr. Rudebeck before it can be confirmed.

The chapter on the labor movement and the Bourguiba regime is informative and interesting and clearly shows the low tolerance point of mobilization regimes for the autonomy of any countervailing political force in the system. Mr. Rudebeck deserves to be commended for paying some attention in his study to the attitudes of students, especially when we know that although students in Tunisia have not yet reached the boiling point in their protest movement like the Paris or the Columbia students, they have nevertheless been the most cantankerous of all the groups with whom President Bourguiba has had to grapple recently. The account is based on a sample survey of fifty-nine students in their first year at the College of Sciences of the University of Tunisia. The survey seeks to elicit some background information as well as attitudes on a variety of subjects ranging from socialism to opposition to the regime. The book concludes with a summary account on the six system functions, namely political socialization, recruitment, communication, articulation and aggregation of political interests, and the application of political decisions. However, the material presented in the book is not directly related to all these functions.

Only at the end, in the note on "Sources, References, and Literature," do we learn a little about the method Mr. Rudebeck followed in his study, and we may suggest that this question deserves a more complete and explicit account as well as a more prominent place in the book. It seems that the author leaned heavily on the interviewing technique and his ommission of the text of his various questionnaires is particularly regrettable in view of their great value in appreciating the results of his study.

Party and Politics is a useful book for students of North Africa and the developing countries and will certainly have its place in the literature.—ILIYA F. HARIK, *Indiana University.*

Government and Revolution in Vietnam. By DENNIS J. DUNCANSON. (New York: Oxford University Press, 1968. Pp. 442. $9.50.)

Dennis J. Duncanson, now on the faculty of the London School of Economics, has impressive credentials for writing a political history of Vietnam. Following long service in the British administration of Malaya during the Communist insurgency, he served in the British Embassy in South Vietnam from 1965 to 1966, and during the four prior years, 1962 to 1966, he was a member of the British Advisory Mission to the Saigon government. (This is the second book of quality to come from the British Advisory Mission, which had six advisers at its peak. The first, *Defeating Communist Insurgency* [London, 1966], was written by its chief, Sir Robert Thompson.) In analyzing the political forces in Vietnamese society, Duncanson draws upon his professional service, which includes a number of Diem's monologues to him. However, this is not an anecdotal narrative but a carefully researched treatise in the spirit of an earlier book by another British scholar-diplomat, Donald Lancaster, whose *Emancipation of French Indochina* [London, 1961] was also published under the auspices of the Royal Institute of International Affairs. Duncanson reads Chinese, Vietnamese and French, and has documented his study from a wide range of sources. The book traces the

Chinese heritage in Vietnam, French colonial rule and its impact, the Viet Minh struggle, the regime of Ngo Dinh Diem and the role of the United States. Unfortunately, only part of the final chapter is devoted to the post-Diem years, a period for which analysis from the author's rich personal experience would have been welcome.

Duncanson selects themes from the past which help to explain Vietnam's troubled present. Whatever else divides the Vietnamese, he shows that they agree that their country must not be regarded as a dependency of China, but he adds the less widely recognized point that, historically, China has reached out to the south to influence Vietnam rather than to conquer it. In an excellent analysis, French colonial rule appears as a system which principally served French interests, though not exclusively metropolitan ones, and further increased the inherent Vietnamese political fragmentation. The French colonial regime is treated sympathetically, but the judgment is frank that it was a failure.

The author does not demonstrate the same emphathy for the Vietnamese Communists as he does for the French. He sees the National Liberation Front, and the Viet Minh before it, as simply the old Indochinese Communist Party which was a "subversive movement launched from outside Vietnam by the Soviet government as an instrument for embarrassing France." He regards Ho Chi Minh as an agent of anarchy who has worked assiduously to transform himself "from an obscure international agent of a foreign state" into a Vietnamese national leader. Though recognizing that the Communists have skillfully used persuasive techniques, he believes they have achieved their success primarily through coercion. They have exploited nationalism and social grievances in their scramble for power, but have little interest in the welfare of the Vietnamese people.

The author's diligent scholarship and elegant argumentation is admirable and yet his interpretation of the Vietnamese Communist movement and of Vietnamese nationalism is fundamentally weak. In this reviewer's opinion, he errs in viewing Vietnamese nationalism as "entirely negative' and "an excuse for sectarian bids for power." Nationalism has been a continuous driving force in the two post-World War II Vietnamese revolutions, and the Viet Minh and Viet Cong leadership have not simply manipulated nationalist slogans to install a foreign-oriented Communist regime, but fight as dedicated patriots—at great personal sacrifice—to achieve what they regard as independence and justice for their country. One need not find attractive the system they would implant, if victorious, to recognize that their strength has been in welding communism to nationalism as a single, powerful force.

Duncanson has captured the autocratic, arrogant style of the ruling Ngo family whose recipe for power he deftly describes as a "judicious mixing of intrigue, nepotism, simony, public humbug and private intimidation." It is surprising, therefore, that he attributes the collapse of the Diem regime not to its fundamental political weakness, which clearly emerges in his book, but to Diem's "ignorance how to administer the ordinary machinery of government." Administrative inadequacy of the Diem regime is his fundamental theme, and the American lack of understanding of this factor, his major reason for their failure in Vietnam. Is the British colonial administrator's "build a better administration" the magic counterpart to the American's "build a better mousetrap"? Despite this overemphasis upon the importance of effective administration, this book is unsurpassed in its description and analysis of modern Vietnamese administrative and legal practices. It is also excellent in describing the weaknesses of the American mission to Vietnam, particularly its lack of direction and coordination, the internecine rivalry between the State Department, the CIA and the military, competition which Diem, Nhu and their minions often exploited for their own interests.

As a valuable history of Vietnam, this book will remain on the reference shelves along with Buttinger, Devillers, Fall, Hammer and Lancaster, long after many of the current polemics have been forgotten.—Joseph J. Zasloff, *University of Pittsburgh.*

Communist China: The Domestic Scene 1949–1967. By Peter S. H. Tang and Joan M. Maloney. (South Orange, New Jersey: Seton Hall University Press, 1968. Pp. 606. $10.00.)

Professor Tang is a frequent contributor to the growing body of literature on contemporary Chinese politics. Of his previous publications the best known is a highly readable and useful text entitled *Communist China Today: Domestic and Foreign Policies* (1957 and 1961). The present volume, which he has written in collaboration with Dr. Maloney, constitutes another welcome contribution to the field. While drawing substantial material from the previous text, this book has incorporated sufficient new information to make its treatment of Communist China's internal affairs comprehensive and up-to-date.

The main body of the volume consists of eleven chapters. The first two chapters trace the ideological and historical background of the Peking regime. Chapter III examines the Chinese Communist leadership with special focus on Mao Tse-tung and his thought. The subsequent chapters give a detailed and balanced description of the entire structure and operations of the Communist system in China, including Party, state, economic,

military, social, and cultural organizations. Particular attention is given to the nature of the Great Proletarian Cultural Revolution and its impact on the various aspects of national life.

In preparing this book the authors have used as their chief sources the great variety of publications and official statements of the Chinese Communists. The significance of such sources is attributed to "the naked frankness of the Chinese Communists in identifying their objectives and aspirations" and "the remarkable consistency in their line of reasoning and between their words and their deeds." The authors express some reservations about the reliability of occasional eyewitness accounts by foreign visitors but make no comment on the value of refugee interviewing as a research tool for the understanding of Communist China.

The book, on the whole, reflects a high quality of scholarship. It is well researched and clearly written. It provides both factual detail and perceptive analysis. The inclusion of a selected bibliography and numerous charts, illustration, and tables further enhances the worth of this volume.

One of the highlights of the book is its coverage of the Cultural Revolution. Although the authors' account and interpretations are generally interesting and objective, there is room for disagreement. They are correct in picturing the Cultural Revolution as essentially an ideological struggle and a determined drive on the part of Mao to rectify the Party and transform human nature in conformity with his revolutionary line. Nevertheless, they tend to overestimate the strength of "the thought of Mao Tse-tung" and are too optimistic about the beneficial effect of the Cultural Revolution. It is true that through the great upheaval Mao has succeeded in generating considerable mass enthusiasm for the revolution and in cleansing the Party of a large number of "revisionists." These gains, however, have been secured at a staggering price, namely, the breakdown of authority and discipline and the spread of factional rivalry and strife. From a short-run perspective, Mao indeed can prevail over the opposition and prove once more the "infallibility" of his leadership. But in the long run "the thought of Mao Tse-tung" is likely to undergo modification as the Chinese Communist revolution, like other crusading movements, will have to come to terms with realities.

There are other arguable points in the book. For instance, this reviewer finds the following statement (p. 29) less than persuasive: "Having tested these principles [Marxist-Leninist principles as refined and applied by Mao Tse-tung] and found them adequate, both pragmatically and intellectually, the Chinese Communists believe in the absolute effectiveness of this ideology for all areas of national and international programs and policies." The fact that the debacle of the Great Leap and setbacks in foreign affairs have led to a shape division between Mao and many of his top associates does not seem to fully substantiate this generalization.

These occasional lapses should in no way distract from the value of Tang and Maloney's commendable work. To the general reader as well as the specialist, *Communist China: The Domestic Scene* is highly recommended.—SHAO-CHUAN LENG, *University of Virginia.*

The Russian Revolution of 1905: The Workers' Movement and the Formation of Bolshevism and Menshevism. By SOLOMON SCHWARZ. (Chicago: University of Chicago Press, 1967. Pp. 361. $8.95.)

Mensheviks are really extraordinary people. In 1917, confronted by the proclamation of a "dictatorship of the proletariat," an act which they condoned although they disapproved, they tried vainly to ameliorate the situation with a socialist compromise. Crudely rebuffed, they retaliated with the only act of protest of which they were capable—they walked out of the hall, and for all practical purposes, out of Russian history. Trotsky, exulting in the triumph of the moment, thundered his famous taunt after them: "Go where you belong from now on—onto the garbage heap of history!" The Mensheviks have deserved far better than "the garbage heap of history." Moving from country to country, in a state of permanent exile, they have nursed no illusions about a triumphal return nor have they recruited any congressional lobbies on their behalf. A half-century later, the surviving remnant asks no more than to be permitted to record the history of their movement, perhaps to clarify thereby the history of the Russian revolutionary movement in its broadest sense.

The fact that Solomon Schwarz was himself a participant in the Revolution of 1905 resurrects the perennial problem of whether revolutionaries should write the history of their own revolutions. Whatever the merits of the negative argument, Schwarz has managed to turn his personal involvement into an asset. Although he is scarcely writing a memoir, he does convey the feeling and spirit of the participant, of the young Social Democrat whose heart later was given to the Mensheviks after the fact but who was then caught up by the more radical and revolutionary activities of the Bolsheviks, the so-called "generation of Lenin." Much of the drama of 1905 is already known: the miserable war with Japan, Bloody Sunday, the incredible obtuseness of Nicholas II, the granting of the October Manifesto, and finally the ebb of the Revolution. However, most western histories are histories of elites in which the revolutionaries usually appear only as a nameless and faceless mass.

Schwarz's effort seeks to tell how the revolutionaries saw the revolution—at least in St. Petersburg.

In its place in the History of Menshevism series, his volume does appropriately provide a thorough analysis of the role, and sometimes lack of a role, of Menshevism in 1905. At the same time, it is a history of socialism in 1905 with as many accolades to the Bolsheviks as to the Mensheviks. As Schwarz demonstrates from the record of his own activities in 1905, somehow the militant activist tactics of Bolshevism seemed ever so much more relevant to the situation of that year. Schwarz does take issue with Bolshevik historians for distorting both Bolshevik and Menshevik roles in 1905 and particularly feels compelled to reassert the Mensheviks' historic role in creating the Soviet in 1905.

The volume is by no means a literary gem—a flaw that cannot be ascribed to the translator. It is pedantic, at times overly tendentious, and certainly far too generous with long quotations which might well have been shortened, summarized, or even omitted. The research is extraordinarily thorough and the author's grasp of the sources unquestioned. Curiously, there is no bibliography—an astounding omission in a monograph published by a distinguished university press. Schwarz is of no help in explaining the rural violence—for that the scholar must still turn to G. T. Robinson's *Rural Russia under the Old Regime*—the ministerial crisis, or the ambiguous position of Russian liberals. Schwarz's concern is with the socialists, a concern hitherto abandoned to Soviet historians who are saddled with their own albatrosses. Defects notwithstanding, by providing this intelligent analysis of socialism in 1905, Schwarz has filled a major gap in the history of the Russian revolutionary movement.—Warren Lerner, *Duke University.*

Lesotho, Botswana, & Swaziland: The Former High Commission Territories in Southern Africa. By Richard P. Stevens, with three chapters By H. George Henry. (New York: Frederick A. Praeger, 1967. Pp. 294. $7.50.)

A decade ago news stories on Vietnam rated no more than page 37 of the *New York Times* under a Macy's ad; today there is similar unawareness, and similar future headline potential, of the new nations of Lesotho, Botswanna, and Swaziland. Tucked away in southern Africa, these former British High Commission Territories are shown on most atlases and globes under their previous names of Basutoland, Bechuanaland, and Swaziland (only the latter name has not changed, but it did receive its independence after the book under review was published). These countries and the even newer named Namibia (formerly South West Africa) are pawns in a fascinating and desperate gambit by South Africa to insulate itself against attack by the Black African states to the north.

The White minority rulers of South Africa cast their shadow over all these countries. Determined not to desegregate their own country out of fear that even a taste of the "good life" will start South Africa down the "slippery slope" of massive revolution, they intend to use the disenfranchised Black majorities as buffer units against the anti-White nations that dominate the African continent. Internally in South Africa, this program is taking the form of "Bantustans"—quasi-autonomous Black enclaves forming worker pools for "border industries" located in the White areas surrounding the Bantustans. Externally, South Africa hopes that Lesotho, Botswana, and Swaziland will become *de facto* Bantustans. The delicate policy appears to be to keep these Bantustans from becoming politically or economically independent, but also to give them enough of a stake in their own growing, dependent economics that they would not become allied with military or sabotage movements from the north. To do this, South Africa has to extend some economic aid, primarily infrastructural, to these enclaves so that they will have a degree of stability, be able to be easily policed, and become efficient sources of daily migratory labor. So far, South Africa's economic overtures and political gestures appear to have achieved some success, inasmuch as there seems to be no overt hostility to South Africa in these territories. Thus they may be on their way toward becoming buffer states in the true sense of that word. On the other hand, quite out of control of South Africa, guerrilla movements and hopeless but harassing border attacks against South Africa and Rhodesia are occurring with increasing frequency.

Against this strategic backdrop is an exceptionally fertile field for scientific political analysis. Are the new Bantustans viable new national units? What degree of integration are they achieving internally? What degree of integration may be found in the entire complex of nations in southern Africa? Is there a rudimentary "common market," as South Africa would desire? How many international "transactions" can be discerned, indicating the degree of reliance of the new nations on South Africa's economy? What is the proportion of imports to GNP? How are the guerrilla movements financed and supported? What is their local, as well as external symbolic significance? How do the United Nations' sanctions against Rhodesia influence the position of the former High Commission territories, and Namibia, against both Rhodesia and South Africa? These are questions not only of the greatest theoretical importance, but of potentially the greatest practical importance as well. By utilizing recent scientific and hard-data techniques developed in the discipline, political scientists are

presented with a unique opportunity to apply their theories and methods to an area that will surely become the focus of world attention.

It is extremely ironic, therefore, that hardly any one at present seems to be taking up the challenge. Certainly the book under review does not even make a nod in these directions. There is no work, to the present reviewer's knowledge, dealing with integration and transactional analysis of the southern African countries. African Study centers seem to exclude this entire area from their main concern. Yet it is here where these centers, drawing upon interdisciplinary methods in political science, sociology, linguistics, and anthropology, among others, can apply their theories in a practical crucible. For example, the problems of political culture, nationalism or integration, linguistics (South Africa's new policy of instructing Blacks in their prescientific Native languages), political development, and so forth, here acquire an immense practical importance in an area that is soon destined for radical change.

The most that can be said for the book under review is that it has chosen an important topic. Otherwise, it is simply a "current history" approach to political science, highlighting willy-nilly those topics that appear in local newspapers and government reports—party histories, political personalities, names, dates, legal movements, economic plans, inventories of natural resources. Missing are the more significant factors of integration and nationalism, guerrilla movements, South African strategy, the policies of Zambia and Rhodesia, the Bantustan concept, transactional economic analysis, and related topics. To an extent, of course, the authors could make the hoary rejoinder that they did not choose to write about these things, and that they cannot be criticized for not writing a different book. However, it might be said by way of reply that they raise expectations by their title that are not fulfilled. And perhaps more importantly, their own conclusions are not proven by the evidence they adduce. Professor Stevens' most important conclusion is that the United Nations and Great Britain should furnish considerable economic aid to these territories so that "South Africa can be prevented from using these Territories as hostages in her conflict with the rest of Africa." But he has not shown what sense of the term "hostages" he means or whether any definition of such a term would be correct in his conclusion. Indeed, South Africa's policy might well be to use these lands as willing allies, and not at all as "hostages," and to this extent may itself be providing them with considerable economic aid. Perhaps by extending aid, the United Nations is simply lifting a burden from South Africa. Or perhaps not; maybe aid is desirable for other reasons. But to make a judgment on this issue, Professor Stevens would have to consider the problem from a much more complex strategic viewpoint than the recounting of facts, names and dates. His present work amounts to little more than three good encyclopedia articles under the headings of Lesotho, Botswana, and Swaziland.—ANTHONY A. D'AMATO, *Northwestern University.*

Die demokratische Willensbildung in den politischen Parteien. By UTE MÜLLER. (Mainz: v. Hase & Koehler Verlag, 1967. Second edition. Pp. 175.)

The Basic Law of the Federal Republic of Germany of 1949 resolved that "the political parties participate in the forming of the political will of the people", that "they may be freely formed", that "their internal organization must conform to democratic principles", and that "they must publicly account for the sources of their funds" (Art. 21,1). The Basic Law further stipulated that "details will be regulated by Federal legislation" (21, 3). Eighteen years and some five abortive legislative proposals later, the *Bundestag* finally acted on this mandate and passed a "Law on the Political Parties" in the summer of 1967 (*Gesetz ueber die politischen Parteien [Parteiengesetz] vom 24. Juli 1967, Bundesgesetzblatt* I, 1967, No. 44 of July 27, 1967, pp. 773–781). After the processes of nominating candidates for public elections had previously been regulated by electoral legislation, and after the Federal Constitutional Court had already interpreted, and effectively applied (in the cases of the *SRP* and the *KPD*), the Basic Law's principles on the unconstitutionality of certain political parties, the debate preceding the legislation of 1967 had primarily focused on the following issues: 1) The publicity of the parties' revenues and financial transactions (cf., for an excellent recent survey, *Rupert Breitling's* article on "Literatur zur Partei- und Wahlfinanzierung", *Politische Vierteljahresschrift* 9 [1968], pp. 99–120, as well as the 1963 special issue of the *Journal of Politics*, edited by *Rose* and *Heidenheimer*); 2) Financial and other support from public sources for parties' election campaigns and related activities (*cf. Heinz-Christian Jülich, Chancengleichheit der Parteien,* Berlin 1967); 3) The internal organization of the political parties, especially with regard to the legitimization of party leadership and the democratic nature of their internal policy-making processes.

After *Ulrich Lohmar* (*Innerparteiliche Demokratie,* Stuttgart 1963) had provided an earlier study on the subject, *Ute Müller's* book is one of the last contributions to the debate on this third issue to have appeared prior to the *Parteiengesetz* of 1967. On the basis of the constitutional provision that the political parties' internal organization "must conform to democratic principles," she

attempts to further interpret this norm, and undertakes to ascertain the degree to which the actual internal organization of the three major West German parties (*CDU/CSU, SPD,* and *FDP*) conform to that norm. The organization of the book reflects this approach: The first part provides an extensive account of the electoral and decision-making practices in the three parties against the background of their respective statutes; at the beginning of the second part, an attempt is made to translate the general constitutional principle of "internal party democracy" into some more specific normative categories which are then applied to the evidence provided in the first part.

It would appear that whatever value this study has for the political scientist, lies in the information contained in the first, descriptive part of the book. The second part, although interesting in its own right as a piece of legal-normative argumentation, is of less relevance to an understanding of the internal functioning of the German political parties. The first part, however, does present a few rather interesting insights into the internal workings of the three major parties—a field in which, given the general scarcity of information, every bit of evidence is highly welcome. With regard to the processes by which party offices are acquired, a rather striking general pattern, as well as some significant variations between the three parties, become apparent. The highest executive organ of each party on the national level, the *Bundesvorstand,* supposedly derives its legitimation from an election process in which the will of the party membership is expressed, even though this expression may be channeled through various levels of delegation from the local party unit on up. This "uninterrupted chain of delegation," however, is far from being a reality in all the parties, and is not more than an insignificant residual category in at least one. Thus, 77% of the members of the *SPD Bundesvorstand* are elected on the basis of such "continuous" delegation—still an impressive figure if compared to the 38% and 6% in the cases of the *FDP* and *CDU,* respectively. The remaining seats on each *Bundesvorstand* are filled in a variety of ways, primarily by cooperation, ex-officio arrangements, and the vote of non-elected party officials on lower organizational levels.

While some of these figures, given the complexity of the electoral and delegating processes, may be arguable within certain margins, the overall picture remains one of the existing party leadership exercising a virtually decisive control over its own perpetuation and replacement, with hardly any serious challenge from the party membership: "The incumbents of leadership roles are in a position to manipulate the electoral mechanism in such a way as to make their will the decisive factor in determining the future occupation of these roles." (p. 66). By and large, this pattern is replicated in the processes of internal policy-making, especially with regard to both the power and the representativity (or the lack of both) of the national conventions (*Bundesparteitage*) of the three parties. The analysis of the rules of procedure, and of the procedures themselves, tends to support, with minor variations across the parties, the author's summarizing statement that "the influence of the membership (on policy decisions) has to be regarded as very moderate, if not totally insignificant" (p. 92). Something of an exception in this regard is, again, the *SPD,* where a great deal of policy initiative is normally generated at the bottom of the party hierarchy but where, due to the traditional discipline of that party, not many of these initiatives are ever allowed to disturb the preconceived will of the party's leadership.

As a contribution to one narrowly defined aspect of the overall debate on the role of the political parties in the West German political system, *Ute Müller's* study provides some interesting data, and an intelligent, if at times less than cogent, normative evaluation of these data. One is struck, however, by the almost total absence of any conceptual framework which, especially in the first part, could have helped to provide some categories for analyzing what is now a rather overwhelming mass of minute detail. While this does not necessarily invalidate the factual information presented, it makes any attempt at explaining some of the more intriguing variations across parties and party levels virtually impossible. In this respect, *Lohmar's* earlier study on the same subject had shown a considerably higher degree of conceptual skill. It would also seem to be worth a critical note that not one non-German source is either used or even listed in the bibliography. Given the vast amount of research and theory on political parties and their internal operations available in such languages as English (*Lipset, McKenzie,* etc.), this selectivity, together with the total absence of any comparative consideration, gives the study a somewhat parochial quality. In terms of the scope of the study, it is regrettable that the author did not see the need for including a discussion of the role of the parliamentary parties (the *Fraktionen*) in the internal decision-making processes. At least for the parties in power, it should be quite obvious—as she herself quotes the then executive chairman of the CDU as admitting in 1962—that "the political influence of the federal party (in the past four years) was confined to approve of the decisions of the federal government and the *Bundestag* parliamentary party, and to accept these decisions as the party's program . . ." (p. 76).—HANS N. WEILER, *Stanford University.*

INTERNATIONAL POLITICS, LAW, AND ORGANIZATION

France, DeGaulle and Europe: The Policy of the Fourth and Fifth Republics Toward the Continent. By SIMON SERFATY. (Baltimore: The Johns Hopkins Press, 1968. Pp. 176. $6.95.)

In a lucid and carefully documented examination of France's European and Atlantic policies, Professor Serfaty finds more similarities than differences between the Fourth and Fifth Republics, even though he characterizes the former as France "declining" and the latter as France "ascending." In the Atlantic Alliance, the leader of Fourth Republic France saw "a means of recovering, at least cost, Great Power status." (p. 40) As in the Fifth Republic, however, the fear existed that the United States was creating a "quasi-permanent system of intervention into European affairs." (p. 40) Between the Fourth and Fifth Republics, French attitudes about the Atlantic Alliance as the "best of the worst systems" did not change greatly. In the Fourth Republic the debate over the Atlantic Alliance had two focal points: Would the Alliance strengthen France and Western Europe? Would it contribute to a reduction in the East-West conflict? In the Fifth Republic, it might be added, French policymakers asked similar questions but reached different conclusions about the efficacy of NATO membership. In both the Fourth and Fifth Republics French leaders strove to restore France to Great Power status. If the preservation of the French presence in Southeast Asia and North Africa occupied the attention of Fourth Republic leaders, President DeGaulle saw the strengthening of France's position in Europe as the key to French influence in international affairs.

The parallels between Fourth and Fifth Republic policy extend to military affairs. Although the author might have addressed himself more fully to the evolution of French strategic thought, he nevertheless reminds the reader that France in the Fourth Republic, as in later years, was not prepared to provide large-scale conventional forces to NATO. If she left her nuclear defense to the United States, France nevertheless began in the Fourth Republic the development of a national nuclear force—a force which Professor Serfaty believes is likely to become obsolescent if the superpowers press their existing technological advantage. The author bases this judgement on the assumption that one or the other, or both, superpowers will develop ABM systems. He might have strengthened his case by reference as well to the offensive strategic capabilities, including multimegaton missile forces, which the superpowers will be able to deploy within the next several years. As a result, the imbalance between French strategic capabilities, on the one hand, and those of the United States and the Soviet Union, on the other, may widen.

The theme of continuity is present in Professor Serfaty's treatment of French policy and the unification of Europe. In the Fourth Republic French leaders expressed exasperation that Britain remained aloof from the European unification movement and showed a preference for closer ties with the United States than the Continent. In this reviewer's opinion, Professor Serfaty is correct in his conclusion that only a wholehearted British commitment to a supranational Europe could have led France to ratify the European Defense Community Treaty. In 1954, however, France was not prepared to be "shut up in a small, dark box" with Germany; by January, 1963, when DeGaulle both excluded Britain from the Common Market and signed the Franco-German Treaty, France had recovered a measure of strength and self-confidence unimaginable just a decade earlier. The French action of January, 1963, represented a change in policy, although the reconciliation with Germany remained a priority objective of French leaders in both the Fourth and Fifth Republics. In the post-war period, French support for German participation in European integration "represented a regional adaptation of the reliance on international guarantees" of the interwar period, in which France had surrounded her potential enemy with bilateral agreements. (p. 11).

For a book devoted to French policy toward the European continent, comparatively little attention is given to French relations with the communist states of Eastern Europe. Although he describes DeGaulle's long-held belief in the unity of states on both sides of the Rhine, the Alps, and perhaps the Channel for the achievement of equilibrium and peace in Europe, Professor Serfaty does not examine in detail the policies chosen by the Fifth Republic to hasten the creation of a Europe based on the Gaullist vision of "Europe from the Atlantic to the Urals." Given their importance in French policy especially between 1964 and 1968, the author might have enhanced the value of his work by addressing himself more fully to relations between France and the communist states of Europe.

Although much of the author's attention is focused on foreign policy, he examines changes in the French political system between the Fourth and Fifth Republics. With the emergence of a "new sociological France" which is more immune to the ideological quarrels of the past, "France is

developing large and loose political formations of the American type." (p. 40) To what extent, the author might have asked, are the quarrels of the past likely to be replaced by a new set of divisive issues which will produce changes such as those which shook the Fifth Republic in the Spring of 1968?

Professor Serfaty concludes that, in the Fourth Republic, it was difficult for a government to devise a coherent foreign policy, or, having done so, to insure its implementation. "With every government described as a caretaker government, and every year regarded as a year of transition, the Fourth Republic itself was a Republic of transition, and even the well-known continuity of the heads of personnel at the Quai d'Orsay could not prevent *immobilisme* from eating at the conduct of foreign affairs." (p. 84) The feature of French policy, which the author does not adequately explain, is its continuity despite basic differences between the institutions of the Fourth and Fifth Republics. But the continuity of French policy, even with institutional changes between the Fourth and Fifth Republics, gives added weight to Professor Serfaty's contention that the demise of DeGaulle is not likely to lead to a transformation of France's relations with other powers. If the author is correct, his book has considerable utility not only because of its insights into French affairs past, but because of the perspective it presents to the student and policy maker concerned with the likely shape of French policy over the next generation.—ROBERT L. PFALTZGRAFF, JR., *University of Pennsylvania.*

Can We Win in Vietnam? BY FRANK E. ARMBRUSTER, RAYMOND D. GASTIL, HERMAN KAHN, WILLIAM PFAFF AND EDMUND STILLMAN (New York: Frederick A. Praeger, 1968. Pp. 427. $7.95.)

The Hudson Institute has found an admirable way to combat two damaging, and often undeserved, public impressions about independent, non-profit defense research contractors: that they are monolithic, and that they are stooges for, or at least constrained to be uncritical of, a Service or Administration policy line. The solution here is to bind together the disparate views of individual members and exhibit the Hudson staff to the public in the process of arguing and in many cases disagreeing sharply with each other and with official U.S. Government positions.

This dialectical approach is, moreover, substantively rewarding when the subject matter is so complex and controversial as that of Vietnam policy. No one writer, even if he tries, is likely to give attention to more than a few of the different relevant points of view and paths of approach to these problems, and each of the five writers represented here adds significantly to the discussion, as does their direct confrontation in a roundtable symposium concluding the book.

In fact, the adversarial format of the book seems so effective a framework for explicating major issues underlying policy questions that one wishes its potential had been exploited somewhat more. The papers by Frank Armbruster and Raymond Gastil, on the one hand, presenting the case for pressing on toward past Administration objectives in Vietnam via a considerably modified military/police strategy, and the opposing papers by Edmund Stillman and William Pfaff, arguing for cutting losses, seem to have been written in isolation from each other and rarely meet head on. Instead of the comparison of evidence and cross-examination of opposing hypotheses that the book's structure seems to promise, one finds that quite different factors and issues are raised in each essay, and the various assertions slip past each other with only occasional challenge. This is compensated by Herman Kahn's three chapters, in which he goes beyond the written contributions of the other authors to summarize and compare their points of view and differences.

The discipline of this responsibility is obviously valuable to Kahn's own style of presentation, which throughout this book is sober, judicious, and lacking in flamboyance. (His concluding sentence—apparently written after the Tet offensive, while the book was conceived and mostly written before it—is understandably but unwontedly diffident: "It seems to me that despite the general feeling today that everything has been tried and nothing works, and that we are in a morass, we can in fact responsibly—with good faith and good heart—dedicate ourselves at the least to an intelligent temporizing action, and possibly to victory itself." As a response to the clarion question of the title, this sounds rather an uncertain trumpet.)

Three of the authors—Kahn, Armbruster and Gastil—are described as in "limited but reasonably broad agreement with many of the policies and objectives of the Johnson Administration" (though Gastil's discussion of Vietnamese realities leads him to such conditional and pessimistic conclusions that it is not clear how far he should really be linked to the other "war-winners.") Nevertheless, they take strong exception to many Administration positions and policies (including "attrition" theory and practice, indiscriminate use of firepower, escalation as it has been pursued, and the then-current bombing campaign in Tonkin) and propose recommendations, many of which, though familiar in classified policy debate (various experts on counterinsurgency have proposed them before in Vietnam), would represent radical, and most improbable, changes in current U.S. strategy, deployment and approach.

These measures would, I believe, be to the

good. (They would still, I also believe, be far from adequate to "win"; throughout, the discussion takes inadequate account of the strengths of the Viet Cong, and underrates the vital importance of political aspects of the struggle.) But the very fact that they have never been implemented properly, or at all, is by now a historical datum whose true significance these three writers manage to ignore. To see its bearing mainly as they do—as positive token that "there is large room for improvement" in U.S.-GVN progress and performance (p. 307)—seems almost perverse. The assertion cited is most certainly, poignantly true in 1968. But it was equally true in 1958, and 1961, and 1965. And the impending "possibilities" for improvement looked equally "great" (to cite another of Kahn's premises for his conclusion that "we may yet win the war in Vietnam") in those years. What makes these perceptions appear clearly inadequate today as a basis for either prediction or policy—just as, we can now see, they were misleading then—is precisely that the room for improvement remains today just as large, and in just the same places, as it was ten years ago, with nearly all the prospective "possibilities" remaining just that.

To understand why we have not done better in Vietnam, and to appraise the prospects and likelihood of doing better in the future, it is by this time far from enough to say, "There are promising measures, some of which worked well, in other hands, against other opponents elsewhere, that have never been fairly tried by the U.S. in Vietnam." One must ask, and attempt to answer, just *why* they were never tried; what reason there is, now, to believe they are more likely to get a fair trial in the future; and if they do, how likely it is they will be fully effective against this enemy, in this country and time?

It is the events of the last seven years, and especially the last three, that have demonstrated the salience of these questions (and suggested discouraging answers); though not, it would seem, to Kahn and Armbruster, whose recommendations and judgments of feasibility seem scarcely different from what they might have been in 1961 or 1965 (when they would have looked more adequate to many readers, including this one). One wonders what, if anything, these writers have learned from the painful and frustrating experience of those years: specifically, about the characteristics of the U.S. as counterinsurgent (and more generally, as interventionist and as *learner*), the GNV as ally, the VC as opponent, and Vietnam as environment for a revolutionary conflict.

These are specifics that must underly judgments that would tell the President (more cogently than "we may yet win") *how he should bet:* the *odds*—on adoption of policies, on implementation, on VC (and GVN) counters, on effectiveness—the *costs*, the *time* required, the *risks*. Such matters are gravely neglected in the discussions of Kahn and Armbruster.

It is only Stillman and Pfaff, in fact, who take seriously the word "we" in addressing the title question. This focus—which seems to me crucial—plus their reading of experience both prior to 1961 and after, leads Stillman to conclude (p. 164) that our present choice is between "a painful ending of the war now and a disaster later." Pfaff's subsequent, brief chapter on possible settlements is the best discussion of realistic negotiating aims I have seen, highly to be recommended to the incoming President and his negotiating staff.

Their emphasis—peculiarly identified with writings by Stillman and Pfaff—on factors of culture, politics and history, and particularly on American limitations ("Of all nations," Stillman remarks on p. 350, later excepting Albania, "I think that the United States may be one of the least capable of intervening in such a situation successfully") reflects a point of view that may not be conclusive but is highly relevant, neglected, and valuable. If it had been more adequately represented and understood much earlier in our involvement in Southeast Asia, we might have avoided an American tragedy.—DANIEL ELLSBERG, *The Rand Corporation.*

Anglo-Soviet Relations, 1917–1921. Britain and the Russian Civil War, November 1918—February 1920. By RICHARD H. ULLMAN. (Princeton: Princeton University Press, 1968. Pp. 395. $10.00.)

In this volume, the second in a series of three, the author deals with a very important case study in the politics and strategy of intervention—the British involvement in the Russian "civil war." Although this subject has been discussed in works by George F. Kennan and John M. Thompson and others, the virtue of Professor Ullman's book is that it offers a highly detailed examination of British cabinet-level decision-making in an area in which it failed. The author has made a very real contribution in utilizing and in quoting extensively from a variety of unpublished official documents and private papers. These include cabinet minutes, Foreign Office and War Office records, letters, telegrams, and memoranda as well as the diaries of Field-Marshal Lord Ironside (Commander of Allied Forces at Archangel) and of Field-Marshal Sir Henry Wilson (Chief of the Imperial General Staff). The quantity of previously unpublished material cited alone makes this volume an important study.

There emerges a record of piecemeal decisions, fragmented policy-making and improvisation. There was much disagreement among British policy-makers and advisers and as a result conflicting

policies were adopted—which prompted Sir Samuel Hoare to observe in the House of Commons: "We have gone on the principle of backing one horse one day and another horse another day. We have sent people to deal with the Bolshevists and other people to deal with the anti-Bolshevists." (p. 117). To judge by the record, every effort to arrive at a statement of policy merely glossed over problems and issues and gave the appearance of being a decision when in fact there was none. The purpose of the intervention had changed over time; originally it had been prompted by military considerations related to the war effort against the Central Powers. As the struggle between Bolsheviks and Whites got underway, various cabinet members and officials adopted different arguments to justify greater (or lesser) involvement. Since the largest force (two divisions) was sent to Azerbaidjan and Georgia, due largely to the presence of Turkish forces in the Caucasus, some held that area and the Caspian Sea to be vital to the defense of India and Persia. Others emphasized the moral responsibility not to desert those Russians who had been "loyal" to Britain in 1917–1918 and who were now opposing Lenin's regime. The fear of a possible Russo-German alliance prompted some, especially Churchill, to favor intervention on the unproved assumption that a "grateful" restored Russia would support Britain after the defeat of the Bolsheviks. Thus Churchill and Henry Wilson were unsympathetic to the claims of the non-Russian nationalities to self-determination while Lloyd George at times expressed support for their cause (although his military aid to Kolchak and Denikin belied this sympathy). Wilson opposed intervention on logistical grounds and contended that Britain's troops were needed elsewhere.

The British intervention was highly limited and conditional (although the conditions varied in the four areas—North Russia, the Baltic, South Russia and the Caucasus, and Siberia), but London could not obtain the support of its allies. The British, Americans, French, and Japanese were often at cross-purposes; thus the Japanese were little interested in the outcome of the Russian civil war and may not have objected to a weak Bolshevik regime in Moscow if it increased their own opportunities for gain; the French spoke much of anti-Bolshevism, but their intervention in the Ukrainian coastal region was brief, and when Lloyd George sought to promote a negotiated settlement (at Prinkipo) he was undercut by the French—and also by the Conservatives in the House of Commons whose support he needed. The British generally favored the independence of the Baltic States while the French did not; the latter favored a restored Russian Empire that would be "grateful" to France and opposed to Germany.

More fundamental in explaining British failure were domestic considerations: industrial unrest, the need for troops in Ireland, the opposition of Labour and of various liberal elements, the impossibility of using conscripted troops, and the high financial cost of intervention. As a result it is not surprising that Lloyd George pursued a policy of drift and deviousness and grasped at various straws.

Although Professor Ullman's treatment conveys the indecision, bickering, and disagreement among British officialdom, it can be said that Whitehall had the following options: (a) withdrawal (in fact, the decision to withdraw forces from North Russia where nearly all British casualties occurred, was taken in March 1919 but was not effected until the autumn); (b) offering military and financial aid to the anti-Bolsheviks (this was done on a selective basis) to "give them a chance" and to preserve an alternative to Lenin's regime; (c) full-scale concerted intervention as advocated by Churchill (who was isolated in the Cabinet) but impossible under the circumstances; (d) negotiation (as, unsuccessfully, at Prinkipo and following the Bullitt Mission or, successfully, in the prisoner exchange agreement concluded at Copenhagen); (e) trade arranged indirectly in an effort to influence Lenin's regime without granting *de facto* recognition. In the end, Lloyd George was to rely on (a) and (e) after (b) and (d) had failed and (c) had been rejected; (e) was to facilitate (d).

The effectiveness of any intervention tends to depend upon the use made of the indigenous population. The failure to raise a large Russian army in support of the government formed at Archangel did not augur well for the intervention. The inability to establish a unified command meant that military operations against Lenin's regime were never coordinated. The existence of four or more Russian anti-Communist regimes as well as those of the non-Russian peoples who had declared their independence (Finland, Ukraine, Poland, Armenia, Azerbaidjan, Georgia, Estonia, Latvia, and Lithuania) made if difficult to form a common front. Kolchak and Denikin, whom the British decided to support, did not make effective use of British aid and were old-style Russian imperialists of a kind not likely to inspire confidence. The author appropriately raises the question of the kind of Russian regime that would have been established had the Bolsheviks been defeated and asks whether the Allies would have been able to come to terms with it.

In Professor Ullman's view, Lloyd George and Woodrow Wilson were wrong in believing that if the Whites deserved to win they would attract mass support. He contends that the two leaders employed a "fallacious analogy—that a civil war was like an especially violent election from which

the party that can attract the most popular support will emerge victorious" (p. 351). Instead, he argues that a civil war is often determined by "the ability to combine rewards and punishments in such a way as to mold together an efficient, disciplined fighting force."

Intervention is a painfully difficult undertaking in the best of circumstances and requires clearly defined objectives that are compatible with capabilities. Professor Ullman's valuable case study and his analytical treatment in chapter nine provide a useful guide to the pitfalls and the costly nature of this kind of enterprise.—JOHN S. RESHETAR, JR., *University of Washington.*

Legal Order in a Violent World. By RICHARD A. FALK. (Princeton: Princeton University Press, 1968. Pp. 610. $15.00.)

Of Richard A. Falk, it can be said at the outset that his is one of the most original and subtle minds addressing the theory and practice of contemporary international law. For nearly a decade, he has been concerned with the appraisal of the persistent resort by states to force as an instrument of national policy in the face of the real threat of nuclear disaster. No captive of the empyrean approach, he seems at times to despair of the prospects for the emergence of an effective legal order in a world of rampant violence short of the coming of World War III which might serve as the catalyst for the general adoption of those international institutional changes which would lead to a centralized, hence supranational, control of the use of force. Yet, he can "acknowledge that there has been a gradual strengthening of moral consciousness and human compassion so as to make the reorganization of the political basis of human existence on a global scale a meaningful goal." (pp. x-xi) Disarmament at the national level and its concomitant, the guarantee of effective maintenance of international security by supranational institutions are his keys to a warless world.

In *Legal Order in a Violent World,* Professor Falk examines with sophisticated insight the international uses of violence today, the problems of legal regulation of human conflict in a decentralized social system, and suggests ways of achieving his dual objectives. The book consists of fifteen essays, written between 1960 and 1967. Ten of the essays have been previously published in scholarly journals or in other collections, and three received limited circulation in a series sponsored by the Center of International Studies at Princeton University. Although the essays have been revised with a view to their incorporation into the present volume, the author states that he has endeavored to retain the policy context in which each was originally presented, so the date of principal composition of each is indicated.

The book is divided into four sections. Part I, "A Statement of Outlook," contains two essays on the control of international violence by international law. As the author points out in a later essay, law should be seen "not as a fixed matrix, but as a dynamic dimension of social change," (p. 345), and in his opinion, international law can be used in this way to bring about the new international order which he envisages. The third essay is a critique of the McDougal-Feliciano position on the subject as presented in their study, *Law and Minimum World Public Order: The Legal Regulation of International Coercion* (New Haven, Yale University Press, 1961). Here, Professor Falk takes exception to their reliance upon "reasonableness" as a control in national decision-making and as "the basis of a progressive system of world order." (p. 96) In Part II, entitled "Civil Strife, Intervention, and Minor Coercion," the author examines the emergence of civil war as a substitute for or alternative to international war and the development of third party intervention into an "interventionary pattern of diplomacy." (p. 103) He analyzes United States policy and practice regarding intervention with specific reference to the Bay of Pigs and Stanleyville incidents. Two essays are devoted to the exposition and defense of his view that United States involvement in the Vietnam war is not legally justifiable. As the alternative to continuing third party intervention by states, he urges the expansion of United Nations Intervention in situations of internal conflict as illustrated, in his opinion, by the Congo operation in 1960. The final essay in this group treats the use of propaganda as a form of intervention and urges caution in attempts to control it by international fiat because propaganda has a "legislative role in international society generating pressure for social change." (p. 364)

Part III on "Nuclear Weapons" explores the function of nuclear weapons technology in international politics. In the first essay in this group, the author analyzes the *Shimoda Case,* the unsuccessful effort by five Japanese claimants to recover damages from the Japanese government for injuries allegedly sustained as a result of the atomic bombings of Hiroshima and Nagasaki. Although the Tokyo District Court found no grounds for recovery of damages, it did hold that the use of the atom bombs by the United States was violative of established international law. The following two essays appraise practical policy limitations on the use of such weapons and the desirability of states' subscribing to a "no-first-use" rule as a control on such weapons. Professor Falk sees the *Shimoda Case,* the 1961 General Assembly Resolution 1653 (XVI) condemning resort to nuclear weapons as violative of the Charter, and the development of

national policy controls on their use as pointing to a growing international consensus looking to the outlawry of these weapons. The last section of the book is concerned with the problem of controlling international violence while the international community moves in the direction of total disarmament. The appendices contain the text of the 1966 memorandum by the Legal Adviser of the Department of State on "The Legality of United States Participation in the Defense of Viet-Nam," the 1963 outline of Basic Provisions of a Treaty on General and Complete Disarmament in a Peaceful World, and the text of General Assembly Resolution 1653 (XVI).

Inevitably, a certain unevenness appears in the book, given the period of time over which the several essays were written. As the author himself points out, his discussion of disarmament predicated on the bipolarism of 1962 is not entirely *apropos* of 1968's "polycentric interpretation of international conflict." (p. 440) Furthermore, although the various essays are broadly addressed to the same general subject matter, there is a disjointed quality about the book which suggests that it should have "collected essays" as its subtitle and that the reader should be prepared to peruse the contents selectively rather than *seriatim*. Some flaws in editing appear, *inter alia* the addition of a paragraph to the Legal Adiver's memorandum which apparently belongs to the draft disarmament treaty text (p. 558), the date of this memorandum, and the title of the Bull *Inter Caetera*.

These considerations aside, Professor Falk's critical examination of the vital issues presented in the several essays makes *Legal Order in a Violent World* a valuable contribution to the current literature on problems of peacemaking. Students of international law and international politics will be glad to have these provocative essays available in a readily accessible form.—ALONA E. EVANS, *Wellesley College*.

Megamurder. By E. L. M. BURNS. (New York: Pantheon Books, 1967. Pp. 297. $6.95.)

Deadly Logic: The Theory of Nuclear Deterrence, By PHILIP GREEN. (Columbus, Ohio: The Ohio State University Press, 1966. Pp. 361. $6.00.)

Megamurder and *Deadly Logic* have more in common than their alarmist and depressing titles. Their authors each argue that gigantic errors in ways of thinking about Western military policy need urgently to be corrected. In neither book do the civilian strategists who have dominated the public debate receive high marks. Both are ambiguous as to relationships between defects of theory and military policies which the authors evidently dislike. One is explicit in urging us to get on with the business of arms control; this is implicit in the argument of the other.

There are contrasts, too. General E. L. M. Burns is a senior Canadian military man with wide experience in two world wars, in international peace-keeping, and in advising his government on disarmament matters. Professor Philip Green is a young American political scientist who stands outside of the "establishment" in the national security affairs community and does not like what he thinks goes on inside. Burns' book is aimed at his fellow military men and especially those who do not share the general's zeal for getting war back onto the battlefield and for putting protection of the civilian population far above "victory" as a military objective. Green's book is aimed at fellow social scientists and warns against "the new intellectual imperialism" of the deterrence theorists. The general writes calmly—he does not use the word "megamurder" in his text until the last sentence of the last page—and traces the evolution of bombing policies of which he disapproves from Douhet to the present. The professor writes polemically and writes much more about what others have written than about what military men and politicians have done.

The military do not, in Burns' view, share the whole responsibility for strategies of victory by urban holocaust, as his treatment of the savage and senseless bombing of Dresden shows. That event he sees as only one more example of the propensity of protracted conflict to escalate. One need not accept the utility of analogies drawn from protracted general war for understanding the arms race of the 1960s to agree that a plateau in arms levels provides the occasion for more intensive efforts to avoid future holocaust by negotiated arms controls.

Philip Green is a self-appointed David out to slay the Goliath of deterrence theory. His criticisms of the Brodies, Kahns, Morgensterns, Schellings, Snyders and Wohlstetters of this world are many and various. Method, motive, basic data and policy relevance are in turn questioned. Professor Green has read widely and carefully, and his criticisms are often provocative. To this reviewer, however, they seem less effective in concert than they might have singly. The central criticism seems to be that the deterrence theorists exhibit a "mutually reinforcing bias and trained incapacity." Evidently, Green would have a smaller share of research on arms and arms control carried on inside the ivory towers of the Rand Corporation, the Hudson Institute and the contract-fed university research institutes.

The deterrence theorists' models, Green complains, are too simple and too static and their data too unreliable. The canons of scientific rigor do not, however, oblige a scholar to stand mute until his model is perfect and his data complete. If that were the requirement, even Mr. Green's model of

the military-intellectual complex might fail to meet the test.—WILLIAM T. R. FOX, *Columbia University.*

Western European Perspectives on International Affairs: Public Opinion Studies and Evaluations. ED. BY RICHARD L. MERRITT AND DONALD J. PUCHALA. (New York: Frederick A. Praeger, 1968. Pp. 552. $17.50.)

This book is rather like a Volkswagen microbus: it has esthetic difficulties but it can (and should) be extremely useful. One of its most valuable aspects is that it serves as introduction and guide to a large body of data which is available for further research—the USIA surveys which have been conducted approximately once a year since 1952 in four or more West European countries. Now comprising more than sixty national samplings, this material, as the editors point out, has a large potential which is still untapped. Some of its features are truly exceptional: not only were the same questions asked in all of the European countries surveyed in a given year, but certain items were used repeatedly over a period of many years, giving a basis for comparisons over time, as well as across cultures. One can undertake cross-national analyses of public response to major international events such as the Hungarian revolt of 1956—and eventually, one hopes, the war in Viet Nam or the Czech revolt of 1968: One might investigate the impact of such events on the images of given foreign powers, or on domestic political party preferences. (It seems most unfortunate that, since 1965, the USIA survey program has been curtailed, and the availability of survey results made subject to unpredictable delays—apparently because of the misuse of USIA reports by partisan leaders to demonstrate the success or failure of given administrations. One might think it would be possible to release at least the raw data from recent surveys, if not the official reports based on them; as of the present writing, however, no surveys made since 1964 had been declassified. American comprehension of European public opinion has probably suffered as a result.)

Besides introducing this material, the authors demonstrate a number of ways in which it can be utilized effectively. It is here that the microbus image may strike one: a surprising amount of worthwhile material is included in the book, without much regard to the attainment of any graceful overall form. Utilization of the material is incomplete; indeed, it scarcely could be complete, even with several volumes as large as the present one. What we have is a sampling of analyses which happen to have been done, coordinated by no readily apparent program. This is understandable in what is frankly an exploratory work.

That which is included has considerable substantive interest: for example, an article by Ralph K. White, on the meaning of the terms "socialist" and "capitalist" to European publics (as contrasted with their connotations in America). Or a series of articles by Leo P. Crespi and by Gabriel A. Almond analyzing results of the U.S.-Soviet race for international prestige based on scientific accomplishments.

Puchala's chapter on factor analysis in international survey research is interesting both substantively and methodologically; the technique should be particularly useful in cross-cultural comparison of responses. Puchala argues convincingly that this method can be valuable in analyzing *why* mass publics take given positions on given issues, by delineating the broader images which appear to influence responses to a particular question. There are limits, however, to what we can conclude from an examination of factor loadings, and Puchala's enthusiasm sometimes carries him beyond them. For example, he implies that the West German image of the U.S. in 1954 was favorable, in part, *because* it was associated with the generally favorable image which Germans had of foreign countries. The factor analysis merely shows that there was an association among images of foreign things, without specifying any causal relationship. As we see elsewhere in the book, German feelings toward the U.S. were much more favorable than they were toward any other country: I suspect that, if anything, the process worked in the opposite direction from that which he indicates. Overall, however, this section displays a number of keen and stimulating insights.

Another especially interesting section is Merritt's chapter on "Visual Representation of Mutual Friendliness." Considerable creativity is apparent in both the technique of graphic presentation and in the author's fruitful use of a distinction between salience and friendliness in mass images of other nationalities. The lack of a theoretical framework is particularly regrettable here, however. Although much of the substantive interest in Merritt's discussion lies in conclusions which one is tempted to draw about the relationships between public attitudes and public policy, any such theory remains implicit in the treatment. A considerable degree of correspondence between the two seems to be present, but we are not even sure of what Merritt regards as input and what as output. A future edition of this work might enhance its effectiveness through an attempt to tie these intriguing threads together in a more explicit manner. Needless to say, this is setting Merritt a large task in addition to the one which he has already handled ably.

This volume is not only a set of readings in Eu-

ropean public opinion, but a reference work as well—containing more than 350 pages of percentaged marginal frequency distributions. The topics include: perceptions of the Cold War and Coexistence, European integration, arms control and disarmament, the Space Race, German reunification, and many more. Leafing through these pages, a number of enduring patterns emerge. For example, we note that French opinion has consistently differed from that of her neighbors, in that a plurality (and often a majority) of the French public has favored having France take a middle position between the U.S. and U.S.S.R., siding with neither. This has held true since 1952, long before De Gaulle's second coming, and France's withdrawal from active participation in NATO.

Because the USIA data recommend themselves for secondary analysis in graduate seminars, this can also serve as a textbook: written on a level which presupposes no previous training in survey analysis, it provides a guide to the items which are available in given surveys, examples of how the material may be exploited and some discussion of specific techniques. (The data are available from the Roper Center, Williamstown, Massachusetts or the Inter-University Consortium for Political Research, Ann Arbor, Michigan. The sampling technique of the respective European survey organizations seems to have improved over time, with the data gathered since 1960 evidently more reliable than earlier material.)

What the book lacks in neatness of organization is more than compensated for by its abundant inventiveness—in material used, techniques applied and the interpretations offered. The editors have also made an important contribution in facilitating the further use of resources which should be of immediate value to political science research and teaching—and may well constitute a significant asset to future historians of West European politics.—RONALD INGLEHART, *The University of Michigan.*

Law in the World Community. BY C. WILFRED JENKS. (New York: David McKay Company, 1967. Pp. 164. $7.95.)

This is a collection of lectures, delivered between 1964 and 1966, to law faculties and international lawyers around the world. The author touched bases in almost all blocs and continents.

One should not underestimate Wilfred Jenks. He is a highly skilled legal craftsman and has performed signal service as a pioneer international civil servant. He has been with the International Labor Organization since 1931—chief of its legal staff since 1940, and for some years now its Deputy-Director General. When he writes in depth on practical legal problems, especially those concerning the law of international organizations, no one speaks out of greater experience or with more authority. Among his more recent works, *International Immunities* (1961), and *The Proper Law of International Organizations* (1962) may be cited as examples of that kind of work. The contribution presently reviewed is of a different kind. Like his earlier books entitled *Law, Freedom and Welfare* (1963) and *The Common Law of Mankind* (1958), this collection deals in broad generalities and is essentially an exhortation to his audiences to construct a logical and consistent rule of law in world affairs.

It is hard to believe that any useful purpose is served by this kind of book. Certainly fallacious is his initial assumption that all international lawyers are dedicated to the same objective—"a common interest in peace, freedom and welfare," he called it in Warsaw; in Tokyo he assumed a central purpose of international law and international lawyers to secure the freedom of man. "Do we believe in freedom? Do we believe in law? . . . If we believe in these things, and have the courage and resolve to translate our beliefs into effective action, all our problems are soluble." (p. 102)

The objections to these assumptions are obvious. Not only is he glossing over the existence of widely differing national objectives, but even within each nation, its international lawyers differ in their objectives as they differ in roles. The policy advisor, the governmental apologist, the lawyer for an interest group, the truly independent scholar, the advisor to an international organization—all of these will have their own individual objectives. Law is instrumental, more often than it is a goal in itself. Indeed conflict, more than harmony, characterizes the objectives of international lawyers.

This reviewer agrees with Jenks' view that "the place of law in the world community is not a technical problem of jurisprudence but a broad issue of public policy." More study of the function and uses of law in the process of making foreign policy, and of its relationship to other factors, psychological, economic, and social, seems needed. But to start such study with generalities and exhortation is to put the cart before the horse. It must begin with detailed analysis of official behavior, based on data compiled from specific cases, and with a special focus on the points at which legal considerations have potential policy relevance. Fortunately, increasing effort is now being focused on studies of this kind. The relationship between law and policy is certainly more complex than Jenks suggests. One conclusion to which studies of the legal factor in policy-making seem to lead is that, even in the United States, the possible relevance of international legal consider-

ations (especially in regard to long-range consequences) has not always been presented in the course of policy-making. In at least one important area—the regulation of the use of force—we have witnessed a steady erosion of the post-war legal consensus, which belies Jenks' optimism, and which is probably traceable in part to a lack of effective presentation in the decision-making process of the complexities of this law-policy relationship.—EDWIN C. HOYT, *University of New Mexico.*

The Relevance of International Adjudication. BY MILTON KATZ. (Cambridge: Harvard University Press, 1968. pp. 161. $4.95.)

In this little book, Mr. Katz deals with two of the more elemental functions of law, which are to curb violence and to resolve controversies. It is clearly argued that the limits to effective legal action in disputes between nations are to be found in the nature of the law itself. The question of what makes a controversy justifiable is not only a matter of the competence of the court to adjudicate, but is dependent also upon what there is in the dispute that makes it amenable, or not amenable, to adjudication by any court. The analysis that follows is, therefore, governed by certain premises with regard to the meaning and compass of the law.

The author significantly points out that international law entered the twentieth century as a comparatively rudimentary system both with regard to its doctrine and its institutions, and that a mutation was necessary to give a new direction both to law and diplomacy. The result has been a multiplicity of international organizations, which are regarded by the author as "specimens" of the mutations. We are reminded, furthermore, that a very large proportion of these organizations are of relatively recent origin. More than four fifths of the basic constitutional documents for international organization in force in 1960, were barely twenty years old.

It is contended that these organizations, dealing as they do with all manner of problems, have contributed in some degree to the development of international law. The main problem with which the author is concerned, however, is the significance of the mutation with respect to the central purpose of international law—the peaceful and constructive resolution of international conflict.

With respect to settlement of Cold War disputes, the author finds that whatever the attempts to enlist the norms of international law in support of respective claims, adjudication has not been resorted to in an effort to settle the conflict nor any other means of peaceful settlement, such as arbitration or mediation. "In sum, in international Cold War disputes, the conditions for adjudication or quasi-adjudication at best lie at the far edge of the spectrum. Typically, they lie beyond the limits." (p. 39). The questions posed at this point are: to what extent the apparent irrelevance of adjudication and arbitration of Cold War issues is due to an absence of tribunals to determine and apply law; or if these exist, to a lack of adequate means by which such institutions may assert authority; or, if the means exist, from the failure of the tribunal to make use of the available means. Moreover, to what extent is the apparent irrelevance here due to some inadequacy in the content of international law?

In chapter four of this study we find a most interesting discussion of the now famous South West Africa Cases. It is the best short account of this *cause célèbre* that this reviewer has yet seen. The apparent contradiction between the Court's Judgment of December 21, 1962 and the Judgment of July 18, 1966 is here effectively handled. Special attention is given to the third of four preliminary objections advanced by South Africa, which insisted that the dispute was not one within the purview of Article 7 of the Mandate, or one in which the Court had competence on the merits. The International Court of Justice, however, found in 1962 that each applicant in the case had "a legal right or interest in the observance by the Mandatory of its obligation . . . toward inhabitants of the Mandated Territory." (pp. 99-100). But in the opinion supporting the 1966 judgment, the Court appeared to be drawing a distinction between *locus standi,* as such, and standing to receive judgment on the merits.

Judge Jessup, in a separate opinion, pointed out the "utter futility" of such a procedure in asking the question: "Why should any State institute any proceeding if it lacked standing to have judgment rendered in its favor, if it succeeded in establishing its legal or factual contentions on the merits?" (p. 101). In his analysis of the 1966 judgment, the author calls attention to the limits of adjudication and quasi-adjudication in the settlement of international disputes. He regards an understanding of the effective reach and the limits of adjudication as imperative to the best results.

The Court, in the South West Africa Cases, for fear of involvement in a morass of political questions, had abandoned the field to political organs. The result was first the General Assembly Resolution No. 2145 (XXI) of October 28, 1966, which terminated the Mandate of South Africa and placed the territory of South West Africa under the direct responsibility of the United Nations. This was further implemented by Assembly Resolution No. 2248 (S-V) of May 19, 1967.

The question of legal authority on the part of the Assembly in this matter is carefully considered by the author. In his analysis of the action of the Assembly regarding the South West African Ques-

tion, he finds that "a foundation in law for the authority of the General Assembly to cancel the Mandate has not yet been established." (pp. 123–124). The author's conclusion is that the withdrawal by the International Court of Justice in this matter brought about a great deal of confusion, and that the General Assembly has done little to mitigate the confusion. (p. 127).

At the end of this little treatise, Mr. Katz turns to a speculative appraisal of what consequences might have followed from the South West Africa Judgment had the one-vote margin been on the other side. In dealing with political action, the author contends that it need not be of the kind that has marked the deliberations of the General Assembly and the Security Council in recent years. He ends his argument on the note that "There could be political action infused with . . . a sense of law." (p. 155).—OSCAR SVARLIEN, *The University of Florida.*

Peaceful Conflict. By EDWARD BERNARD GLICK. (Harrisburg, Pennsylvania: Stackpole Books, 1967. Pp. 234. $6.50.)

In this rather optimistic assessment of "civic action" functions of armies in developing areas, Dr. Glick explores a topic that has considerable contemporary significance for students of modernization, arms control, and counter-insurgency. He performs a valuable service in documenting uses of the military other than conducting war or engaging in recurrent coups. Unfortunately his treatment is marred by a lack of theoretical sophistication and critical treatment of source material.

Dr. Glick's principal thesis is that armies can, and in some cases do, provide positive benefits in economic and social development. Assuming that conflict and hence armies are inevitable, he argues that policy-makers should give increased attention to civic action programs—such as road-construction and education—in an attempt to direct military resources along peaceful channels of social construction. More extensive use of the military for non-combative purposes, Dr. Glick maintains, can facilitate in turn progress toward effective arms control agreements.

While a number of scholars have demonstrated at least a marginal interest in the socio-economic role of the military in influencing political change, Dr. Glick progresses beyond most existing studies by casting the developmental functions of armies in a broad comparative mold. He draws on the frontier experience of the American army in the nineteenth century, civic action aspects of U.S. foreign military assistance programs, and the modernizing role of armies in Latin America, the Middle East, Africa, and Asia to demonstrate the diversity of non-military uses of the military. His approach is largely policy-oriented, utilizing historical and contemporary data to establish general criteria to evaluate the effectiveness of civic action efforts and suggest guidelines for their future implementation.

Having established a potentially challenging framework of investigation, Dr. Glick nevertheless disappoints the reader on several counts. One major shortcoming is that the very scope of the book, while useful in providing necessary comparative perspectives, invites superficiality. Since Dr. Glick devotes less than 150 pages of the volume's total length to his global survey of civic action programs in countries as divergent as Vietnam and Israel, his account can obviously only skim the surface of extremely complex phenomena. As a result much of the book is a mere listing of civic action operations with little analysis in depth.

Even more disturbing is Dr. Glick's tendency to rely heavily on official government spokesmen and publications for most of his primary data. While former War Minister General Alberto Ruiz Novoa can undoubtedly provide crucial insights into the impact of civic action activities in Colombia, for example, his conclusions (pp. 104–5) may not necessarily coincide with the actual results of their implementation. To the extent that Dr. Glick repeatedly accepts official sources at face value, his book suffers seriously from the absence of critical evaluation of specific civic action programs.

A third major weakness of *Peaceful Conflict* is the author's neglect of systematic theoretical generalizations. Dr. Glick makes a modest attempt at classification when he distinguishes between "systematic and continuous," "formal but idiosyncratic," "insurgency-generated," and "non-insurgency-oriented" civic action programs in his concluding chapter. But his "theory" of non-military uses of armies is limited to an implicit ideological assumption that the military should participate in economic and social development—in the absence of civilian capacity to provide all necessary conditions of development—because this is a useful means to help prevent Communist-led insurgency and maintain international stability. This omission of explicit theoretical considerations makes it difficult to relate the book's findings to existing scholarly literature.

The shortcomings of Dr. Glick's basically intuitive approach are clearly evidenced in his definition and use of the development concept. Viewing development primarily as increased economic productivity, an enhanced standard of living, and advances in levels of mass education, Dr. Glick appears to underestimate the importance of accompanying attitudes as a necessary additional dimension of modernization. While civic action programs can undoubtedly contribute to quantitative changes within the former categories, their ultimate effect on the course of development will de-

pend to a considerable extent on the degree of commitment by indigenous elites to genuine socio-economic progress. Civic action programs may serve simply as political palliatives to shore up existing regimes. Or they may function as a positive reenforcement of directed social change. Dr. Glick does not disregard such considerations altogether, but he would have considerably strengthened his presentation had he incorporated a more rigorous theoretical analysis of these and other possible patterns of development.

Despite its defects *Peaceful Conflict* is an interesting and provocative book. If it stimulates policy-makers to greater awareness of the importance of civic action programs and scholars to more thorough comparative investigations of the military's role in developing societies, it will fulfill an eminently worthy purpose.—M. Donald Hancock, *University of Texas at Austin.*

ANNOUNCEMENTS

The sixty-fifth Annual Meeting of the Association will be held September 2–6, 1969, at the Commodore Hotel, New York City.

ARTICLES ACCEPTED FOR FUTURE PUBLICATION*

June, 1969

F. M. Barnard, University of Saskatchewan, "Culture and Political Development: Herder's Insights"

Richard W. Boyd, Indiana University, "Presidential Elections: An Explanation of Voting Defection"

John F. Manley, University of Wisconsin, "Wilbur D. Mills: A Study in Congressional Influence"

Norman H. Nie, University of Chicago, G. Bingham Powell, University of California, Berkeley, and C. Kenneth Prewitt, University of Chicago, "Social Structure and Political Participation: Developmental Relationships, I"

Richard G. Niemi, University of Rochester, "Majority Decision-making with Imperfect Agreement on Norms"

Kenneth Prewitt, University of Chicago, and Heinz Eulau, Stanford University, "Political Matrix and Political Representation: Prolegomenon to a New Departure from an Old Problem"

Howard Rosenthal, Carnegie-Mellon University, "The Electoral Politics of Gaullists in the Fourth Republic: Ideology or Constituency Interest?"

Bruce M. Russett, Yale University, "Who Pays for Defense?"

Giovanni Sartori, University of Florence, "Politics, Ideology, and Belief Systems"

Robert A. Schoenberger, University of Michigan, "Campaign Strategy and Party Loyalty: The Electoral Relevance of Candidate Decision-making in the 1964 Congressional Elections"

Henri Theil, University of Chicago, "The Desired Political Entropy"

* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

September, 1969

Graham T. Allison, Harvard University, "Conceptual Models and the Cuban Missile Crisis"

Charles F. Cnudde and Donald J. McCrone, University of Wisconsin, "Party Competition and Welfare Policies in the United States"

Wayne A. Cornelius, Jr., Stanford University, "Urbanization as an Agent in Latin American Political Instability: The Case of Mexico"

Ralph M. Goldman, San Francisco State University, "A Transactional Theory of Political Integration and Arms Control"

Henry S. Kariel, University of Hawaii, "Expanding the Political Present"

Manfred Kochen, University of Michigan, and Karl W. Deutsch, Harvard University, "Toward a Rational Theory of Decentralization: Some Implications of a Mathematical Approach"

Richard M. Merlman, University of California, Los Angeles, "The Development of Political Ideology: A Framework for the Analysis of Political Socialization"

Norman H. Nie, University of Chicago, G. Bingham Powell, University of California, Berkeley, and C. Kenneth Prewitt, University of Chicago, "Social Structure and Political Participation: Developmental Relationships, II"

Nelson W. Polsby, University of California, Berkeley, Miriam Gallaher, Center for Advanced Study in the Behavioral Sciences, and Barry Spencer Rundquist, Stanford University, "The Growth of the Seniority System in the U.S. House of Representatives"

Ira Sharkansky, University of Wisconsin, and Richard I. Hofferbert, Cornell University, "Dimensions of State Politics, Economics, and Public Policy"

Jack L. Walker, University of Michigan, "The Adoption of Innovations by the American States"

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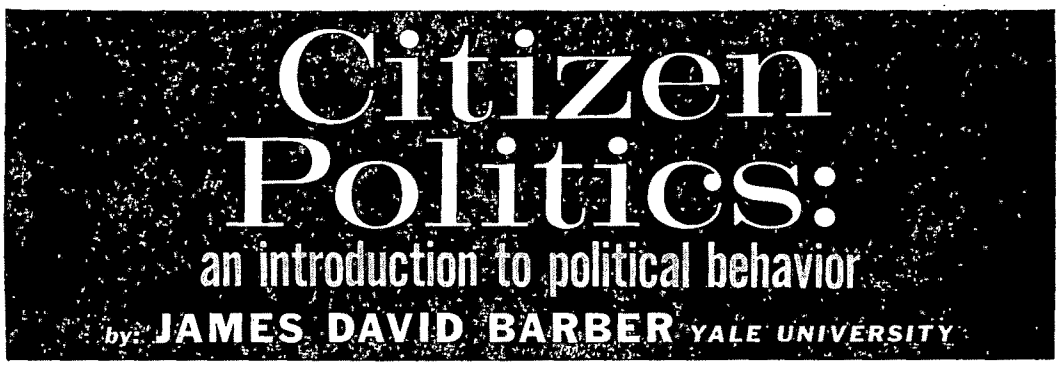
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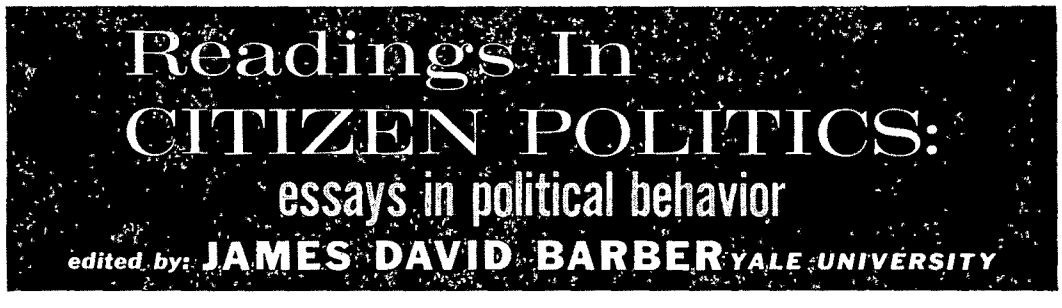
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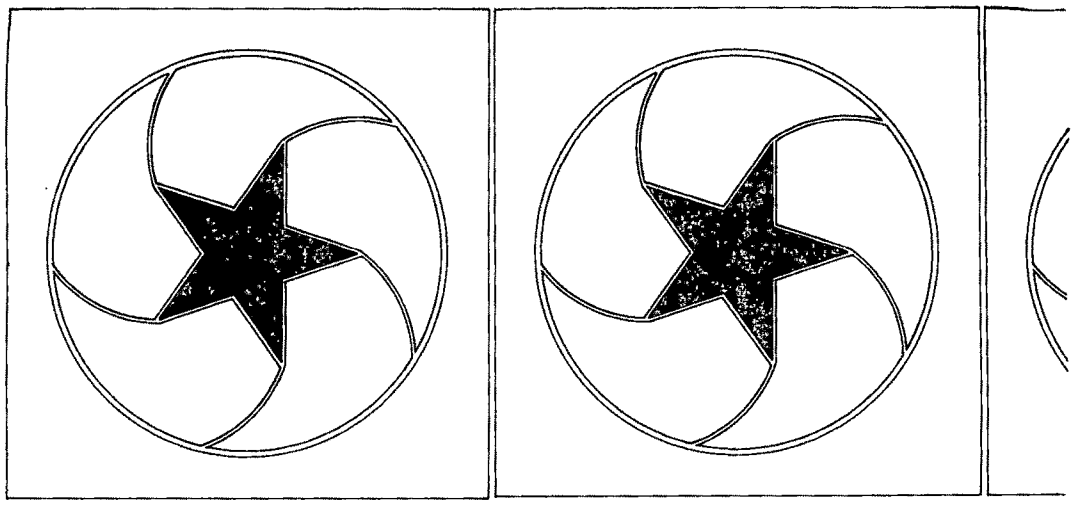
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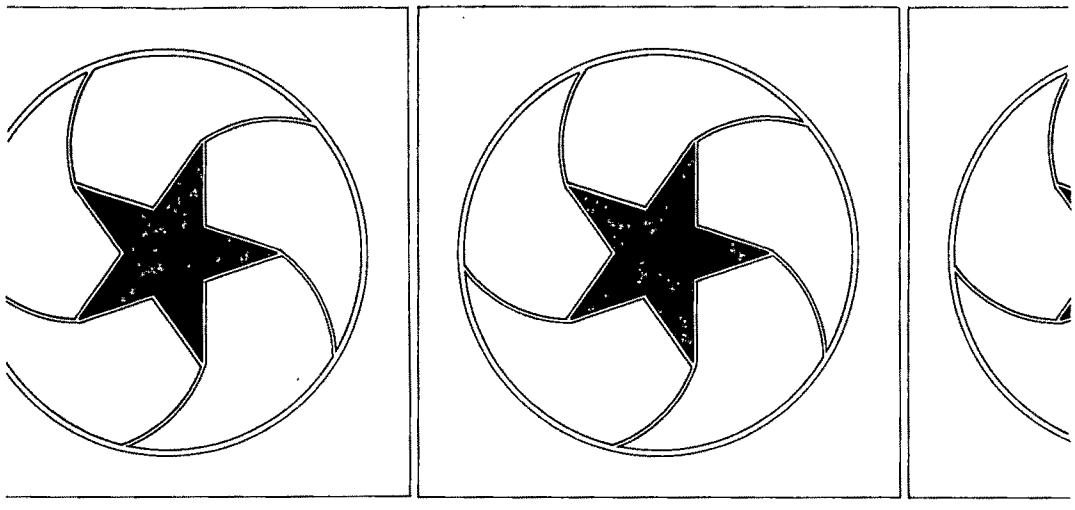
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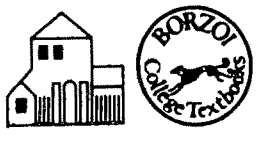
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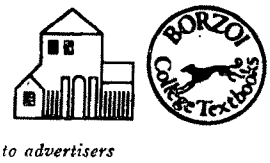
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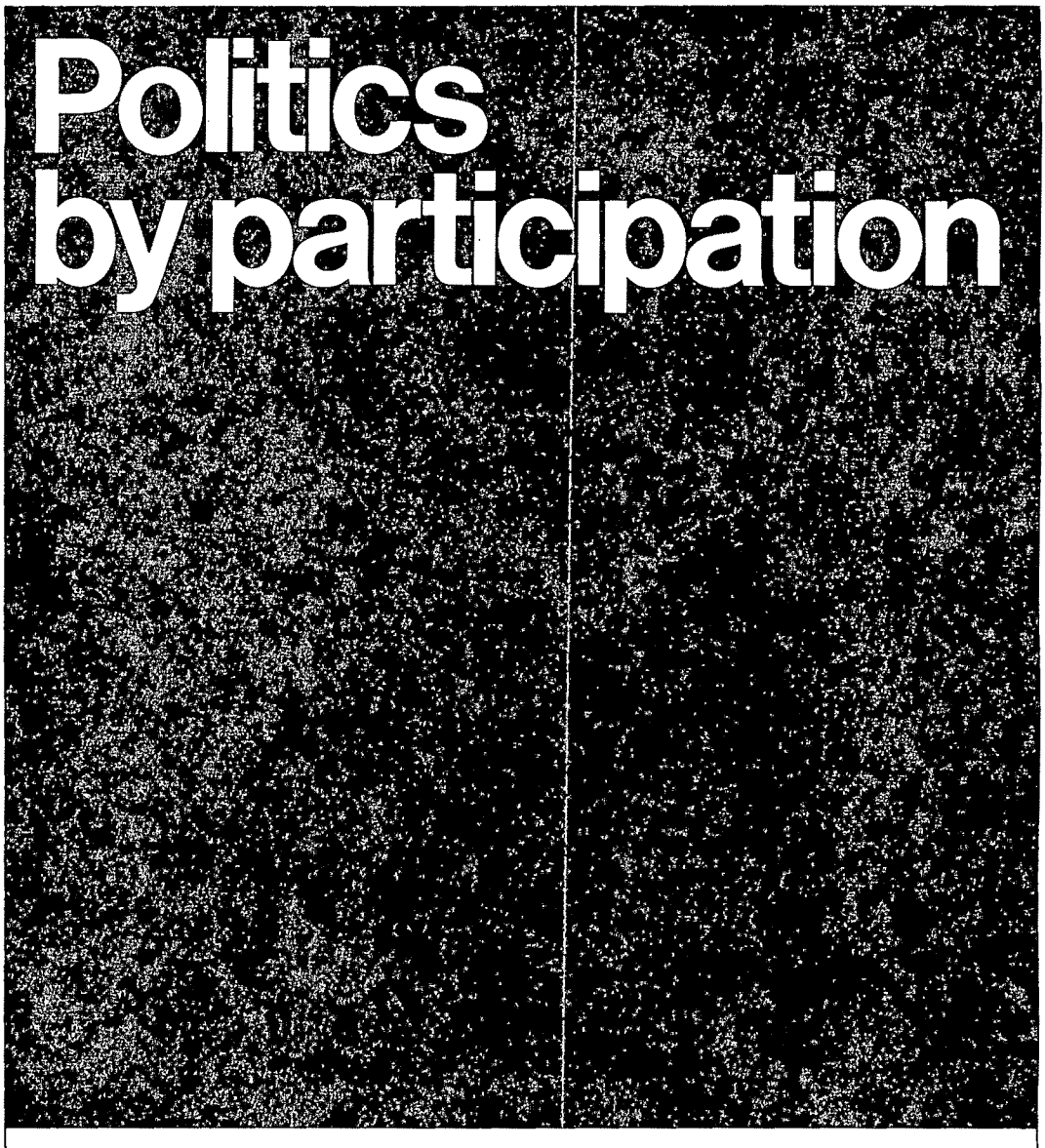
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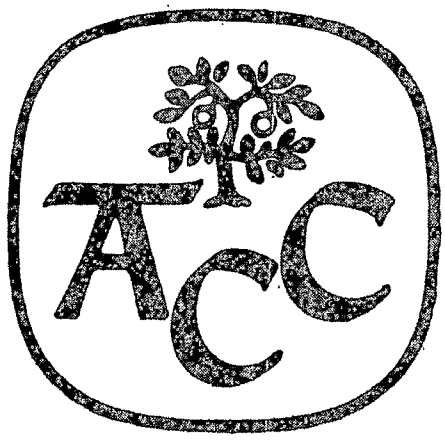
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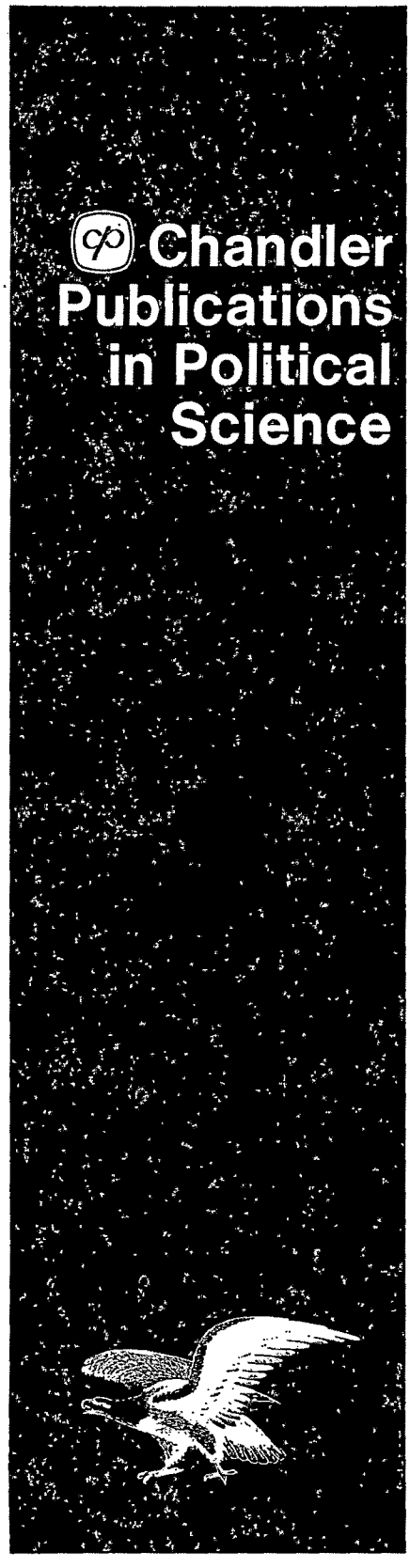
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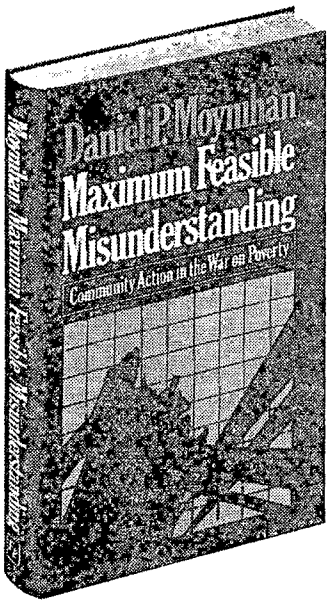
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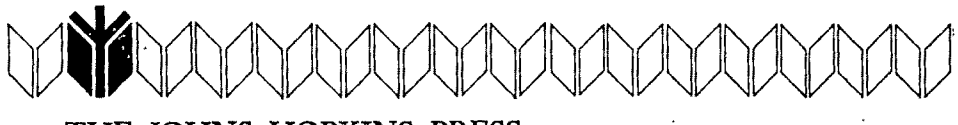
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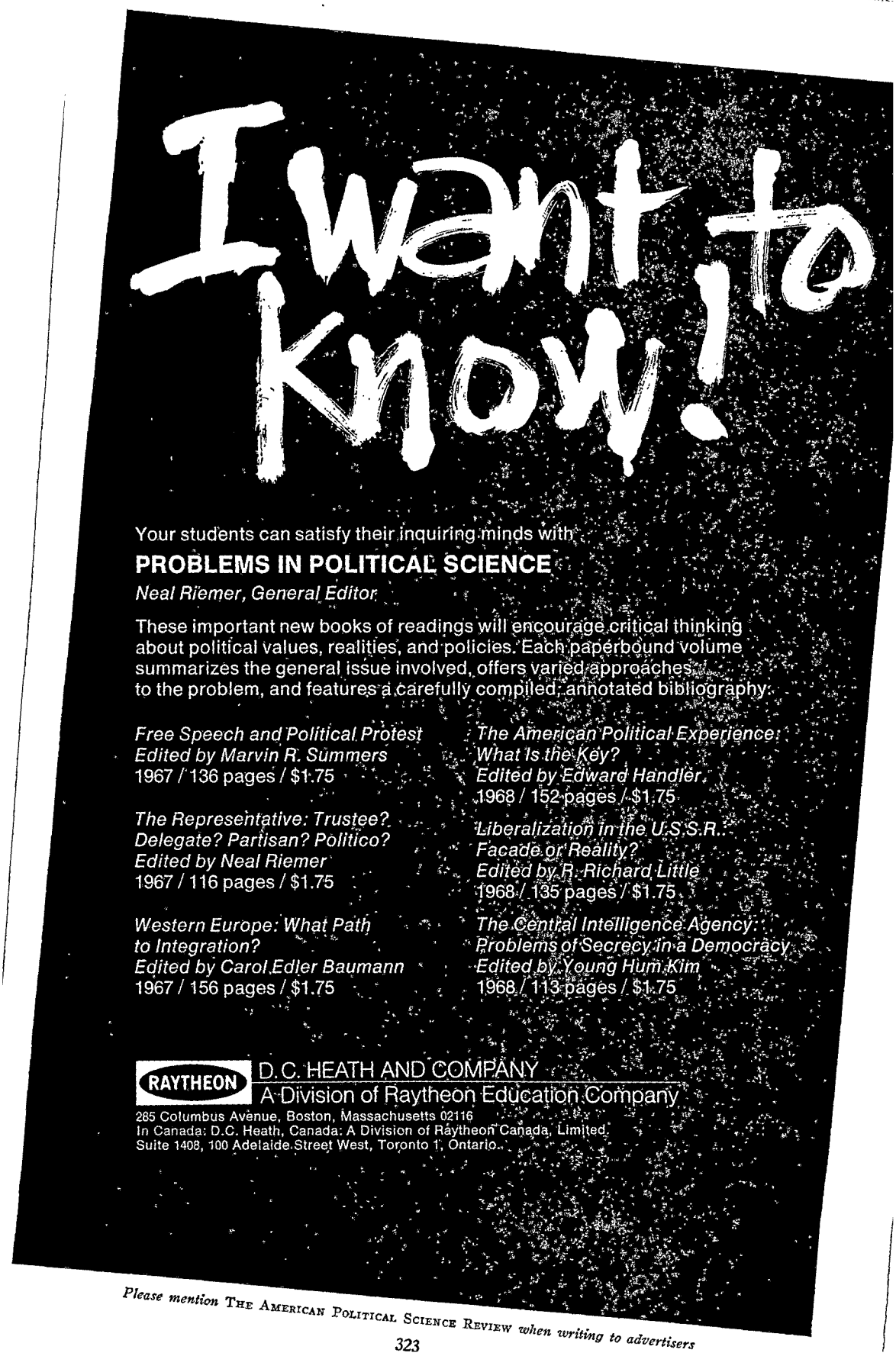
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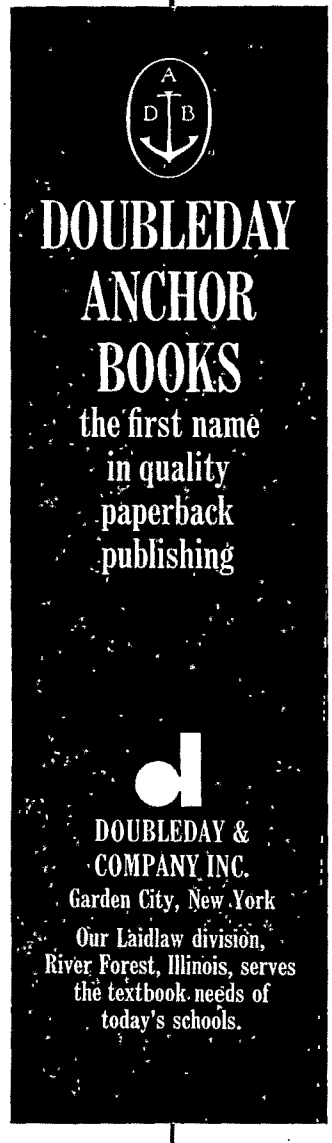
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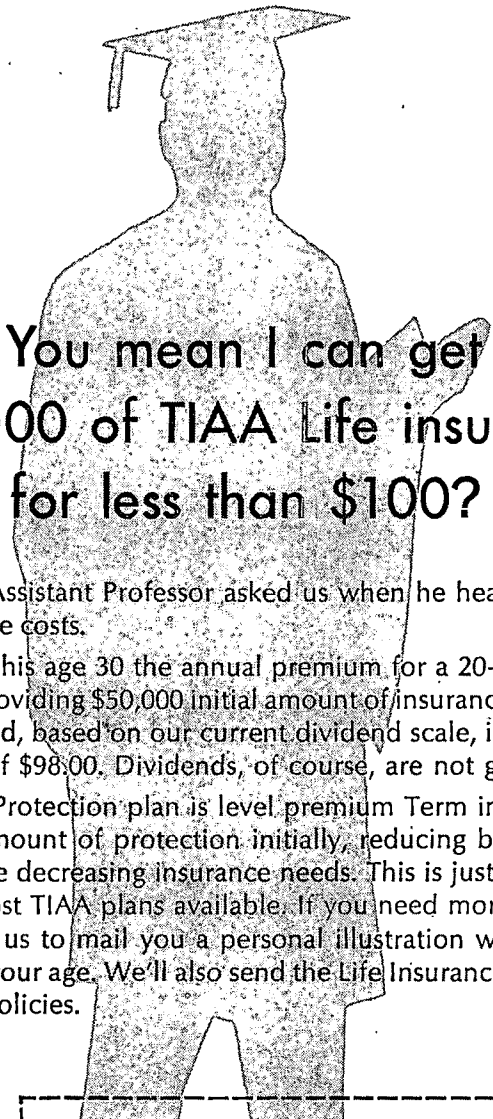
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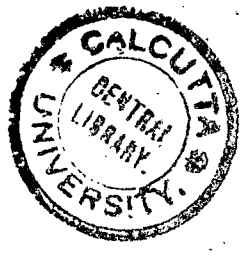
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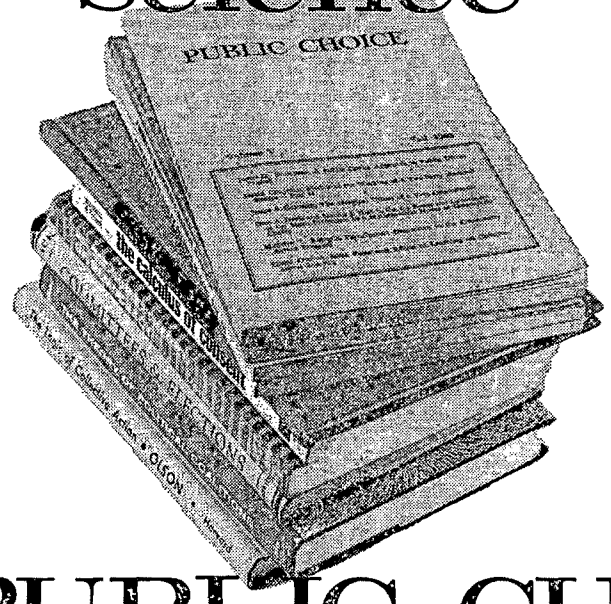
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The American Political Science Review

VOL. LXIII JUNE, 1969 NO. 2

SOCIAL STRUCTURE AND POLITICAL PARTICIPATION: DEVELOPMENTAL RELATIONSHIPS, PART I*

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University of Chicago

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KENNETH PREWITT
University of Chicago

Economic development has consequences for many aspects of social life. Some of these social consequences, in turn, have an impact on a nation's political life. Studies of social mobilization, for example, have demonstrated that economic development is associated with sharp increases in the general level of political participation.[1] These studies report strong relationships between aggregate socio-economic measures such as per capita income, median level of education, and percentage of the population in urban areas, on one hand, and aggregate measures of political participation, such as voting turnout, on the other. Simultaneously, scholars conducting surveys of individual political participation consistently have reported that an individual's social status, education, and organizational memberships strongly affect the likelihood of his engaging in various types of political activities.[2]

* Various institutions and individuals have been of considerable help to the authors in the preparation of this research. The department of Political Science, Washington University, provided summer support to two of the authors at an early stage of data preparation. Under NSF grants, the computation centers at both Washington University and Stanford University made machine time available. The facilities and the personnel of the Stanford Institute of Political Studies, particularly Mr. C. Hadlai Hull, the Institute's computer consultant, were invaluable aides to the completion of this research. The Center for the Comparative Study of Political Development at the University of Chicago provided funds to help us prepare the final draft for publication. For critical comments we are indebted to many readers, in particular to Sidney Verba, Duncan MacRae and Hayward R. Alker, as well as to Warren E. Miller, who, as a referee for the REVIEW, made a major contribution to the current version of the paper. Other, more specific, debts are acknowledged below. This is Part I of a two part article.

** The authors are listed alphabetically to indicate equal co-authorship.

[1] Karl W. Deutsch, "Social Mobilization and Political Development," this REVIEW, 55 (September, 1961), 493–515. Also, particularly, Daniel Lerner, *The Passing of Traditional Society* (New York: Free Press of Glencoe, 1958). For important analysis which in part contradicts the social mobilization hypothesis, see Walter Dean Burnham, "The Changing Shape of the American Political Universe," this REVIEW, 59 (March, 1965), 7–28. Burnham indexes political participation with voter turnout; we deliberately exclude voting from our scale of participation (see footnote 7). It is not clear, therefore, whether our general findings are in opposition to Burnham's. There is some reason for presuming that voting and other types of political participation are much more independent than a previous generation of scholarship has assumed.

[2] Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963); Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), pp. 282–301; V. O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, 1961); Lester

In spite of the consistency of both sets of findings across many studies and although the findings appear frequently in analysis of political stability, democracy, and even strategies of political growth,[3] we know little about the connections between social structure and political participation. With few exceptions the literature on individual participation is notable for low level generalizations (the better educated citizen talks about politics more regularly), and the absence of systematic and comprehensive theory. While the literature on the growth of national political participation has been more elaborate theoretically, the dependence on aggregate measures has made it difficult to determine empirically how these macro social changes structure individuals' life experiences in ways which alter their political behavior.

I. THE PROBLEM AND THE APPROACH

The task is to identify the significant social experiences which explain the growth of political participation in economically advanced nations. Having identified these social concomitants of economic development, it is also necessary to fill in the *causal* links between macro socio-economic processes and citizen political participation. These links, we hypothesize, are the resources, the attitudes, and the needs of individual citizens.

Multi-level analysis requires data about individuals and about nations. Such data is available in the form of the survey research data from the Almond-Verba five-nation study and aggregate statistics on the level of economic development in these five nations.[4] The theoretical assumptions we explore require a technique showing the relationships between socio-economic attributes, intervening attitudinal characteristics and rates of political participation; such a technique is provided by causal modeling, which is applied to the analysis in Part II of the study.

II. SOCIAL STRUCTURE AND ECONOMIC DEVELOPMENT

The process of economic development drastically alters the social structures of a nation, and consequently, the social life of its citizens. The effects of social change on political behavior and on the operation of the political system are numerous and complex. In a given nation the political consequences of economic development cannot easily be separated from such issues as historical social cleavages, existing social patterns, and the timing of the developmental process itself.[5] The complexity and variations notwithstanding, some changes in social life appear inevitably to be brought about by the process of economic development. It is these persistent changes which, we hypothesize, have a universal and predictable impact on citizen participation in political life.

Economic development and changes in the class structure. As a nation develops economically, the shape of its social stratification structure is substantially altered. There are increased requirements for trained labor, a growth of opportunities for social mobility, and expanded facilities for formal education. As a consequence, the pyramidal class structure associated with peasant and peasant-worker societies changes to a more diamond-shaped structure. The middle stratum expands and eventually becomes the majority class as great numbers of citizens, whose parents were agricultural or unskilled workers, find jobs in the service industry or otherwise become members of the educated white-collar class.

Economic development and changes in organizational structure. As a nation develops economically, its organizational infrastructure becomes increasingly complex. Differentiation and specialization occur. Social life becomes more organized; work groups (such as trade unions and professional societies), leisure groups (such as youth organizations and voluntary associations), and special task groups (such as civic associations) expand in number and take over duties formerly carried out by the extended family or the small, face-to-face social group. Additional organizations become necessary to coordinate activities of an increasingly interdependent social and economic life. There is a corresponding growth of membership in secondary organizations. The group life structure of a society cannot avoid being substantially affected by the development process.

Economic development and changes in residence patterns. As a nation develops economically, it becomes more urban. Youths leave the farms and small towns in search of education or better paying jobs in the nation's cities. Indus-

W. Milbrath, *Political Participation* (Chicago: Rand McNally Co., 1965).

[3] For example, Samuel P. Huntington, "Political Development and Political Decay," *World Politics,* 17 (April, 1965), 386–430.

[4] Some of these data were analyzed by Almond and Verba, *op. cit.* For description of the methods employed and the sampling problems, see Chapter II and Appendices A and B of that work.

[5] See, for example, the introductory chapter in Seymour M. Lipset and Stein Rokkan, *Party Systems and Voter Alignments* (New York: Free Press, 1967).

try expands where there is population concentration, and availability of jobs attracts yet more people. Larger proportions of the population seek employment and establish homes in urban areas as a consequence of development.

Thus, along with Deutsch and other students of development, we identify three areas of social life known to vary with level of economic development: social stratification, organized economic and secondary associations, and urbanization. The research task is to move from these macro variables to an explanation of whether a given individual citizen will engage in political activity. To carry out this research task, we proceed in four steps:

1. We determine the strength and the cross-national consistency of correlations between the citizen's class position, his organizational involvement, his place of residence, (i.e. urban versus rural) and his level of political participation.
2. We integrate the survey data with aggregate information available for the five nations in order to clarify and evaluate the effect that the individual level correlations have on the larger relationship between economic development and national rates of participation.
3. Within this analysis we explore the possibility that other variables in the development process might have additional effects on national rates of participation. We also examine the rates of participation among similar groups in the five nations, searching for the presence and magnitude of the effects that discrete cultural or historical variables might have on rates of participation. The magnitude of these differences is then compared with the more patterned impact of the development variables.
4. Finally, with the aid of causal modeling, we examine the role played by several cognitive and attitudinal variables in explaining the persistent relationship between socio-economic attributes on the one hand and political participation on the other. In so doing we attempt to refine further the linkages between national economic development and individual acts of political participation. Part II of this study (which appears in the September 1969, issue of the *Review*) presents this analysis.

III. THE PRIMARY VARIABLES: IDENTIFICATION AND CORRELATION

The impact of economic development on class structure, secondary group structure, and residence patterns has been documented.[6] However, in an attempt to assess their impact on levels of participation, we need some means of determining whether they are independent phenomena or a package of relationships which are so tightly intertwined as to be inseparable in terms of their impact on citizens' participation in politics. If, for example, the changes in social class structure take place only in the urban population centers, and if only urban middle and upper class citizens are active in a nation's organizational life, then there would be only one major dimension of social structural change important for this study—urbanization.

In order to answer these questions, we set forth an extensive list of socio-economic items presumed to be related to the social structural changes. We subjected these items to a factor analysis in each nation and then for all five nations grouped together as a single population. In both cases there emerged two relatively independent and very consistent clusters of items. Other items, fewer in number, were consistently unrelated to each other or to either cluster.

The two clusters and two unrelated items are analogs for the individual citizen of the structural variables outlined above. One cluster includes items which measure socio-economic status: education, income, occupation, and an interviewer's rating of social status. The second cluster includes items which measure organizational involvement: number of organizational memberships, amount of organizational activity, involvement in the economic market-place, and involvement in group-related leisure time activities. Thus, there is a cluster which corresponds to the social stratification system and a cluster which corresponds to the organizational infrastructure of the society. Unrelated to these clusters were two items corresponding at the individual level to urbanization: size of present community and length of residence there.

[6] For the relationship between economic development indicators and various indices of unrbanization and class structure, see the data in Bruce M. Russett, *et al., World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964). Also see Donald J. McCrone and Charles F. Cnudde "Toward a Communication Theory of Democratic Political Development," this REVIEW, 61, (March, 1967), 72–79. The literature on general processes of change in the social structure during modernization is, of course, very large. See, among other analyses, S. N. Eisenstadt, "Social change, Differentiation, and Evolution," *American Sociological Review,* 29 (June, 1964), 375–387; and Talcott Parsons, "Evolutionary Universals in Society," *American Sociological Review,* 29 (June, 1964), 339–357.

TABLE 1. SOCIAL STRUCTURE AND POLITICAL PARTICIPATION: PRODUCT-MOMENT CORRELATION COEFFICIENTS IN FIVE NATIONS[a]

Correlation Relationship	Nation				
	U.S.	U.K.	Germany	Italy	Mexico
Urban Residence with Participation Scale	.068[b]	−.023	−.022	−.002	.073
Social Status with Participation Scale	.431	.303	.181	.283	.238
Organizational Involvement Scale with Participation Scale	.523	.480	.480	.490	.515
Urban Residence with Social Status	.159	.040	.166	.175	.118
Urban Residence with Organizational Involvement Scale	−.010	.043	−.011	−.021	.017
Social Status with Organizational Involvement Scale	.435	.313	.213	.304	.227

[a] See Appendix A for comparison of Spearman rank order correlation coefficients and these product-moment coefficients.

[b] Conservative estimates of statistical significance in a random sample whose minimum size is 800 would be as follows:

Significant at .05 level is any coefficient greater than .059.
Significant at .01 level is any coefficient greater than .081.
Significant at .001 level is any coefficient greater than .108.

With a minimum sample of 800 for each coefficient, and using the Fischer Z transformation, the following would represent estimates of significant *differences between* any pair of coefficients:

Significant at .05 level when the difference is greater than .083.
Significant at .01 level when the difference is greater than .116.
Significant at .001 level when the difference is greater than .154.

Actual samples are always greater than 860 after missing data are excluded. Calculations based on Frederick C. Mills, *Statistical Methods*, (N.Y.: Henry Holt and Co., 1955), pp. 306–307. However, it should be noted that, as described in Almond and Verba, *op. cit.*, pp. 509–525, the national samples were cluster samples and collected in somewhat different ways with different degrees of success. In Mexico, no interviews were collected in towns under 10,000. Given these differences and the fact that much of the sampling data were not available in our analysis tapes, we have not attempted to compute a sampling error—it could only be a guess. In general we prefer to rely on the strength and consistency of the relationships rather than on significance tests presuming probability samples. These significance estimates are included primarily to give readers unaccustomed to such data some idea of magnitude.

Three summary indexes were constructed: social status, organizational involvement, and size of place of residence. A political participation scale was also constructed, including the following items: talking politics, contacting local authorities, contacting national authorities, involvement in electoral campaigns, and membership in political organizations and political parties.[7] Table 1 presents pertinent correlation coefficients.

The relationships in Table 1 are clear and remarkably consistent from nation to nation. Although problems in the design of the samples are such that significance tests must be used

[7] See Appendix B for specific scale items and methodology of scale construction. The urban residence variable is size of place of residence for individuals reporting two or more years of residence in that location. (Adding the length of residence control increases the correlation slightly.) It should be noted that the political participation scale does not include voting turnout, partially because the Almond-Verba questions on voting do not lend themselves well to participation analysis, partially because electoral participation seems to include a slightly different dimension than these other factors. There are, of course, variances among these different participation items themselves. Although factor analysis does justify a single scale, future analysis must explore differences among the items. Some separation of the political participation variable is undertaken at various points below.

with care, the following inferences are warranted: (1) Organizational involvement is the predictive variable with the most strength. Within each of the five nations the citizen who is an active member of social groups is more likely to be a political participant than the citizen with few or no organizational involvements. The relationships observed in Table 1 are extremely strong for data of the type available.[8] If the correlation coefficients are squared, using the general rule of estimate, organizational involvement alone predicts approximately 25 percent of the variation in participation. The strength of this relationship is consistent across the five nations. (2) In addition, as expected, the citizen's tendency to be politically active is related to his social status; however, and not fully expected, the relationship between social status and political activity is weaker and less consistent cross-nationally than the relationship between organizational involvement and political activity.[9]

On balance, these findings are familiar ones. The educated, wealthy and occupationally skilled citizen participates in political life at a greater rate than the uneducated, poor and occupationally unskilled.[10] It is well known that social and economic resources provide a citizen with currency he transfers to the political sphere. To turn the statement around, political influence is not randomly distributed across social categories but, rather, tends to concentrate in very disproportionate amounts among the well-to-do and socially involved. Perhaps, however, the difference between social status and organizational involvement as predictive variables is not as familiar. We will have much more to say about this difference.

Urbanization and Political Participation. The least expected pattern in Table 1 is the consistent *absence* of any relationship between urban residence and political participation. Only in two nations (the U.S. and Mexico) is the relationship significant at the .05 level, and even in these nations the relationship is weak, explaining less than one percent of the variance in participation. For nations as developed as the five reported on in Table 1, the tendency for urbanization and mass political activity to co-vary is *not* because city-dwellers are more active than country-dwellers. The absence of a relationship between urban residence and activity rates at the individual level may, thus, help to erase from the literature on political development an ecological fallacy.[11]

We reason as follows: as nations develop economically, two things increase, the proportion of urban-dwellers and the proportion of active political participants. However, the ecological correlation between urbanization and mass political participation is spurious, as is indicated by the absence of a relationship between urban residence and participation *within* each nation. To further investigate the inconsistency between the widely reported ecological correlation and the absence of the correlation at the individual level, we merged the file from the five nations. The correlation between urban residence and activity for the merged file is positive and significant and thus is consistent with the relationship found when aggregate data are used.

It is evident that the ecological correlation is a derivative of the greater levels of urbanization and political participation in more advanced nations. For when the aggregate correlation is decomposed, as in Table 1, political activity is seen to vary hardly at all by urban-rural differences. The fourth and fifth lines in Table 1 suggest

[8] For readers unfamiliar with correlation coefficients in assessing the strength of relationships, Table 4 below presents some of the same patterns in terms of differences between percentages. The reader may wish to review that Table before continuing.

[9] It is interesting that social status is a stronger predictor of political participation in the United States than in any of the other nations. However, it still lags behind organizational involvement. For all the nations, of course, the status measure and the involvement measure are moderately correlated, with the U.S. showing the strongest relationship.

[10] When the individual items composing these summary indices were correlated with political participation, the coefficients obtained were almost always considerably lower than those shown in Table 1. Further, no single item accounts for a disproportionate amount of the correlation between the summary indices and the participation scale. This indicates (1) that the unidimensionality indicated by the factor loadings is justified and (2) that no single variable, such as education, for instance, is the "real" explanation for the strong correlations which appear in Table 1. Social status, in other words, seems to be the common dimension being tapped by the varied findings linking participation to education, income, and the like.

[11] For a discussion of the general problem of ecological fallacies see W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review,* 15 (1950), 351–357. Also see Hayward R. Alker, "A Typology of Ecological Fallacies: Problems of Spurious Associations in Cross-level Inferences," International Social Science Council: Symposium on Quantitative Ecological Analysis in the Social Sciences, Evian, France, September, 1966.

part of the reason: while urbanization is somewhat associated with social status, it is not at all related to organizational involvement. As we have seen, in every nation organizational involvement is the strongest predictor of participation.

In making these initial observations we were troubled by the fact that the five nations being studied would be clustered toward the top end of any development continuum on which all nations of the world could be arrayed. Even Mexico, the "low" case, ranks well above the median. What was needed was a nation, with a democratic form of government, much less developed than those studied in the Five-Nation study. Such a nation would provide a "hard test" for the inferences drawn from the patterns in Table 1. As a matter of scientific interest and personal generosity, Professors Sidney Verba, Rajni Kothari, and Bashir Ahmed made available data from a forthcoming study of political participation in India for our purposes. These data, which suit our needs ideally and which have been preliminarily analyzed by Nie, are from the "Cross National Research Program in Social and Political Change."

The India data provide a striking confirmation of the findings presented in Table 1. The correlation between political participation and organizational membership is .420; between participation and social status, .300.[12] These correlations are of the same order of magnitude as those reported in Table 1, although the former is slightly lower.[13] Social status and organizational involvement appear to be similar in their explanatory power even in the much less developed nation of India.

The India data also confirm our inferences about urbanization and political participation. As is to be expected, citizens of high social status and high organizational involvement are more concentrated in urban areas in a nation at India's developmental level than in the more advanced nations. The correlation between urban residence and social status is .339, much higher than in the five more developed nations; the correlation between urban residence and social involvement is .127, again much higher than the other nations. In spite of the greater concentration of persons with social and organizational resources in the cities of India, the correlation between urban residence and political participation is an insignificant .035. Place of residence is no predictor of political activity.

Urbanization and Local vs. National Political Participation. The findings at the individual level showing no relationship between urban residence and disposition to be politically active run contrary to findings reported in studies using aggregate data. For this reason, and because of the long-standing interest in how urbanization affects political behavior, we felt it necessary to probe more deeply into this question. In particular it is important to insure that a true relationship has not been accidently or mechanically blurred.

The literature about the effects of urbanization on political participation reveals an interesting anomaly. First, stemming largely from Deutsch, there are theoretical notions found in the studies of social mobilization and modernization. Urbanization, it is stressed, creates among citizens new ties to the national scene, increases the amount of political communication, and leads to greater awareness of social and political needs. In short, urbanization is one of the processes of modernization which shifts the political orientations of citizens from parochial to national and participant.[14]

Second, using individual survey data rather than national aggregate data, other investigators have noted that the process of urbanization weakens the ties between individual and community. Presumably one consequence of this has been to decrease citizen participation in local politics. For instance, Dahl writes how living in large political communities can lessen the likelihood of political participation:

> Yet, the larger and more inclusive the unit with a representative government, and the more complex its tasks, the more participation must be reduced for most people to the single act of voting in an election.
>
> Conversely, the smaller the unit, the greater the

[12] Although the effort was to tap the same basic variables of social status, organizational involvement, and participation as in the 1960 study, different specific questions were believed appropriate to the Indian context. The Indian sample is also particularly useful for testing urbanization hypotheses because interviews were conducted in towns as small as 200 persons.

[13] The slight falling off of the relationship between participation and organization membership might be due to the differences in the measures used in India, on the one hand, and the original five nations, on the other. In India, only information on number of memberships was available and this had to be used in place of a composite involvement scale. The correlations between participation and organizational membership only in the five nations are comparable to the correlation produced by the India data.

[14] See citations under footnote 1 above.

TABLE 2. URBAN RESIDENCE AND ATTEMPTING TO INFLUENCE LOCAL OR NATIONAL POLITICAL DECISIONS: PARTIAL CORRELATION COEFFICIENTS CONTROLLING FOR STATUS AND ORGANIZATIONAL INVOLVEMENT IN SIX NATIONS[a]

Correlation Relationship	Control	Nation					
		U.S.	U.K.	Germany	Italy	Mexico	India
A							
Urban Residence with Political Participation *Scale*	NONE	.068	−.023	−.022	−.002	.073	.035
	SES	−.001	−.037	−.054	−.054	.046	.104
	ORG. IN.	.085	−.049	−.019	.010	.035	.020
	SES and ORG. IN.	.039	−.055	−.036	−.022	.059	−.109
B							
Urban Residence with Attempting to Influence *Local* Decisions	NONE	−.008	−.060	−.089	−.006	−.028	−.016
	SES	−.054	−.066	−.114	−.054	−.041	−.126
	ORG. IN.	−.006	−.071	−.089	−.002	−.032	−.037
	SES and ORG. IN.	−.031	−.073	−.108	−.044	−.040	−.155
C							
Urban Residence with Attempting to Influence *National* Decisions	NONE	.087	.037	−.003	.064	.030	.098
	SES	.043	.029	−.011	.044	.019	−.036
	ORG. IN.	.094	.027	−.002	.068	.027	.058
	SES and ORG. IN.	.063	.023	−.005	.057	.022	−.036

[a] Again we wish to express our thanks to Sidney Verba, Rajni Kothari, Bashir Ahmed and the "Cross National Research Program in Social and Political Change," for permission to present the data on India. See notes in Table 1 on significance tests and levels of significance for correlation coefficients. The sample sizes are roughly comparable with the exception of India which has a much larger sample.

opportunity for citizens to participate in the decisions of their government. . .[15]

These seemingly contradictory observations about urbanization and political participation are not actually in direct conflict. The social mobilization literature refers to national participation and mainly uses voter turnout to support the hypothesis. The Dahl thesis refers to local participation and deliberately excludes the act of voting from the consideration.

However, the two observations do present us with difficulties. The operational procedures used in Table 1, which show little to no relationship between urbanization and political participation, combine both national and local items in the participation scale. Thus it is possible that both the Deutsch and Dahl views are correct and that our data manipulation hides two opposing effects, urbanization is increasing national participation and decreasing local participation. To check this possibility, we decomposed the participation scale and separately correlated urban residence, controlling for social status and organizational involvement, with *national* and *local* acts of political activity.[16] Table 2 presents the data.

Part A of Table 2 presents the correlations between urbanization and the overall participation scale, with and without controlling for social status and organizational involvement. In all six countries, the relationship between urban-

[15] Robert A. Dahl, "The City in the Future of Democracy," this REVIEW, 61, (December, 1967), p. 960. For a very different perspective on size of city and citizen representation in local matters, see Kenneth Prewitt and Heinz Eulau, "Political Matrix and Political Representation: Prolegomenon to a New Departure from an Old Problem," this REVIEW, 63 (June, 1969) pp. 427–441.

[16] Methodologically it is quite possible and, for that matter, common for items to demonstrate a strong relationship to a principle factor (e.g., participation level) and at the same time contain uncorrelated portions which are strongly related to different orthogonal factors. This may be what is taking place with the local-national distinction.

ization and overall participation is weakened by introducing controls. If the uncontrolled relationship is positive it either becomes less positive (U.S. and Mexico) or reverses direction (India) when status and organizational involvement are entered as controls. The relationships which are negative to begin with (U.K., Germany, and Italy) become more negative when the controls are introduced. The first and second-order partials confirm the inference drawn from Table 1: urbanization has little independent effect on political participation when the latter is measured by a scale including both national and local acts of political participation.

Part B of Table 2 repeats the same analysis with one part of the participation scale, attempts to influence *local* governmental decisions. Here the simple correlations are all negative. When controls are introduced, the relationship between urban residence and local participation becomes even more strongly negative in every case. While the correlations are weak, they are all in the predicted direction and shift further in that direction when the confounding effects of status and organizational involvement are removed from the relationship.

The findings in Part B lend support to the proposition set forth by Dahl: the larger the place of residence, the less likely is the citizen to be involved as an active participant in local political life. This finding is the first indication that the lack of relationship between urban residence and political participation may partly relate to the different manner in which urbanization affects local and national participation.

Part C of Table 2 is addressed to the social mobilization hypothesis: urbanization is presumed to increase the rate at which citizens attempt to influence national political decisions. The hypothesis is not unequivocally supported. Although five out of the six simple correlations are positive, when social status and organizational involvement are introduced as controls, all but one of these correlations are reduced to an insignificant level. (In the U.S. the coefficient remains just above the .05 level of statistical significance.) It appears that living in an urban area has no significant independent effect on rates of national participation but that urbanization is associated with higher social status which does increase political participation.[17]

[17] The very strong correlation between social class and urban residence and the quite substantial correlation between urban residence and organizational involvement in India may help explain why the aggregate correlation between urbanization and political participation is so powerful when the entire spectrum of the world's nations are included in the analysis. The fact that the correlations between all three of the independent variables and particularly between urban residence and organizational involvement are much stronger in India than in the five more developed nations under study, suggests the magnitude of the changes in social structure which take place in the development process.

The patterns in the data tend to confirm the conflicting implications of urbanization for political participation. The citizen living in the city is likely to have the social status and, in less developed nations (if India is a representative case) the organization involvement which leads to political participation. But the city-dweller loses the facilitating effect of small town life on local participation. The general problem sketched out by Dahl appears to be confirmed by these findings: with respect to participation in local matters there is an "optimal" size for cities. However, the effects of city size are not strong, especially when compared with the impact on participation of social status and organizational involvement. In no case does urbanization explain more than two percent of the variance in rates of participation; in most cases it is less than that. This compares to about ten percent of the variance explained by social status and about twenty-five percent explained by organizational involvement. The comparative weakness of urbanization as a predictive variable leads us to pay much less attention to it than to the other two variables in the remainder of the analysis.

Political Participation: Shifting From Individual Correlates to National Patterns. Analysis presented thus far shows: first, within each of the five nations a citizen's tendency to be politically active strongly varies according to his social status and his organizational involvement; and second, the citizen's tendency to be politically active is but slightly affected by his place of residence, and this weak relationship masks two counter trends. Living in an urban environment depresses the likelihood of local participation while not significantly altering the likelihood of national participation.

We infer from these findings that economic development increases mass political participation *because* associated with economic development are greater numbers of citizens in the middle and upper social classes and greater numbers involved in organizations. From data presented thus far this can only be an inference. We have yet to examine in a direct manner how economic development affects political participation. It is to this issue that we now turn.

With only five cases it is not possible to de-

vise a single definitive test which would substantiate a hypothesis linking economic development with national rates of political participation. What is possible is to carry out a number of smaller tests, each of which can confirm or reject a part of the more inclusive theory. If each of the independent tests confirms the general theory, the cumulative effect will provide some rationale for considering the theory verified.

If true (a) that economic development is the major determinant of national rates of participation; and (b) that this relationship is largely the result of how development affects society's class structure and organizational infrastructure, then data should demonstrate the following:

1. Strong covariance between a nation's level of economic development and its rate of mass political participation.
2. The disappearance of this relationship when social class and organizational involvement are controlled.
3. The lack of strong relationships between mass political participation and non-development-associated variables.
4. Similar *absolute* rates of participation among citizens of similar social class and organizational involvement irrespective of nation, as well as the same *relative* differences from one nation to the next already indicated by the coefficients in Table 1.

Comparing National Rates of Political Participation. Comparing rates of political participation among the five nations shows that the United States and Great Britain have the highest rate; Germany is slightly lower; Mexico and Italy follow in that order and have considerably lower rates of participation than the three more developed nations.[18] With the exception of the reversal between Italy and Mexico,[19] this ranking corresponds with an economic development ranking constructed from aggregate statistics. (Aggregate data on which the rankings are based are presented in Appendix C.)

[18] The ranking was achieved by calculating the national means on the participation variable. This is the same variable used in Table 1. Medians were also calculated and in no case did this alter the ranking.

[19] The Mexican sample contains no respondent from communities with populations less than 10,000. The .232 correlation between size of place of residence and social status in Mexico may account for the reversal in rankings between Italy and Mexico. Indeed, the means are so close at present that it is highly probable that they would reverse with a representative Mexican sample.

Five cases "prove" nothing, but the predicted pattern is apparent. The correspondence in rankings of economic development and mass political participation, based in the present case on survey and aggregate data from five nations, does conform to the rankings reported in studies using only aggregate data but based on many more than five cases. It is consistent with Deutsch, Lipset, Lerner, and others to conclude that national rates of political participation covary with levels of economic development.

The Component Variables: Social Class and Organizational Life. It comes as no surprise to learn that a nation's level of political participation covaries with its level of economic development. The more difficult task is to identify what components of economic development account for the growing numbers of citizens who become active in political life. The theory which guides our analysis points to two component variables: social class and organizational life. These variables are chosen from a long list of social factors known to be affected by development for two reasons. Findings about individual political behavior indicate that status and organizational membership are strongly related to political activity. Further, at the national level, social class composition and organizational life change in highly predictable ways during the process of economic development. It is on the importance of these variables at the individual *and* the national level that our theory depends.

What is needed is some manner of determining whether social class and organizational structure are indeed the components of economic development which predict rates of mass political participation. The theory is strengthened if it can be shown that other components of development are not associated with participation. Although the data available impose constraints on us, and we cannot directly examine the possible impact of additional variables, one simple test can be performed. The relative importance of the two structural variables can be compared to all remaining development-associated variables taken as a residual group.

This can be accomplished by correlating economic development with political participation while controlling for social status and organizational involvement. To accomplish this each respondent was given a score based on his nation's level of economic development. Thus each respondent in the U.S. was scored 5, each British respondent was scored 4, each German 3, each Italian 2, and each Mexican 1. This variable was added to each respondent's data record and the files from the five nations were merged. Level of economic development, now treated as a prop-

TABLE 3. RESIDENCE IN NATIONS AT DIFFERENT LEVELS OF DEVELOPMENT AND POLITICAL PARTICIPATION: SIMPLE AND PARTIAL CORRELATIONS CONTROLLING FOR SOCIAL STATUS AND ORGANIZATIONAL INVOLVEMENT

Correlation Relationship	Control	Residence According to National Level of Economic Development
Residence in Nations at Different Levels of Development *with* Political Participation	NONE	.133[a]
	SES (only)	.048
	ORG. IN. (only)	.015
	SES and ORG. IN.	−.012

[a] Sample size is about 4000. The zero-order correlation is, of course, significant at better than .001.

erty of the individual, was correlated with political participation.

Table 3 presents the results. The now familiar hypothesis derived from social mobilization literature is confirmed. There is a significant relationship between national economic development and individual political participation, as the uncontrolled relationship indicates. The simple correlation between political participation and economic development is .133.

The question we ask, then, is how much of this relationship can be attributed to national patterns of social class and organizational involvement and how much of it can be attributed to other factors associated with economic development. To answer this question we compute the partials, controlling first for social class, then for organizational involvement, and finally simultaneously controlling on both class and organizational involvement. Controlling for social class, the .133 correlation is reduced to .048; controlling for organizational involvement it is reduced to .015; controlling for both (the second order partial), it is essentially zero, a non-significant −.012.[20]

[20] As an additional check stemming from our concern for the accuracy of the rankings among the more developed nations a second dichotomous variable was created giving each respondent in the three developed nations a score of "2" and those in the two less developed nations a "1." The same analysis was then performed on this variable.

The results of this analysis are similar to those reported in Table 3. The simple correlation between the development index and level of participation is .142. The partial for this relationship controlling only for SES is .073. That controlling for only organizational involvement is .030. The second order partial controlling for both SES and ORG. IN. is an insignificant .002.

If other factors associated with economic development were independently affecting levels of citizen political participation, then some significant relationship should have remained when the effects of status and organizational membership were partialed out. However, this clearly is not the case. The partial correlations show that *the two variables alone account for all of the difference between the participation levels of citizens in the more and the less developed nations.*

This finding can be rephrased in terms of the general theory being advanced in this paper. Economic development leads to greater rates of political participation *because* associated with economic development is an expanding middle-class and an expanding organizational infrastructure. Social class and organizational life are the components of economic development which most strongly affect mass political participation.

Since we have only five nations and since there can be nation to nation differences not attributable to economic development, the test we devised is hardly a conclusive one. The test is adequate, however, for us to conclude that the inference about economic development and political participation withstands this particular attempt to falsify it. We are unable to show any relationship between development and rates of participation which cannot be explained by reference to the two important social structural variables—social class and organizational involvement.

Three types of data have now been presented: (a) individual correlations showing the association between social status, organizational involvement, and urban residence, on the one hand, and political participation, on the other; (b) rankings of national levels of economic development and of aggregate measures of political participation; (c) partial correlations showing the effect of economic development on political participation when social class and organizational involvement are controlled. These data, from different perspectives, tend to confirm the general hypothesis that economic development alters the class and organizational structures of societies which in turn increases the level of mass political participation. No single test is conclusive, but taken together they present a strong case for the theory. Each of the following sections in Part I of this article present additional analysis further testing and clarifying these relationships.

Idiosyncratic National Differences and Cohart Comparisons Across National Boundaries. The analysis has left two important questions

TABLE 4. POLITICAL PARTICIPATION LEVELS AMONG STATUS AND INVOLVEMENT GROUPS IN FIVE NATIONS

Organizational Involvement Level	Social Status Level	Percentage of Active Participants				
		U.S.	U.K.	Germany	Italy	Mexico
High	High	75[a]	77	62	66	61
High	Low	67	67	65	53	71
Medium	High	41	53	30	34	26
Medium	Low	27	33	30	23	22
Low	High	32	38	21	10	16
Low	Low	10	21	14	11	8

[a] That is, 75% of those American respondents classified as having high involvement and high status report participation levels high enough to be classified as "active." Cutting points are the same in absolute score terms from nation to nation, but are intrinsically arbitrary. The size of the groups upon which the percentages are based are as follows: High-high 315, 134, 198, 62, 101; High-low 43, 101, 130, 30, 149; Medium-High 203, 170, 142, 211, 108; Medium-low 106, 345, 123, 242, 315; Low-High 154, 49, 137, 80, 127; Low-low 134, 146, 116, 162, 495.

unanswered. First, are the absolute levels of political participation among social status or organizationally involved cohorts similar from one nation to the next? To learn (as we do in Table 1) that a Mexican laborer participates in politics less frequently than a Mexican businessman *and* that a German laborer participates less frequently than a German businessman cannot tell us whether Mexican and German laborers or Mexican and German businessmen display similar rates of political participation.

The second unanswered question is a corollary of the first. Are there historical or cultural factors particular to a nation which, independently of that nation's level of economic development, affect how politically active its citizens are? To a degree the answer to such a question will always be yes. However, the theoretical thrust of this paper suggests that the effects of discrete national experiences will be minimal in comparison to the effects directly attributable to economic development, at least to the two components of economic development identified in the analysis. Our next task, then, is to devise a way to test this assumption. In answering the first question about political participation of coharts across national boundaries, we provide an indirect answer to the second question. If we find that individuals at the same level of social class and organizational involvement are approximately equal in their political participation from one nation to the next, it is unlikely that unique cultural, historical or political variables are causing different rates of mass political participation.

Table 4 presents relevant findings. Respondents in each nation are classified into six groups by trichotomizing the organizational involvement scale scores and dichotomizing the social status scores.[21] Using the same absolute criteria in all nations, the proportions of "politically active" citizens in each of the six groups are calculated for each nation. These proportions range from 8% among lower class organizationally uninvolved Mexicans to 77% among upper class organizationally involved British.[22]

The soundest and most obvious conclusions to be drawn from Table 4 are those already clear from the correlation coefficients in Table 1; Social status and organizational involvement, especially the latter, are extremely powerful predictors of political participation. An average of 11% of the Low-Low citizens are classified as active participants; an average of 70% of the High-High citizens are politically active. These relationships, it should be emphasized, overwhelm all other factors shaping the participation patterns.

Table 4 does show, however, that there are important differences between nations which *do not* correspond to levels of economic development. For instance, in each of the six cohort groups, Germans are less politically active than British. Yet these two nations are at roughly similar levels of development. Similarly, British, within each group, are more active than Americans and yet the U.S. measures higher on economic development.

Note as well the unusually high proportion of political participants among those low on both social status and organizational involvement in Great Britain. Historical or political factors appear to be operating in Britain which increase mass political participation at a rate greater than would be predicted from knowledge of Britain's economic development level alone.

Other patterns in Table 4 are also indica-

[21] The same absolute cutting points were used in each nation. Cutting points were chosen to equalize, as far as possible, the size of the groups. Initial tables showed a trichotomization of both variables, but the size of the groups obtained was too small, especially among upper class respondents in the less developed nations, and the results of that procedure were somewhat misleading. On the other hand, a three group division along organizational involvement lines does help show visually, more effectively than a simple dichotomy, the strength and consistency of that variable.

[22] Politically active citizens are arbitrarily defined as citizens participating at or above the grand mean of the participation variable when all five national samples are combined.

tive of non-developed associated differences between nations. Mexicans of low status but high organizational involvement have an exceptionally large number of political participants (Mexico also shows an unusually large number of low status high involvement citizens). Germany reveals a similar, if less dramatic pattern. These figures possibly indicate a more organized, and therefore politically relevant lower class in these two nations.

In short, idiosyncratic differences between countries do exist, but they are very small when compared to the extremely strong pattern revealed in Table 4.[23] With a few important exceptions *individuals with similar social statuses and similar organizational involvement display similar absolute levels of political participation no matter in which of the five nations they live.* This finding confirms once again the hypothesis that the two components of economic development, expanding middle-class and growing organizational infrastructure, sharply affect levels of mass political participation.

IV. THE LINKING VARIABLES: POLITICAL ATTITUDES AND THE DEVELOPMENTAL PROCESS

Examining general relationships between social structures and political participation can take us only so far. To explicate more fully how economic development relates to political participation it is necessary to complicate the causal model. Up to this point, we have utilized the model suggested by Deutsch and others which looks as follows:

Economic Development → Changes in the Social Structures → Increased Political Participation

Discussions involving this model suggest that changes in the social structure affect political participation *because* citizen attitudes about politics are altered. Numerous survey studies in the U.S. and elsewhere show, for example, that persons who feel confident of success in influencing decisions are more likely to be political participants, and that middle class persons are more likely to feel confident.[24] Hence, increasing the number of middle class citizens increases the number of confident citizens, and this, in turn, increases political participation.

The causal model with the intervening attitude component looks as follows:

Economic Development → Changes in the Social Structures → Changes in the Distribution of Certain Political Attitudes → Increased Political Participation

There may be feedback loops in this process. Successful political participation, for instance, promotes greater confidence; successful participation may even alter the political structures of society so that participation itself is facilitated (expanding the franchise, for instance). However, for the moment we set aside the problem of feedback effects and limit attention to testing the basic assumptions of the model.

Questions included in the Five Nation study allow us to explore this causal model in a limited way. We begin by identifying five attitude sets which frequently are cited as the likely intervening variables explaining why socio-economic attributes predict rates of political activity.[25] The five are: 1) sense of citizen duty, 2) basic information about politics, 3) perceived stake in political outcomes, 4) sense of political efficacy, and 5) attentiveness to political matters.

We examine the relationship of these intervening variables to the general theme of economic development and political participation in two steps. First, at the nation-state level, we examine the relationship between measures of economic development and aggregate scores for the five attitude sets, and between the attitudes and general rates of political participation. Second, using causal modeling techniques, we investigate the importance of these attitude sets in linking socio-economic attributes and political participation.

Table 5 presents data pertinent to the first step. The five nations are ranked by level of economic development. Then, in column B, the nations are ranked in terms of mean level of organizational involvement and social status. The next five columns (C) present the rankings of the various political attitudes (again using mean level). The final column shows the mean level of political participation for the five nations.

Although there are various methodological difficulties in such absolute rankings (and we especially must recall the bias in the Mexican sample),[26] Table 5 does confirm the reasoning outlined. Across Table 5, the three more developed nations *always* rank above the two less developed. The Kendall coefficient of concordance, or W, which measures the amount of agreement among multiple rankings, is .859 and is statistically significant at the .001 level. (The maximum observable value of Kendall's W is 1.0.)

Economic development alters the social class structure and the organizational infrastructure of a society. One consequence of these alterations

TABLE 5. ECONOMIC DEVELOPMENT, SOCIAL STRUCTURE, ATTITUDE STRUCTURE, AND POLITICAL PARTICIPATION

A	B		C					D
Economic Development	Social Structure		Attitude Structure					
Rank Order of[a] Countries with Respect to Measures of Economic Development	Social Status[b]	Social Involvement	Citizen Duty	Political Information	Impact of Politics	Political Efficacy	Political Attentiveness	Political Participation
United States 1	(1)	1	1	2	1	1	1	1.5
United Kingdom 2	(2)	2.5	3	3	3	2	3	1.5
Germany 3	(3)	2.5	2	1	2	3	2	3
Italy 4	(4)	5	5	4	5	4.5	5	5
Mexico 5	(5)	4	4	5	4	4.5	4	4

Kendall Coefficient of Concordance W = .859 Sig. > .001.

[a] Appendix C presents the aggregate data of measures of economic development for the five countries on which the rankings are based.

[b] Because three of the four measures of social status (income, occupation, and interviewer rating of SES) included in the survey were designed only to measure relative positions of respondents within each of the nations, meaningful comparison of mean level of social status between nations is not possible. Therefore, the rankings of social status in this table are based on information available from aggregate indicators of social status rather than on the survey samples. For this reason the rankings in this column were excluded from the computation of the coefficient of concordance presented above.

is the greater proportion of citizens who have social attributes—education, stable incomes, white-collar jobs, organizational memberships—which, as already shown, relate to political participation. Further, the more developed nations have more citizens holding the views normally associated with political activity. Greater proportions of Americans, British, and Germans

[23] Table 4 also reveals a systematic pattern for citizens of the three developed nations, at any given status and involvement level, to be slightly more active in politics than their coharts of the two lesser developed nations. This finding is in direct contradiction to Table 3 which reports, and was so interpreted, that once social status and organizational involvement have been controlled there are no significant differences in participation rates directly related to the development process. Table 4 appears to suggest just the opposite.

A brief methodological excursion is necessary to explain the seeming contradiction. The explanation turns on how sub-categories are constructed. What is happening in Table 4 is that those classified as upper status in the more developed nations contain a much higher proportion of *very high status* citizens than does the cohart group in the less developed nations. The converse is true as well. There is a higher concentration of *very low status* persons in the low status categories in Italy and Mexico than in the three more developed countries. There is no easy way to compensate for this artifact of data manipulation. Irrespective of where these continuous variables are cut, there are greater proportions of very highs or very lows in comparable categories when shifting from the more to less developed nations. This reflects the social structures of the different nations—a reality which derives directly from overall level of economic development. The unequal concentrations of very highs and very lows explains most, if not all, of the differences between most and least developed nations detected in Table 4. It also alerts us to the methodological problems of making inferences from category groupings.

[24] See, for example, Dahl, *Who Governs?, op. cit.*

[25] A more detailed discussion of these five attitudes as well as the logic connecting them to socio-economic traits and to political participation can be found in Part II, appearing in the September 1969 issue of this REVIEW.

[26] See footnote 19 above.

than Mexicans or Italians have a sense of civic duty, are politically informed, see the impact of public policies on their lives, feel politically efficacious, are attentive to politics—and, finally, actively participate in political life.

We should emphasize again that there are national differences in political attitude scores which cannot be explained by differences in developmental levels alone. Obviously cultural heritages and specific national experiences or diverse system arrangements leave their particular impact on national attitude patterns. Germany ranks higher than the United States on mean level of political information, even though its general level of development is lower. An exploration of other attitude patterns, especially those associated with affect towards national institutions and inter-group perceptions and evaluations would reveal further differences.[27] Many of these were pointed out by Almond and Verba in their initial analysis and we shall not explore them here.[28] Nonetheless, we wish to emphasize that insofar as attitudes bearing on individual political involvement and attentiveness and personal political competence are concerned, the development-associated patterns seem to be extremely powerful. Future analyses of cross-national differences should be sensitive to the often overriding power of these relationships.

We can summarize the theoretical argument briefly: economic development alters social structures, particularly the class structure and the secondary group structure. Expanding the middle class and increasing the organizational complexity of society changes political socialization patterns; greater proportions of citizens have those politically relevant life experiences which lead to attitudes such as political efficacy, sense of civic duty, etc. These attitudes motivate the citizen to participate in politics, sensitize him to available opportunities, and provide him with political resources. Thus it is that economic development increases the rate of mass political participation.

The many implicit hypotheses in this causal model cannot be directly examined in a table reporting aggregate measures. To investigate systematically the pathway from development to political participation we must examine the extent to which individual traits associated with position in the class structure and with involvement in organizational life do in fact affect political attitudes and the extent to which political attitudes are intervening variables between socio-economic characteristics and political participation. These tasks are undertaken in Part II of this paper.

The second part of the paper will return to the data presented in this part insofar as these data are relevant to several important issues. For instance, that organizational involvement leads to political participation irrespective of the social status of the individual is directly related to questions about mass politics and political mobilization. Further, as we shall stress in the second part, those who are politically active because they are organizationally involved express different political attitudes from those who are active because of their social status. This finding leads to consideration of the ancient debate between advocates of mass democracy and apologists for elite democracy. Our inattention in Part I to some of the "larger" issues suggested by the data will hopefully be remedied in Part II.

[27] Further research will have to distinguish, of course, between the different dimensions of the variables here considered. For example, the "information" questions here only cover very basic items like the names of national party leaders. It might be that detailed information would tend to follow, rather than proceed, participation. The various dimensions of political efficacy are also a matter for investigation. At a very abstract level, efficacy seems to become more related to general national affect items. At the more concrete and personalistic level, it follows the pattern here outlined: linked to personal status and involvement, varying little by national pattern. Thus, the only item on which an underdeveloped nation scored higher than a developed one was on an estimate as to whether possible national participation might succeed in influencing outcomes. Here, Mexicans scored higher than Germans, although very few felt they understood local or national events, or could name any potential strategy of influence. Again, the factor analysis justifies the dimension of the scale, but there are obviously additional dimensions to explore.

[28] Almond and Verba, *op. cit.*

APPENDIX A

*General Methodology and Data Analysis**

A Note on the Use of Parametric Statistics with Survey Data:

Most of the data analysis reported in this two-part paper consists of product moment correla-

* Data analysis for this paper was performed at the Stanford Computation Center under an NSF grant to Stanford University for unsponsored research. Most of the analysis was accomplished with programs from the *Statistical Package For the Social Sciences*. See Nie, Norman H., Dale Bent, and C. Hadlai Hull *SPSS: Statistical Package For the Social Sciences* (New York McGraw Hill Book

tions, partial r's, and standardized regression coefficients (path coefficients) all of which assume interval ratio scales. When employing this type of analysis with indexes which are based primarily on groups of ordinal survey items, there is always some question as to the validity of the relationships. We also quickly discovered that there is less than unanimous agreement among statisticians concerning both the advisability and/or the effects of violating the assumptions of interval scales.

As a check on the validity of the matrix of simple correlations (between our eight indexes) an identical matrix was computed using Spearman rank order coefficients.** The product moment correlations were then compared to the Spearman correlations. A visual comparison of these two matrices revealed that the majority of pairs of coefficients were separated by less than .01 and that no pair had a spread greater than .03. The mean difference between the pairs of coefficients was .014. As a further check we examined the direction of these differences to determine if the small differences tended to introduce any systematic bias. This analysis indicated that the errors were also totally random with the Spearman coefficients tending to be *very* slightly lower on the average.

We feel relatively confident from this comparison that very little distortion was introduced by the data's inability to meet the assumptions of interval ratio scales. We also feel, however, that this may to a large part be due to the method of index construction used. The vast majority of our scales had an absolute zero point; most approached a normal distribution and most were variables ranging from 0 to 100.

A Note on the Desirability of Multiple Item Indexes:

Throughout our experiments with different scale construction techniques for our analysis one clear finding emerged. When some test of unidimensionality could be demonstrated for any group of three or more of our survey items, a composite index of those items always produced a relationship with a given dependent variable which was greater than twice the relationship between any single item in that index and the dependent variable. This remained true irrespective of the type of scale construction technique used or the type of test of unidimensionality employed. Thus the relationship (r^2) between the social status index and the participation index is greater than two times the (r^2) between any item in the SES index and the participation index. If we reverse the procedure, decomposing the participation index, the results are similar, i.e., the r^2 between the two indexes is greater than two times the r^2 on any item in the participation index correlated with the SES index. A *very* small part of this finding may be explained by the slight tendency of correlation coefficients to vary with the number of categories contained in the variables. The vast majority of these differences, however, are clearly a function of the greater reliability of multiple item indexes. It seems clear that multiple item indexes greatly reduce random or idiosyncratic errors introduced by the interview situation, the wording of specific questions, and the meaning of specific attitudes and behaviors as they relate to a larger underlying dimension.

APPENDIX B

Scales and Indexes: Construction and Contents

I. Scale Construction Techniques.

A. Weighted Scales

A number of different techniques for scale and index construction were experimented with during the preparation for our analysis. Guttman scale analysis had to be abandoned because of item rotation within the scales among national samples. While the same items were usually scalable in the five countries, the position of the items would often be arranged in totally different patterns from one nation to another. Further, a number of indices that we desired to create did not meet the Guttman criteria—some for conceptual reasons and others for operational ones.

After some experimentation, an index construction technique was used which we felt would meet the following desired criteria:

1. Items would be weighted in the index proportional to their relationship to the common underlying dimension of the index.
2. Missing data should be handled in a way which minimized the number of missing cases and at the same time insured that the multi-variate analysis would be based on roughly the same populations from one set of variables to another.

For indexes containing three or more items, the following procedure was employed:

1. The items were entered into a factor analysis, and the item loadings from the first unrotated (principal component) factor were used as item weightings for the indexes. A loading of $\geq .50$

Co., forthcoming). Several special purpose programs required by our analysis were written by C. Hadlai Hull, systems programmer for Stanford's institute of Political Studies.

** We are grateful to Professor William Paisley of the Communications Department at Stanford for consulting with us on this matter and for suggesting this checking procedure.

was required for an item to be used in an index.*

2. The respondent's score was divided by the number of categories in the item so that a given item's contribution to an index would be based solely on its factor loading and not inflated or deflated by the number of categories in the original coding.
3. A minimum number of non-missing responses for each index was decided upon based on the amount of missing data in the items entering a given index and on what we felt would constitute a minimum reliable score. Respondents with a greater number of missing items than were specified by this minimum were excluded from further analysis. A procedure was worked out so that scores based on a different number of items would be comparable and have a minimum distortion on the weightings. With this procedure no more than 14% of any national sample was missing on any index, and the average was about 5%.
4. Sums were then adjusted and rounded so that scores always ranged from 0 to 100.

The formal procedure for computing these weighted indexes is:

$$\text{weighted index}_i = \left(\frac{(X_i/C_i \cdot W_i) + (X_j - C_j \cdot W_j) \cdots + (X_n/C_n \cdot W_n)}{\sum_{i=1}^{1(n)} W_n} \right) \cdot 100$$

where X_i = a respondent's score for the ith item of the index.

where C_i = the number of categories or values in the ith item of the index

where W_i = the factor loading of the ith item of the index.

The sum of these values is then divided by the sum of the weights for all items used in the index. This remainder is then multiplied by 100 and rounded, yielding index scores between 0 and 100. When one or more items is missing, the corresponding $(X_i/C_i \cdot W_i)$ steps in the numerator are deleted, and the denominator is adjusted by the following procedure:

$$\sum_{i=1}^{1-N} W = \sum_{i=1}^{1-N} - (\sum mw)$$

where ΣMW corresponds to the sum of the weights of the missing items for a given respondent. A tally is kept on the number of missing items for each case. If this tally was greater than the maximum allowed for that index a scale score was not assigned for that case.

B. Simple Additive Indexes.

1. The items were required to correlate with each other at .45 or higher.
2. The coding categories were adjusted where necessary so that each item contributed equally to the index.
3. Cases were rejected when either of the items were missing.

II. Contents of the Indexes

Items Included in The Socio-Economic Status Index—Weighted Index—Three Non Missing Responses Required

Item 1: Occupation of respondent coded from unskilled worker to higher management. Note: If respondent was housewife, the spouse's occupation was coded instead.

Item 2: Family income. Eight categories of income were used in all countries. See the Five National Study Codebook.

Item 3: Educational level. Collapsed recoding required for compatibility in five nations into following categories: no schooling, grammar school, high school, college, missing data.

Item 4: Interviewer rating of socio-economic status. Coded in four categories. See documentation from NORC interviewer instructions for Five Nation Study for criteria used.

Items Included in the Organizational Involvement Index—Weighted Index—Three Non Missing Responses Required.

Item 1: Labor force participation. Economically active versus non-active. Housewives, persons permanently unemployed or unemployable were coded as economically inactive.

Item 2: Organizational memberships coded by number of memberships. Special recording: If respondent was a member of a political organization as indicated by another item or interview schedule it was subtracted out of tally on this variable to remove auto correlation between this scale and the scale of political participation.

Item 3: Leadership activity in organizations, held leadership position, yes or no.

Item 4: Activities outside of work and family. Exact wording "Now aside from your work and your family what are the activities that interest you most that you spend your free time on?" Coded as (1) loner activities (2) informal group activities (3) formal or organizational group activities. Recoded from Five Nation Study data.

Items Included in the Place of Residence Index

Item 1: Size of community of residence at time of interview.

* For a discussion of the basic concepts of principle component factor indexes see Hagood, M. J., and P. O. Price, *Statistics for Sociologists* (New York, Henry Holt and Company, Inc., 1952), Chapter 26.

Item 2: Length of residence in Community at time of interview. (Used only as a control. Respondents with less than 3 years of residence excluded from the index).

Items Included in the Political Participation Index —Weighted Index

NOTE: A slightly different index was used in the U.S. due to the absence of the phenomena of memberships in political parties in this country. In all countries except the U.S. out of a possible five non-missing responses were required out of a possible of six.

Items used in all countries.

Item 1: What about talking about public affairs to other people? Do you do that nearly every day, once a week, from time to time, or never?

Item 2: Have you ever done anything to try and influence a local decision?

Item 3: Have you ever done anything to try to influence an act of (SPECIFY NATIONAL LEGISLATURE)?

Item 4: (IF A MEMBER OF SOME ORGANIZATION) Are any of the organizations you belong to in any way concerned with governmental, political, or public affairs? For instance, do they take stands on or discuss public issues or try to influence government actions?

Items Used in All Countries But the United States

Item 5: Now we would like to find out something about your party preference and how you vote. Are you currently a member of any political party or organization?

Item 6: Were you ever a member?

Item Used Only in the United States

Item 5: Have you ever been active in a political campaign? That is, have you ever worked for a candidate or party, contributed money, or done any other active work?

Items Included in the Index of Citizens Duty— Simple Additive Index

Item 1: We know that the ordinary person has many problems that take his time. In view of this, what part do you think the ordinary person ought to play in the local affairs of his town or district? (IF NEEDED) What *specifically* ought he to do?

Item 2: People speak of the obligations that they owe to their country. In your opinion, what are the obligations that every man owes his country?

NOTE: Both items were coded with most active types of participatory obligations given the highest value and the most passive types of obligation the lowest value.

Items Included in the Index of Political Information—Simple Additive Index

Item 1: We are also interested in how well known the national leaders of the various political parties are in this country. Could you name three leaders of the ———— party? Could you name three leaders of the ———— party? Could you tell me the name of a leader from the ———— party?

Item 2: When a new Prime Minister (MEXICO AND UNITED STATES: PRESIDENT) comes into office, one of the first things he must do is appoint people to cabinet positions and ministries. Could you tell me what some of these cabinet positions are (IF NEEDED) Can you name any others? (PROBE UNTIL RESPONDENT NAMES FIVE CABINET POSITIONS OR UNTIL HE KNOWS NO MORE)

Items Included in the Index of Political Efficacy— Weighted Index—four non-missing responses required.

Item 1: How do you feel about this? Thinking of the important national and international issues facing the country, how well do you understand them?

Item 2: How about local issues in this town or part of the country? How well do you understand them?

Item 3: Suppose a regulation were being considered by (SPECIFY MOST LOCAL GOVERNMENTAL UNIT: TOWN+ VILLAGE+ ETC.) that you considered very unjust or harmful. What do you think you could do? (IF NEEDED) Anything else?

Item 4: If you made an effort to change this regulation, how likely is it that you would succeed?

Item 5: Suppose a law were being considered by (SPECIFY APPROPRIATE NATIONAL LEGISLATURE FOR EACH NATION) that you considered to be very unjust or harmful. What do you think you could do? (IF NEEDED) Anything else?

Item 6: If you made an effort to change this law, how likely is it that you would succeed?

NOTE: Items 3 and 5 were coded so that respondents who felt they could do something (no matter what) were coded high while those who said they could do nothing were coded low.

Items Included in the Index of Political Attentiveness—Weighted Index 3 non missing responses required

Item 1: Do you follow the accounts of political and governmental affairs? Would you say you follow them regularly, from time to time, or never?

Item 2: What about newspapers—do you follow

accounts of political and governmental affairs in the newspapers nearly every day, about once a week, from time to time, or never?

Item 3: What about on the radio or television? Do you listen to accounts of public affairs nearly every day, about once a week, from time to time, or never?

Item 4: What about magazines? Do you read about public affairs in magazines about once a week, from time to time, or never?

Items Included in Index of Perceived Government Impact on Life—

Item 1: Thinking now about the national government in (LONDON, BONN, ETC.), about how much effect do you think its activities, the laws passed and so on, have on your day-to-day life? Do they have a great effect, some effect or none?

Item 2: Now take the local government: about how much effect do you think its activities have on your day-to-day life? Do they have a great effect, some effect, or none?

APPENDIX C

Ranking of Countries by Measures of Economic Development

Country	% of Labor Force in Service Occupations*	Gross Nat'l Product in U.S. $**	Literacy	Overall Rank
U.S.	42.0%	$2577	98%	1
G.B.	39.8	1189	98.5	2
Germany	29.9	927	98.5	3
Italy	22.9	516	87.5	4
Mexico	21.3	262	50	5

* Source: United Nations, Compendium of Social Statistics, 1963, New York, 1963.

** Source: Mikoto Usui and E. E. Hagen, World Income, 1957. Center for International Studies, M.I.T., Cambridge, Mass., 1959.

CULTURE AND POLITICAL DEVELOPMENT: HERDER'S SUGGESTIVE INSIGHTS*

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I. INTRODUCTION

When in the early thirties Harold Lasswell declared that "political symbols and practices are so intimately intertwined with the larger array of symbols and practices in culture that it is necessary to extend the scope of political investigation to include the fundamental features of the culture setting"[1], he was very much a voice in the wilderness. Today Lasswell's words have almost become commonplace in the vocabulary of political science. In this, as in many other current concerns, Lasswell's early work has rightly been judged seminal.[2] It substantially contributed towards the prolific expansion of the academic boundaries of political enquiry within the last three decades, in particular to the growth of interest in psychological and sociological approaches. Increasingly students of political behavior in both 'established' and 'emergent' nations have come to realize that purely formal and legalistic conceptual frameworks are inadequate to provide meaningful answers to such problems as persistence and change, socialization, political cohesion, and the complex bases of political authority and legitimacy. This realization, though it has made political science a more rather than less problematical undertaking, nonetheless has had the result of adding new dimensions or perspectives to its analytical vision. Indeed, in the course of this development the very notion of the political has undergone a profound re-appraisal.

In this paper I wish to draw attention to an eighteenth-century thinker, J. G. Herder, many of whose ideas can be said to have foreshadowed both the current re-appraisal of the political and the terms in which it has found expression. His challenge to the accepted, and essentially Bodinian and Hobbesian doctrines of law, sovereignty and nationhood remarkably presages not only Lasswell's injunction to heed the relevance of culture patterns to political processes, but also several other themes that figure prominently in contemporary socio-political theorizing. It would seem, therefore, that Herder's thought has gained rather than lost in significance in the period separating his time from our own.

To say this is not to suggest, however, that Herder was a political thinker first and foremost. Indeed, only one of his publications, the essay *On the Reciprocal Influence of Government and the Sciences* (1780) bears a title which readily identifies it as a political treatise. But then, political conditions in eighteenth-century Germany generally—Herder called it a *terra obedientiae*[3]—and his official position at the court of Weimar in particular, were hardly conducive to the public expression of political ideas. The chapter on government in the *Ideas for a Philosophy of the History of Mankind* (1784–91), the work for which Herder is best known, had to be rewritten four times in order to pass the censor. In contrast to other West-European countries, Germany was a political desert. None of the major or minor states that composed the only nominally-existing Empire offered any scope for shared political activity. There was no effective parliamentary life, no political parties, few, if any, political clubs, and merely the semblance of a popular press. 'Politics' was virtually tantamount to personal rule by the princes, assisted by councillors so privy that no one ever heard them speak.[4] Under

*An award by the Canada Council in 1967 enabled me to do further research on unpublished Herder sources in Germany.

[1] Harold D. Lasswell, *World Politics and Personal Insecurity* (New York: McGraw Hill, 1935, Free Press ed. 1965), p. 158.

[2] Heinz Eulau, "The Maddening Methods of Harold D. Lasswell: Some Philosophical Underpinnings", *Journal of Politics*, 30 (1968), 3–24.

[3] XVIII, 315 and 535. (All references are to the Suphan edition of Herder's *Werke*, 33 vols., Berlin, 1877–1913).

[4] Thus wrote F. K. von Moser, one of the outstanding publicists of the period in his challenging tract *Der Herr und der Diener* (1759), cited in A. Fauchier-Magnan, *The Small German Courts in the Eighteenth Century* (London: Methuen, 1958), p. 55. Regarding the political and intellectual background of Herder's Germany, probably the best recent works in English are Leonard Krieger, *The German Idea of Freedom* (Boston: Beacon, 1957), pp. 8–80; W. H. Bruford, *Culture and Society in Classical Weimar (1775–1806)* (Cambridge: Cambridge University Press, 1962) and Isaiah Berlin, "Herder and the Enlightenment," in Earl R. Wassermann (ed.), *Aspects of the Eighteenth Century* (Baltimore: Johns Hopkins University Press, 1965), pp. 47–104.

those circumstances, it is scarcely surprising that men of humble birth like Herder could cherish little hope of making any impact by word or deed on political life. Acutely aware of these conditions, Herder preferred, therefore, to confine his more radical ideas to private letters or to disguise and disperse them in non-political contexts.[5]

An equally weighty reason for Herder's merely sporadic writing on specifically political questions lies in the heterogeneity of his intellectual interests, ranging from literary criticism, aesthetics, philology, folklore and history to psychology, anthropology, philosophy and theology. His was a synoptic quest. What may to an intellectual in the age of specialization seem a suspiciously diverse, if not odd, array of fields was to Herder a necessary condition for acquiring a 'whole view' of man and his world. This multi-dimensional approach no doubt accounts for the fragmented nature of most of his writings. But it also has the qualities of its defects. Herder's social and political thought, devoid though it is of systematic elaboration, makes up in breadth and originality of insight for what it lacks in orderliness of treatment. Indeed, at times it reveals a perspicacity of vision which perhaps we can only now truly recognize. This holds true for both 'substantive' and 'methodological' issues. Herder was one of the first to acclaim the French Revolution as the most important historical event since the Reformation. He saw the magnitude, and foresaw many of the consequences of ethnocentrism, racial discrimination and colonialism, and the various forms of social disintegration associated with industrialization and urbanization. In his stress on interaction, the dialectic interplay of opposites, and the essential oneness of persistence and change, he anticipated in important respects subseqent methodological trends in the social sciences. There is also a striking similarity between Herder's interest in 'wholes' and *Gestalt* or systemic approaches, and between his theory of inter-generational value transmission and contemporary critiques of replicative socialization. Finally, his problematic notion of social and political activity free from norms of centrality and hierarchy appears less utopian in the light of the 'stateless communities' described by political anthropologists, or of some current thinking on administrative processes and decision making.[6] Several of these themes have been elaborated elsewhere.[7] Here I shall primarily focus on those ideas which bear upon problems of nation-building; by sketching in their philosophical underpinnings I also hope to point up aspects of methodological import.

II. CULTURE AND CIVILIZATION

Although the intimate linkage of science, technology, secularism and the nation state is characteristic of only one particular form of historical development, it has come to be identified as a 'world culture.'[8] Similarly, the notions of dynamic change, of 'modernization' and ineluctable progress, said to underlie this particular historical development, have come to assume the status of cultural absolutes, of universal value

[5] Herder's *Vom Geist der Ebräischen Poesie* (The Spirit of Hebrew Poetry), published in 1783, is perhaps the best example of this. I have found this rather neglected source of Herder's sociopolitical thinking quite intriguing. For an interpretation of this work, see F. M. Barnard, "The Hebrews and Herder's Political Creed," *Modern Language Review,* 54 (1959), 533–546, or "Herder and Israel," *Jewish Social Studies,* 28 (1966), 25–33.

[6] See, for example, Charles E. Lindblom, *The Intelligence of Democracy: Decision Making Through Mutual Adjustment* (New York: Free Press, 1965), pp. 293–310; Marc J. Swartz, Victor W. Turner, and Arthur Tuden (eds), *Political Anthropology* (Chicago: Aldine Publishing Co., 1966); David Easton, "Political Anthropology," *Biennial Review of Anthropology,* ed. B. Siegel (Stanford: Stanford University Press, 1959), pp. 210–262; and Carl J. Friedrich and Morton Horwitz, "Some Thoughts on the Relation of Political Theory to Anthropology," this REVIEW, 62 (1968), 536–545.

[7] F. M. Barnard, "Herder's Treatment of Causation and Continuity in History", *Journal of the History of Ideas,* 24 (1963), 197–212; *idem, Herder's Social and Political Thought: from Enlightenment to Nationalism* (Oxford: Clarendon Press, 1965, rev. ed. 1967); *idem,* "Metaphors, Laments, and the Organic Community", *Canadian Journal of Economics and Political Science,* 32 (1966), 281–301; *idem,* with Jene M. Porter, "Discontent and Political Development: a Discussion of Some Contemporary Methodological and Substantive Problems," paper presented at the 1968 meeting of the Canadian Political Science Association; and *idem, Herder on Social and Political Culture* (Cambridge: Cambridge University Press, 1969). This work includes translations from the *Travel Diary, Essay on the Origin of Language, Yet Another Philosophy of History, Dissertation on the Reciprocal Influence of Government and the Sciences, Ideas for a Philosophy of the History of Mankind,* and shorter extracts from the *Letters for the Advancement of Humanity.*

[8] See Lucian W. Pye, *Aspects of Political Development* (Boston and Toronto: Little, Brown, 1966), p. 8.

objectives.[9] Coupled with this mode of thinking is the belief that the leading representatives of the world culture are inherently capable of responding to the diverse challenges attending rapid and continuous change.[10] Hence they are presented (and frequently accepted) as models worthy of emulation. To be sure, élites in emerging states are not altogether unaware of the conflicting pulls of socio-cultural distinctiveness on the one hand and modernization through the adoption of the world culture on the other. Yet even if it is sensed that the diffusion of the 'scientific world culture' might constitute a threat to indigenous cultures, such fears are often assuaged by identifying the world culture with purely 'material' elements incapable of impairing the 'spiritual' content of a society's traditional values. Underlying this desire to fuse traditional cultures with the new 'ideals' of scientific and material progress, is unfortunately, a great deal of confusion about what is to be confirmed and what negated, about how much adaptation is necessary and what form the fusion should take. As Lucian Pye rightly observes, "the seemingly easy formula of the distinction between 'technology' and 'culture' cannot satisfactorily resolve the profound problems of identity which are in fact its origins."[11] More often than not, therefore, chaos and bewilderment rather than a new social and political order have followed attempts to fuse modern scientific, technological and administrative practices with basic components of indigenous cultures.

Two crucial misunderstandings seem to be at the heart of this confusion and bewilderment: (1) a misconception of the notion of tradition and (2) an erroneous belief that 'spiritual' elements of culture are easily separable from 'material' elements. I propose to deal with the second misunderstanding first since it provides a convenient handle for contrasting the philosophy underlying it with Herder's conception of culture and also because the first misunderstanding is symptomatic of a more general and fundamental problem, that of the relationship between persistence and change, which is the subject of a later part of this paper.

The separation between material and spiritual elements in the dichotomous manner indicated parallels the distinction that is at times made between 'civilization' and 'culture'. According to this distinction, civilization is identified with the development of material conditions and man's growing control over natural forces, while culture is identified with man's spiritual development in which intellectual, moral, and artistic values are the prime concern. Perhaps Alfred Weber's analysis of this distinction comes closest to the culture diffuson 'theory' that has gained currency among leaders of transitional societies.

In his main work *Kulturgeschichte als Kultursoziologie* (1935) Weber classified the chief structural components of man's development as the social process, the civilization process, and the culture process. By 'social process' he understood the recurrence of certain societal sequences which, notwithstanding individual variations, reveal sufficient uniformity to provide the basis for a comparative study of social development. The succession from kinship organization to territorial groupings is cited as an example of a social process. The 'civilization process' consists in the accumulation of knowledge about techniques of controlling natural and material energies and resources. Weber considered the discovery of these techniques as a continuous and cumulative process susceptible to the generalizing methods of science and hence causally explicable. Such knowledge is transferable. The 'culture process,' on the other hand, is not transferable. Culture can be understood only by acknowledging the incomparability or uniqueness of each of its manifestations, since it derives from the creative spontaneity of man which is not susceptible to the generalizing methods of science. Causal laws, therefore, have no applicability to the realm of culture. Nor can one speak of developmental stages of culture, for culture does not follow a lineal order of development but occurs sporadically, defying the causal determinism operative in the realms of science and technology.[12]

In contrast to Weber, Herder rejected the

[9] See S. N. Eisenstadt, "Breakdowns of Modernization," *Economic Development and Cultural Change,* 12 (1964), 345–367 and *Modernization: Protest and Change* (Englewood Cliffs, N.J.: Prentice Hall, 1966), p. 40; and E. Shils, *Political Development in the New States* (New York: Humanities Press, 1964), pp. 7–8.

[10] Manfred Halpern, "The Rate and Costs of Political Development," *Annals of the American Academy of Political and Social Science,* 358 (1965), p. 22. See also Claude E. Welch, Jr., (ed.), *Political Modernization* (Belmont, Calif.: Wadsworth Publishing Co., 1967), pp. 319–382; Fred R. von der Mehden, *Politics of the Developing Nations* (Englewood Cliffs, N.J.: Prentice Hall, 1964), ch. VII; and Charles W. Anderson, Fred R. von der Mehden, and Crawford Young, *Issues of Political Development* (Englewood Cliffs, N.J.: Prentice Hall, 1967), pp. 220–235.

[11] Pye, *op. cit.,* p. 97.

[12] This is a revised and abridged version of my article on Alfred Weber in the *Encyclopedia of*

dualism between 'material' and 'nonmaterial' activity. It is one of his basic ontological principles that "nature has not put iron bars between what commonly is called material and immaterial. . . . The fundamental antithesis which is presumed to exist between them is entirely unfounded."[13] Not surprisingly, therefore, culture is viewed as an inseparable whole. Western science and technology are part and parcel of the Western cultural ethos rooted in its Judaic, Christian and Greek tradition. Artifacts are as much part of culture as ideas, beliefs and values. Formed to walk erect, man has his hands free to manipulate objects and is thus capable of making tools, weapons and objects of art.[14] Accordingly, 'culture' would seem to comprise all of man's activities: what he *does* and what he *thinks*. That so wide a connotation of the term threatens to make it meaningless appears to have been evident to Herder. In the very first pages of the *Ideas* he remarked that nothing is apt to be more elusive and indeterminate than this word.[15] He therefore tried to bring it into sharper focus. And while, in doing so, he cannot perhaps be said to have attained the "modern scientifically generalized concept of culture,"[16] his relativistic, pluralistic and process-centered approach undoubtedly did blaze the trail of modern socio-cultural inquiries. We shall now consider these strands in Herder's approach to culture, reserving the question of what constitutes the integrative element of a 'cultural whole' to a later stage.

III. CULTURAL RELATIVISM

At a time when it was common to distinguish cultured from uncultured nations, Herder's insistence upon culture as a universal phenomenon was a novel idea. There is no such thing, he flatly declared, as a people devoid of culture. Wherever men have lived together as a group over a period of time there is a culture. To be sure, there are differences, but these are differences of degree, not of kind. "The picture of nations," he wrote in the *Ideas*, "has infinite shades, changing with place and time. But as in all pictures, everything depends on the point of view or perspective from which we examine it."[17] Yet despite his recognition of culture as a universal phenomenon, Herder denied that the standards and goals of human development can be subsumed under any single or immutable universal. He treated with the utmost scorn his contemporaries' attempt to use the concept of European culture as a comparative criterion or as a universal standard of human values. "To apply such a standard," he wrote,

> is not just misleading, it is meaningless. For 'European culture' is a mere abstraction, an empty concept. Where does, or did, it actually exist in its entirety? In which nation? In which period? . . . Only a misanthrope could regard European culture as the universal condition of our species. The culture of *man* is not the culture of the *European;* it manifests itself according to place and time in *every* people.[18]

As early as 1769 Herder laid down what may be regarded as the basic premise of his cultural relativism. "Human nature under diverse climates is never wholly the same."[19] If, he asked, we can scarcely express the individuality of one man, determine what specifically distinguishes him and how he feels and experiences things, why should we presume to be able to survey "an ocean of entire nations, times and countries" at one glance, with two or three general categories, without even making the slightest effort to enter into the spirit of their thoughts and deeds? Who can compare the different forms of satisfaction perceived by men in different times and different worlds? Each man, each nation, each period, has its center of happiness within itself, just as every sphere has its center of gravity.[20]

This principle of relativity has far-reaching implications. Taken to its logical extreme, it denies not only any basis of comparison, but the very possibility of an explanation. If each culture and each generation carries within itself its own immanent validity, if we have to think of the world as being composed of uniquely different socio-cultural entities, each with its own peculiar pattern of development, its own inner dynamic growth, it is hard to see how any general theory or even taxonomy could validly be applied as a tool of explanation. And if it cannot, how can one ever explain or analyse a given culture? For surely we need at least some general concepts in order to see and convey meaning or significance in any complex of relationships.

Herder's way of meeting this criticism can be discerned from his epistemological treatise, the *Metakritik* (1799), a work intended as a critique of Kant's *Critique of Pure Reason*. One of the major principles he wished to establish

Philosophy (New York: Crowell, Collier and Macmillian, 1967), Vol. 8, pp. 281–282.

[13] VIII, 193 and 178.

[14] XIII, 137.

[15] XIII, 4.

[16] A. L. Kroeber and Clyde Kluckhohn, *Culture* (New York: Vintage Books, 1963), p. 286.

[17] XIII, 348.

[18] XVIII, 247–249.

[19] IV, 38.

[20] V, 502 and 509.

there is that no single thing can be known or understood outside the context of its inter-relationships.[21] Accordingly, before we can even start to analyse a thing we have to *identify* it within a given temporal and spatial situation. And to identify is to explore the characteristics that govern its contextual relationships which, in the case of social actions, essentially comprise the norms, values and customs that are operative in a particular society at a particular point in time. Facts, by themselves, are meaningless. A chronicle or array of unrelated facts is neither history, nor sociology, nor philosophy. To understand the meaning of a fact, say that of an action, therefore, we have to understand the position of an actor in a specific situation. And in order to be able to do this, we must attempt to relive the action, to feel ourselves into the situation surrounding it, taking into account the times and circumstances. This requires both empathy and analytical imagination. Only by a careful blending of contextual enquiry and imaginative interpretation can we avoid the pitfalls of either drawing glib analogies based on *a priori* general frameworks or of naive factual empiricism.[22]

It may rightly be argued that this reply does not adequately meet the logical criticism of Herder's cultural relativism. For while contextual analysis may enable us to discover what the characteristics of a given situation are, the determination of their significance, and hence at least part of their explanation, hinges on the possibility of drawing comparisons, however implicitly, with apparently comparable situations that are more familiar to us. But the heuristic intent of Herder's emphasis on relativism and contextual understanding is evident enough. It is meant to alert us against the danger of assuming that similarity between two situations with a number of characteristics in common necessarily implies that other, and possibly more significant ones, are also alike. This danger is particularly real in metaphorical or analogical reasoning. For while the function of a metaphor is to express similarity rather than identity, in practice the degree of similarity implied by a metaphor has a wide range of variation, and this may easily cause attention to be withdrawn from unique attributes that may prove on occasion to be the most decisive. Since analogies, once chosen, have the peculiar tendency of acquiring credibility through repeated use, the risks of faulty understanding and hence of empty explanations are compounded still further.[23]

[21] XXI, 179.

[22] XIII, 290 and V, 504.

[23] See M. Landau, "On the Use of Metaphor in Political Analysis," *Social Research,* 28 (1961), 353; F. M. Barnard, "Metaphors, Laments and the Organic Community," *op. cit.,* pp. 281–285, and A. James Gregor, "Political Science and the Uses of Functional Analysis," this REVIEW, 62 (1968), 425–439.

However, Herder's cultural relativism need not be carried to such logical extremes that it wholly inhibits comparison and generalization. Herder's own historical and sociological thinking, in particular his treatment of causation, certainly does not suggest this. He would therefore have scarcely subscribed to Alfred Weber's view that causal laws have no applicability to the realm of culture. To be sure, Herder did distinguish between the mode of causation operative in nature and that applicable to human action. Yet at no time was he prepared to deny the feasibility of sociological explanations in terms of law-like propositions.[24] In short, by upholding cultural relativism as a methodological principle, Herder primarily wished to stress that comparative and generalizing treatments of social cultures must not be simplistic in terms of an arbitrary taxonomy or a preconceived universal (though invariably ethnocentric) criterion, but rather that they must be sensitive to diversity, to differences no less than similarities. To this end, no effort should be spared to study each culture in terms of its own particular pattern or structure.

Before turning to the 'substantive' implications of Herder's cultural relativism at least a passing mention of a common objection to cultural relativism—that it entails, if not condones, ethical nihilism—would seem in order. Frequently such criticism involves confusing universals with absolutes. Now Herder indeed questioned the wisdom of approaching or judging cultures in terms of a single immutable standard of value. But he would have dismissed the idea that one value was as good as another as a vulgarized distortion of cultural relativity. Just as he did not wish to imply that cultures are in all respects utterly disparate and incomparable entities, so he likewise had no desire to impugn the authenticity of universals as such, properly un-

[24] For a more detailed discussion of Herder's approach to causation and determinism, see G. A. Wells, *Herder and After* (The Hague: Mouton, 1959), pp. 262–269; F. M. Barnard, "Herder's Treatment of Causation and Continuity in History," *op. cit.,* pp. 197–212; and *idem, Herder's Social and Political Thought, op. cit.,* pp. 51–53. See also Lewis W. Spitz, "Natural Law and the Theory of History," *Journal of the History of Ideas,* 16 (1955), 453–475, for a most penetrating study of Herder's conception of historical laws.

derstood and concretely perceived "within the actuality of the particular"—to borrow Ernst Cassirer's apt phrase.[25] Indeed, his philosophy of *Humanität* is an eloquent affirmation of the hope that ethical norms will assume the status of meaningful universals, in terms of which men will come to recognize the standards of mutuality to which they ought to aspire as human beings.[26] But the realization of this hope seemed to him contingent upon, rather than contrary to, the recognition of their temporal and spatial relativity. For in order to be operative, ethical universals have to be believed and cherished, and this is plainly inconceivable, he felt, in abstraction of time and place. Thus Herder's admission of the relativity and plurality of values must be seen principally as a challenge to that myopic form of universalism which views every variant as a deviant, and as a corrective to the tempting desire to underrate the problematic tension that characterizes the relation between diverse human goals.

An approach which so markedly highlights diversity and the immanent distinctiveness, if not uniqueness, of socio-cultural entities cannot fail to have a bearing upon problems of nation building and political development generally. Indeed it could be said to suggest a particular archetype or paradigm of development, that of *internal* development or growth. For while Herder did not deny that external factors can and do affect the distinctive culture configuration of social entities, promoting or inhibiting it, he saw the essential driving power as coming from within.[27] The essence of development is fulfillment, the actualization of that which is latent or germinal. This way of viewing social and political development invites us to respect the distinctive configuration of each sociopolitical whole or system that lays claim to its separate existence in terms of criteria of its own making, however particularistic these might seem in terms of other, more or less common, collectivity symbols or boundary determinants. Any attempt, therefore, to come to grips with the political sensibilities of new, but also of not quite so new, nations, can scarcely dispense with the perspective afforded by the paradigm of internal development and the principle of cultural relativism. There is enough evidence to refute the belief that emergent nations take kindly to the idea of embracing lock, stock and barrel, political systems that are urged, if not imposed, on them from outside.

This is not to deny the possibility, or even desirability, of cultural diffusion, nor to suggest that environmental geopolitical and economic pressures can be dismissed as irrelevant. What Herder's paradigm of internal development and his emphasis on cultural relativism, however, make amply clear is the need to see cultural diffusion as an *historical* process, as something that takes time and something that is not mono-dimensional, unproblematic, or without its inner sources of tension.

Anthropologists, of course, have realized this all along; yet policy-makers in both the Communist and non-Communist world have been rather slow in acknowledging that what has become known as 'polycentrism' is a trend not confined to socialist countries wishing to travel their own road to a given universalist goal. Nationalism may be one of the most persistent features of the western 'world culture' but it also embodies 'primordial sentiments,' not unknown in pre-western or non-western cultures, in favor of particularistic distinctiveness.[28] In short, the conflicting pulls of particularistic and universalistic tendencies are not new in the history of political development. It may well be that parochial or particularistic tendencies within diverse political cultures receive too little rather than too much attention by governmental elites in their zeal for rapid economic development. That this zeal is often frustrated as the result of an inadequate recognition of the problems entailed in cultural diffusion, in particular the problem of reconciling external with indigenous cultures, has been the burden of argument so far. The inability, or reluctance, to conceive of political development as a problem *sui generis*, as something

[25] Ernst Cassirer, *The Logic of the Humanities* (New Haven: Yale University Press, 1961), p. 25.

[26] F. M. Barnard, *Herder's Social and Political Thought, op. cit.*, ch. 5, esp. p. 98.

[27] To this inner driving power Herder applied the term *Kraft*. It signifies the prime source of all that is alive and the dynamic force of its continued existence. See his *Ideas for a Philosophy of the History of Mankind* (1784–91), esp. XIII, 172–188 and 274–277. For a fuller discussion of the notion of *Kraft* see Robert T. Clark, Jr., "Herder's Conception of Kraft", *Publications of the Modern Language Association of America*, 57 (1942), 737–752 and F. M. Barnard, *Herder's Social and Political Thought, op. cit.*, pp. 38–39 and 44–53 where Herder's views are compared with those of Rousseau, Locke, Spinoza and Leibniz.

[28] See, for example, Edward Shils, "Primordial, Personal, Sacred and Civil Ties," *British Journal of Sociology*, 8 (1957), 130–145; Clifford Geertz, "The Integrative Revolution: Primordial Sentiments and Civic Politics in the New States," in Welch, *Political Modernization, op. cit.*, pp. 167–188; and David E. Apter (ed.), *Ideology and Discontent* (Glencoe: Free Press, 1964), p. 22.

which, though related with, is also different from economic or other forms of development is another and no less important, reason for the frustration and disenchantment that has beset nation builders throughout the world. I now propose to inquire to what extent Herder's pluralist conception of culture might help to shed some light in this direction.

IV. CULTURE AS A COMPLEX

When Herder thought of culture as an integral whole, he conceived this 'whole' not as a homogeneous or unstructured mass, but as a composite or complex *configuration* which, by virtue of its inherent relational characteristics, is something more than a mere sum total or aggregate. In an aggregate the parts are separate and unrelated, and their number can be increased or reduced without this having any effect on the *nature* of the total but merely on its *size*. A whole, on the other hand, is something more than the sum of its constituent parts. This 'more' is not contained in the parts considered in isolation, but rather arises from their interrelation and the varying degree of their integration. Herder contrasted the holism characterizing culture with the atomism characterizing an aggregate, by comparing the former with the unity of an organism. In so doing he wished to focus on two crucial qualities: functional interrelatedness and self-generated activity.

The coherence of a culture, like that of an organism, is held to be contingent upon relations that arise from, but at the same time govern, the degree of reciprocal interaction between the constituent parts, where the parts (unlike those of a mechanical assembly) are activated by their *own* source of energy. In other words, the distinguishing characteristic governing culture viewed as a complex whole is the *intrinsic* creativity of its inter-related parts. And in order to underline its *composite* nature, Herder spoke in terms not of a single organism but of a complex of organisms. Thus, like St. Thomas's 'accidental whole' (as distinct from the substantial whole of a physical body) or Aristotle's concept of the state as a composite, Herder's holism comprises multiplicity as well as unity. And though each of the component parts is considered as an active whole in itself, comparable to the Leibnizian monad, it also differs from it in that the Herderian monad *has* windows: it is not wholly self-contained but interconnected.[29]

Herder's organismic conception of culture as a complex whole posits a further characteristic, which indeed is implicit in his philosophy of cultural relativism: diversity. The notion that the world consists in nature, no less than in society, of inter-related and interacting processes, each of which pertains to the whole in its most distinct individuality, is the principal theme of Herder's organismic cosmology and sociology and one of the central theses of his philosophy of history.[30] Multiplicity is but a corollary of the postulate of diversity and the unity characterizing Herder's brand of holism, therefore, is a unity in and through diversity. To be sure, Herder was sufficiently a child of the Enlightenment to hazard the belief that amidst infinite diversity there was a tendency "striving for unity which lies in all things and which urges all things forward."[31] But he was also sufficiently a critic of the Enlightenment not to identify this tendency with continuous, linear progress.[32] Thus while he maintained that diversity united by creating the conditions of reciprocal activity, he also recognized and indeed emphasized that the plurality of values did not rule out their incompatibility. Tensions and conflicts must be accepted as the price of diversity. Incongruities and contradictions are not mere accidents; they are of the very essence of the human condition and so cannot be divorced from social and political development. "A nation," he writes in *Yet Another Philosophy of History* (1774)

> may have the most sublime virtues in some respects and blemishes in others, show irregularities and reveal the most astonishing contradictions and incongruities. These will be all the more startling to anyone carrying within himself an idealized shadow-image of virtue according to the manual of his century, one so filled with philosophy that he expects to find the whole universe in a grain of sand. But for him who wants to understand the human heart within living circumstances, such irregularities and contradictions are perfectly human.[33]

It follows that to speak of a cultural whole, or the unity of a social or political culture, is not necessarily a way of referring to a state of blissful harmony; it may just as conceivably refer to

[29] The most systematic exposition of Herder's philosophy of organicism is to be found in his *Metakritik;* see esp. XXI, 152–182.

[30] See *Ideas*, esp. XIV, 83–84.

[31] XIII, 346; see also XIV, 227; XVI, 119, 551; XVII, 116; XVIII, 302, 408; XXIV, 375; and XXIX, 133, 139. This faith in the attainability of harmony between conflicting interests is by no means absent from modern pluralistic ideology.

[32] Undoubtedly the most vigorous expression of Herder's repudiation of the idea of linear progress is to be found in *Yet Another Philosophy of History;* see esp. V, 527, 559, 564.

[33] V, 506.

a field of tension. This is borne out also by Herder's process-oriented conception of culture, about which more will be said later. According to it, culture is seen both as the product of the past and as the creator of the future. A cultural whole, therefore, it not a 'thing' at all, but a relational *event*. It is a continuum which, moreover, is not unilinear but subject to an inherent dialectic entailing preservation *and* innovation. Within such a continuum 'unity' is not a state but a *tendency,* a potentiality seeking realization, not a necessary actuality.

It will be evident by now that Herder understood by culture a *relational* attribute pertaining to a group of individuals rather than to individuals in isolation, and that it involves a *two-dimensional* relationship. The mode of causality said to be operative in this relationship is depicted rather vividly in a formulation dating back to one of Herder's earliest works, the *Fragments* (1767):

> A series of causes act together, reciprocally and in succession; one wheel interlocks with the other, one spring acts upon the other.[34]

The formulation indicates an analytic distinction between two *types* of processes which, empirically, are closely interwoven. In one type of process the emphasis is on inter-relations and interactions occurring *at any given time* between culture constituents, which may be described as the 'horizontal' dimension, whereas in the second type of process the emphasis is on inter-relations and interactions occurring in *succession over time,* which may be described as the 'vertical' dimension. This distinction is, I believe, analytically useful in that it helps to point up the principal characteristics of situational and process analysis.

Of these two interrelated dimensions each is concerned with the tracing of causal connections: the horizontal dimension aims at the explanation of social action in terms of 'givens' operative at a certain time; the vertical dimension aims at the evaluation of its purposive direction in terms of both situational *origins* and situation-affecting *development*. I am aware that this distinction does not substantially diminish the problems attending the selection of relevant 'givens' or the determination of decisive situation-affecting development factors. Nonetheless I believe that the distinction between 'cause' as a situational given and 'cause' as a directional purpose is important in the treatment of human affairs. For by drawing this distinction it seems perfectly admissible to uphold the idea of causal theorizing without committing oneself to viewing any particular action as inevitable.

In the remaining part of this section I shall briefly illustrate Herder's conception of culture as a *complex* of segmental cultures where the horizontal dimension is chiefly involved, returning to the vertical dimension in the subsequent section. A closer analysis of the concept of interaction itself will be postponed until we discuss Herder's approach to political culture.

In view of the co-existence at any given time of diverse social and political cultures, Herder felt that it would be more accurate to speak of specific cultures (in the plural) rather than of culture in general. This would, he thought, help to reduce the inherent ambiguity of the concept and at the same time direct attention to the distinctive culture patterns arising from social, psychological, religious, economic, political or geographic differences as well as from diverse historical traditions. Although he was mainly concerned with eliciting sources of socio-political cohesion, i.e., the *integrative* elements of a particular socio-political culture such as a nation, he recognized that there are diverse cultures existing side by side even within any one nation, owing to differences in occupation, education, religion, social birth, private wealth and so on. He distinguished, for example, between the culture of intellectuals and the culture of craftsmen and traders.[35] The reason he thought of these co-existing cultures as sub-cultures, or more precisely, as *segmental* cultures (owing to his pluralistic and non-hierarchical orientation) is that he considered the differences between, say, occupations within one nation, to be less fundamental or significant than those existing between the same occupations among different nations. Herder's careful formulation on this point merits, I think, quoting in full. Its methodological import has scarcely been out-dated.

> It is customary to divide the nations of the world into hunters, fishermen, shepherds, and farmers, not only to determine accordingly their level of cultural development, but also to suggest that culture as such is a necessary corollary of a given occupation or mode of life. This would be most admirable, provided the diverse modes of life were defined in the first place. Since these, however, vary with almost every region and for the most part overlap, it is exceedingly difficult to apply such a classification with accuracy. The Greenlander who harpoons the whale, hunts the reindeer and kills the seal, is engaged in both hunting and fishing, yet in quite a different manner from that of the Negro fisher or the Arancoan hunter. The Bedouin

[34] II, 65.

[35] XIV, 34–35.

and the Mongol, the Lapp and the Peruvian are shepherds; but how greatly do they differ from each other, with one pasturing camels, the other horses, the third reindeer, and the last alpacas and llamas. The farmers of Whidoh are as unlike those of Japan as the merchants of England are to those of China.[36]

Of particular interest to our purpose is Herder's realization that segmental cultures can develop at different rates and that, in doing so, they may result in acute imbalance capable of threatening the cohesion or 'wholeness' of a society. For this realization highlights the point made earlier regarding the unilinear development fallacy. Once again this point has not lost relevance. Not many social theorists today, it is true, share their nineteenth-century precursors' faith in unilinear progress. Yet seemingly this does not prevent some eminent sociologists and economists from theorizing about political development in a way which clearly suggests that they are correlating progress in one direction, say in the consumption of telephones, automobiles, or bathtubs, with progress in another, say in achieving stable democracy.[37] And only fairly recently have Marxist policy-makers begun to realize that the exclusive emphasis on highly centralized and hierarchical economic structures did not necessarily entail *pari passu* the disappearance of the state or the attainment of other cherished political goals.[38]

To put the matter differently, what Herder reiterates here is the possible clash and conflict between interacting cultures and co-existing forms of development. The horizontal dimension of interaction, that is to say, may involve areas of tension no less than spheres of co-operation. One crucial consideration concerning horizontal interaction, therefore, is the degree of balance between segmental cultures. Significantly, Herder remarked to this effect that it is not the *number* of cultural divergences, nor the *progress* achieved in this or that direction that is decisive for the social and political cohesion of a collectivity, but the *balanced relationship* of its constituent cultures.[39] When certain forms of culture are widely out of step with other forms, in particular if 'techniques' and 'values' markedly diverge, alienation results.[40]

In this connection we may also note Herder's interesting treatment of the relation between co-existing 'ideas' and 'institutions'. For the tendency to view the former as epiphenomenal, as mere superstructures of the latter, is still widely prevalent. Certain myths, for example, may co-exist and, indeed, be intimately associated with particular religious or political institutions. But this does not prove anything about their respective origins or significance, nor does it deny their essential autonomy. By the same token, whatever 'functions' either of them may be said to perform within a given 'system' proves nothing about their necessary or even sufficient inter-relationship. Myths and religions may or may not serve the function of maintaining authoritarian (religious and/or political) structures, but this is not tantamount to saying that the latter would necessarily disappear with the disappearance of the former (or vice versa), or that shamans, priests, or dictators invented certain beliefs, or again that they deliberately used such beliefs to deceive people. "By dismissing them as cheats," Herder wrote,

one is inclined to think that one has explained everything. They may well have been cheats in many or most places, but this should not induce

[36] XIII, 310.

[37] See, for example, S. M. Lipset, *Political Man* (New York: Doubleday, 1959, Anchor ed., 1963), pp. 31–37. If European stable democracies are characterized by the ownership of 43–400 telephones per 1,000 persons and European dictatorships by 7–196 telephones per thousand, one wonders how those nations whose population possesses 43–195 telephones manage to remain stable democracies. See also, on this point, Phillips Cutright, "National Political Development" in Polsby, Dentler and Smith (eds.), *Politics and Social Life* (Boston: Houghton, Mifflin, 1963), pp. 569–582, Joseph La Palombara, "Decline of Ideology: a Dissent and an Interpretation", this REVIEW, 60 (1966), 8; and Deane E. Neubauer, "Some Conditions of Democracy", this REVIEW, 61 (1967), 1002–1009. If one prefers automotive production to bathtubs as an indicator of democratic development, W. W. Rostow, *The Stages of Economic Growth: a Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1960), or his more recent "Unsolved Problems of International Development", *International Development Review*, 7 (1965) no. 4, 15–18, and 8 (1966) no. 1, 2–5, will prove instructive. See also Daniel Lerner, *The Passing of Traditional Society*, (New York: Macmillan, 1958) and Max F. Millikan and Donald L. M. Blackmer, *The Emerging Nations* (Boston: Little, Brown, 1961).

[38] Milos Samardzija, "The Yugoslav Economic System and Contemporary Socialist Economic Theory" in J. M. Porter (ed.), *Contemporary Developments in the Theory and Practice of Communism* (Springfield, Mo.: Drury College, 1967), pp. 28–38.

[39] XIV, 149.

[40] XIII, 371.

us to forget that they themselves were people too, and the dupes of myths older than themselves.[41]

Any attempt, therefore, to explore the origins or significance of a particular culture, be it values, ideas, institutions or artifacts, can scarcely dispense with process analysis. The horizontal dimension of interaction, which dominates situational-functional analysis, taken by itself is plainly inadequate as an explanatory tool if what we seek in terms of an explanation is content evaluation and/or purpose determination. Thus, far from being inconsistent, functional and historical approaches are indeed complementary, one could almost say interdependent. Moreover, Herder's heuristic principle of treating every manifestation of culture as something which, though interrelated in the two-dimensional sense indicated, is essentially autonomous, also enjoins us to view the mode of causality that is applicable to the culture process in terms of multiple causation. Now, both the idea of two-dimensional interaction and the idea of multiple causation are merely perspectives, conceptual categories. As such, we may reiterate, they provide no guarantee of obtaining correct answers, or indeed, of even asking the right questions. Yet, without them, it is hard to see how any questions could be raised at all.

V. CULTURE AND PROCESS

Two inter-related concepts characterized Herder's process-oriented approach to culture: *Bildung* and *Tradition*. Both words were used by him in their original dynamic sense of 'becoming' or 'building up' and of 'passing on' respectively, and not in their better-known, acquired sense.

Thus *Bildung* is not identified with a particular *state* of development nor confined in its connotation to intellectual or strictly individual pursuits as was (and still is) customary in German literature (and everyday use).[42] Instead it is viewed as an interactive social process in which men receive from and add to their distinctive heritage. This interactive and reciprocal building up of societal values (including knowledge and skills) represents for Herder the true purpose of human association. The modern concept of 'socialization' comes, perhaps, closest to his interpretation of *Bildung*. It is important, moreover, to note that Herder conferred upon the concept not only a dynamic denotation, but also a distinctly dialectic meaning. Man, by virtue of his capacity for speech, perceives nature not directly but through the mediation of ideas. By means of ideas the 'imitator' (or 'internalizer') has powers to receive and convert into his own nature what has been transmitted to him, just like the food he eats. What and how much he receives, where he derives it from, and how he applies it to his own use, is no doubt in part contingent on environmental factors; but largely it is determined by the learner's own receptive and creative powers. In view of this, Herder spoke of the process as a continuous genesis, investing it with both 'genetic' and 'organic' attributes. It is genetic, by virtue of the inter-generational manner of transmission, and organic, by virtue of the creative manner in which that which is being transmitted is assimilated and reapplied.[43]

Bildung, then, is a process not only of assimilation, but also of *evaluation*. Man both collects impressions and also incessantly evaluates the supply collected, and thus he not only receives stimuli but at the same time cumulatively and creatively converts them in his own peculiar manner.

> One activity is increased by another; builds upon, or evolves from, the foregoing. . . . We are always in motion, restless and dissatisfied. The essence of human life is never fruition, but continuous becoming. . . . The bee, on the other hand, was a perfect bee when building her first cell.[44]

Clearly, therefore, *Bildung*, thus understood, is not simply a replicative process; it is not socialization conceived solely or chiefly in terms of 'system maintenance'. On the contrary, Herder saw in it the only alternative to abrupt socio-

[41] XIII, 307.

[42] Of German contemporaries, Goethe perhaps comes closest to Herder's conception of *Bildung*, although he views the process in wholly apolitical and individualist terms, as 'self-improvement.' (See W. H. Bruford, *Culture and Society in Classical Weimar, op. cit.*, p. 236). Herder's faith in the power of *Bildung* to engender in a people the will and ability to govern itself reflects, of course, the influence of Rousseau's educational gospel which in like manner came to dominate Jefferson's democratic creed.

[43] XIII, 343–348; see also XIV, 84. Lest these terms are interpreted in a biological, as distinct from a socio-cultural sense, Herder—much to the chagrin of Kant—goes out of his way to assure us in the *Ideas* that he is not thinking of biological or 'racial' heredity. In fact he bluntly declares that he has no use for the concept of race. (XIII, 257–258). Regarding Kant's criticism, see his review of the *Ideas* in the *Allgemeine Literaturzeitung* of 15 November, 1785. Surprisingly enough, Herder has been presented as a racist by some commentators.

[44] V, 98; see also XIII, 182.

cultural discontinuities attending the replacement of societal values through their prior destruction rather than their transformation.[45]

Tradition, likewise, is not identified with a stock of accumulated beliefs, customs and ways of doing things, but with an ongoing process of inter-generational transmission which, like *Bildung,* entails by its very nature the continuous merging of the old and the new. Herder was aware, however, that the merging of the old and the new involves in its operation both affirmative *and* negative properties, and that change, therefore, if it is to mean the coming into being of new and specifically different constellations and not merely the replacement of one set of conditions by a new set of the same specific type, is not tantamount to a smooth advance or progress. Tension may indeed be the rule governing this innovative process, rather than the exception. Every discovery in the arts and sciences, in technology and industry, he wrote in the *Ideas,* knits a new pattern of society. New situations create new problems, and every increase in wants (even if they are satisfied) is not necessarily conducive to an extension of human happiness.[46]

At the same time Herder refused to view stages of development in a dichotomous manner. Although he was one of the first of modern thinkers to make use of organismic and mechanistic typologies, he did so for essentially 'ideological' reasons, employing them as sociological props for his community model.[47] One reason for his refusal to apply such dichotomies as traditional and modern, primitive and civilized, static and dynamic, sacred and secular, was his cultural relativism. The other, and perhaps even more significant reason was that he found these dichotomies inherently unhistorical and incompatible with the conception of development as an interacting, dialectic process of change. Hence in place of the idea of polarity, Herder advanced the idea of *interplay.* Tradition and progress no longer embody two opposed entities, but a single continuum. Progress, or more precisely, change, becomes a built-in characteristic of tradition. And since the inter-generational process of transmission is held to involve the fusing of both negation and affirmation in a manner analogous to that described with reference to *Bildung,* tradition, too, entails its own immanent dialectic. Change or development, therefore, is at once part of a given culture continuum *and* the instrument for its transformation. If tradition fails to grow it becomes warped and, as such, constitutes "a real opium of the spirit and the death-knell of states and individuals alike."[48] Healthy social development, then, requires not only historical antecedents but also emerging goals pointing to the future.

Since *Bildung* and *Tradition* reveal so close a similarity in their mode of operation, and since Herder also considered them inextricably linked, why did he bother to distinguish between them? Herder gave no explicit answer to this question. It appears, however, reasonable to assume that he found the distinction useful in separating the process of internalization as such from the social context of communication, association, and institutionalization, in and through which it takes place. These two processes entail, as it were, 'culture' as both a product and an emergent force at any given time, in so far as *Bildung* leads to shared patterns of social behavior that have become 'patterns' by virtue of a greater or lesser degree of institutionalization through *Tradition.*

There is a tendency towards indeterminacy, if not circularity in Herder's process analysis of culture. For it would seem that 'cause' as a directional determinant (emerging goals) could have a decisive bearing on 'cause' as a given or temporal antecedent. If actual consequences would always, or at least in most instances, match intended consequences or goals, this should occasion no concern. But this is an unlikely prospect, especially in 'developing' societies where the directional determinant will be predisposed towards drastic change and innovation, injecting into the existing situation a highly incalculable element. On the other hand, this very circumstance helps to underline the distinction made earlier between causality and inexorable determinism or that between explanation and prediction in regard to the culture process. It also tends to militate against 'interaction' being conceived as a hopelessly circular mode of operation.

What is also worth noting is that Herder admitted of conflict and tension as potentially inherent in the processes of *Bildung* and *Tradition,* yet categorically denied the possibility of *complete* discontinuity within any given culture. In this he revealed considerable astuteness. For it is difficult to see how one can speak of 'development' in terms of complete or total change

[45] In view of this, Herder, though hailing the French Revolution as the most momentous event since the Reformation, could not help feeling that it was a regrettable alternative to *Bildung* as he conceived it. See esp. XVIII, 313, 314–320 and 331–332.

[46] XIII, 372–373.

[47] On this point see F. M. Barnard, "Metaphors, Laments and the Organic Community", *op. cit.,* pp. 281–301.

[48] XIV, 89.

without raising serious problems of identity. When a culture or a 'system' is said to have changed completely, what exactly *is* being said? How can one identify a system as still being in some respects the same system if it has undergone complete change?

But undoubtedly Herder's most original idea in this context lies in his insistence upon the essential oneness of the problem of persistence and the problem of change. His analysis of socialization as a non-replicative process and his interpretation of tradition as a dialectic continuum clearly suggest that any attempt to explain change must entail a recognition of persistence, or vice versa. A theory which cannot account for both is therefore unlikely to account for either. This realization is not an obvious commonplace in our own day. It was appreciably less so when Herder wrote.[49]

VI. INTEGRATIVE ELEMENTS: POLITICAL CULTURE

Herder did not explicitly offer a definition of culture, and it almost seems that this was deliberate. At the same time we cannot complain of a dearth of indications of what, essentially, he meant by it. The characteristics that emerge as basic to his scheme of thought can perhaps best be summarized as: relativism, pluralism, historism, a non-deterministic form of causalism, and a type of holism which embraces diversity as well as unity. Within the conceptual framework formed by these categories, culture—we found—is presented as the relational attribute of a social group, indeed as the very hallmark of its existence as a group, in that it is the distinctive quality which is at once the product and the source of its shared activities, ideas, values, artifacts and process, in short, its shared way of life over an inter-generational period of time. Though complex in its composition, it is nonetheless held to constitute an integral whole by virtue of one or more determinants that act as cohesive agents.

Herder was particularly concerned with determinants that help to produce a sense of collective identity in terms of which men come to recognize not only what unites them with, but also what distinguishes them from, others. Hence much that is relevant to social culture is also relevant to political culture understood in the broader sense of social organization, shared symbols and values, customs and norms of reciprocity. Indeed we shall see how firmly political culture in the narrower sense is embedded, for Herder, in the broader socio-political culture, with which it is said to interact. Yet despite his stress on interaction and on viewing political culture as a dimension of social culture, Herder makes an analytical distinction between levels of political culture which approximates to David Easton's system categories of political community, regime and government or "authorities".[50] I am not suggesting, of course, an identity of nomenclature or explicit treatment; moreover, Herder's distinction between 'regime' and 'government' is not merely analytic but also real. Nonetheless, the similarity of analytical insight seems close enough to warrant the adducing of Easton's more familiar terms. They should, I believe, prove illuminating rather than misleading. Let us roughly equate, then, Herder's 'political culture in the wider sense' with Easton's 'political community' and survey briefly the range of determinants that he deemed pertinent to both its emergence and persistence.

Among external determinants, physical environmental factors—to which Herder applied the generic term of 'climate' (*Klima*)—are considered of secondary importance. Climate, he said, can only influence, favorably or unfavorably, but it cannot compel a given course of development.[51] The *social* environment, on the other hand, is quite another matter. Indeed, external social conflict and hostility—and here Herder anticipated Georg Simmel—are posited as the most potent agents in arousing a sense of collective identity and solidarity. "The more a group is threatened", he remarked to this effect, "the more it will turn in upon itself and the closer will be the ties of its members."[52] But the degree to which external conflict can perform a positive function in strengthening the cohesion of collectivities is made contingent upon the existence, or at least the potential existence, of integrating agents *within* a social group.

It is on internal determinants, therefore, that, fundamentally, Herder placed the greatest emphasis. A group threatened by hostile forces may or may not succeed in repelling the aggressor, but it will not even act as a group in the

[49] For modern critiques of replicative socialization see, for example, Anthony F. C. Wallace, *Culture and Personality* (New York: Random House, 1961), esp. pp. 84, 111–119; and David Easton, "The Theoretical Relevance of Political Socialization", *Canadian Journal of Political Science,* I (1968), 125–146.

[50] See David Easton, "An Approach to the Analysis of Political Systems", *World Politics,* 9 (1957), 383–400; see also *idem,* "Political Anthropology", *op. cit.,* pp. 228–229 and *A Systems Analysis of Political Life* (New York: John Wiley, 1965) pp. 171–219.

[51] XIII, 273.

[52] V, 141.

first place unless there are some actual or potential foci around which individual members can rally and in terms of which they can see themselves as part of a larger whole. It is interesting, in this connection, both from the point of view of modern sociology and psychology and also against the historical background of the 'age of reason,' to note Herder's recognition of the crucial significance of non-rational elements as molding agents of social and political cultures, that is to say of sentiments that defy 'logical' analysis and/or 'empirical' verification. Neither myths nor prejudices are dismissed as irrelevant aberrations. And very much in contrast to the cosmopolitan thinking of the Enlightenment, particularism (or even 'parochialism') is given right of place in the process of socio-political self-identification which Herder regards as the *sine qua non* of eventual *inter*nationalism. "If, in this development of particular national tendencies towards distinctive forms of collective happiness", he declared in *Yet Another Philosophy of History,*

> the distance between existing nations grows too great, we find prejudices arising. . . . Similarly, prejudices, mob judgment, and narrow nationalism, arise when the dispositions and spheres of happiness of two nations collide.[53]

But prejudice, he added, is good, in its time and place, for "self-esteem and happiness may spring from it. It urges peoples to converge upon their center and attaches them more firmly to their roots."[54] Herder regretted that so little had been done to explore the intermixture of notions that are rationally analyzable and/or empirically verifiable with those that are neither, yet equally relevant molding agents of social and political cultures. Folklore, myths, folksongs, rituals and customs are, he felt, vital clues to a people's collective 'personality', its social or political sense of identity.[55]

Language plays another, if not *the* pivotal role in Herder's anatomy of political culture in its broader meaning. As for Humboldt, and more recently, Sapir, Wittgenstein, and Peter Winch, language constituted for Herder the inescapable bedrock of social life.[56] For it is both the medium through which man becomes conscious of his inner self *and* the key to the understanding of his outer relationships, of the world in which he lives. It also links him with the past. It is through language that man is able to enter into communion with the way of thinking and feeling of his progenitors, to take part, as it were, in the workings of the ancestral mind. Language, therefore, is a living link in the chain of tradition. This is so, Herder significantly remarked in his highly original essay *On the Origin of Language* (1772), because words do not merely describe or classify objects; they also transmit feelings, and they do this not only explicitly but also implicitly via emotional overtones which they acquire in the process of transmission. Particularly words we learn during infancy and adolescence carry secondary associations which are re-kindled in our minds whenever we use them. And frequently it is these associations which sway the mind more powerfully than the main concepts with which they were originally linked. These "companions of the dawn of life", as Herder called them, permeate social cultures far more than we are wont to recognize, and may well prove, he caustically added, more decisive than "all political contracts and systems devised by the philosopher," in that they vitally determine the distinctive character of family, kinship or national modes of thought.[57] The question, therefore, is not solely, or very often chiefly, "what a word can mean according to a few dictionaries," he already observed in the *Travel Diary,* "but what it means in the consciousness of living people—here, now, in all its capriciousness."[58] Here again Herder regretted that the origin and significance of turns of phrase had not been suffi-

[53] V, 510.

[54] *Ibid.*

[55] Herder's early interest in this direction is evident from his highly perceptive observations in the *Travel Diary* of his voyage from Riga to France in 1769, which was not published during his lifetime. He resolved there to collect data from the mythology of all ages and cultures and "to examine everything from the point of view of politics" (IV, 363–364), which in fact he subsequently did in his pioneering collection of folksongs from the most diverse and almost forgotten nationalities, and in other works.

[56] Peter Winch's close similarity to Herder's ideas on this point was brought to my attention by my colleague, Dr. J. M. Porter. Winch writes: "Our idea of what belongs to the realm of reality is given for us in the language that we use. The concepts we have settle for us the form of the experience we have of the world. . . . There is no way of getting outside the concepts in terms of which we think of the world. . . The world *is* for us what is presented through those concepts. That is not to say that our concepts may not change; but when they do, that means that our concept of the world has changed too." (Peter Winch, *The Idea of a Social Science,* London: Routledge, 1958, p. 15).

[57] V, 117–136.

[58] IV, 423.

ciently studied from the sociological and political point of view.[59]

Language, then, embodied for Herder the living manifestation of historical continuity and the psychological matrix in which man's awareness of his distinctive social heritage is aroused and deepened. A nation may lose its state and political independence, but it will survive as a national community if it succeeds in preserving its distinctive linguistic traditions.[60] On this view it is through language that the individual becomes at once aware of his selfhood *and* of his membership of a collective culture. Such close identification between the private and public self unmistakably reveals the influence of Rousseau's philosophy of the general will; yet at the same time it also signals its transcendence. For by substituting a 'common language' for the 'general will' Herder stipulated an empirical and historically derived basis for socio-political association in place of a metaphysical and moral absolute. What is more, by advancing such an empirical basis, he also hoped to render the concepts of both sovereignty and contract superfluous. This seemed essential to him in order to present the idea of 'political culture'—he explicitly used the term in the *Ideas*[61]—as a *postulate* rather than merely as a concomitant of that level of a political system which Easton designates as 'regime.' In a sense, this line of thought is a logical corollary of Herder's paradigm of internal development, according to which only that is capable of actualization which is already latent or germinal. On the other hand, Herder was realistic enough to recognize—albeit rather reluctantly at times—that political culture is not a wholly natural growth, not something that just happens, but rather a carefully nurtured 'plant.' Nation-building, in short, *is* an act of will, even though the 'will' Herder had in mind has little or nothing in common with either Rousseau's transcendentalism or Hobbes' Leviathanism.

We finally have to enquire, therefore, what Herder meant by 'political culture' in the narrower sense of Easton's 'regime', and how he conceived of political development *sui generis*.

Regarding Herder's conception of political culture proper, the most succinct formulation can be found in his treatise *On the Reciprocal Influence of Government and the Sciences*, a work that, unfortunately, has been far too neglected by Herder scholars. This formulation depicts the 'regime' as something that, though crucially determinate per se, is also part of a broader physical and social environment; it is revealing, too, in the way it illustrates Herder's understanding of 'interaction.' Climate, he reiterated, is only a conditioning factor, promoting or inhibiting political development as all other activities, making them "thrive better in one place than in another"; national character (by which he meant those characteristics that distinguish one political culture in the broadest sense of 'political community' from another) "may more closely determine the kind of seed; whilst the political constitution of a nation *in its widest sense*—its laws, principles of government, civic traditions and ideals—undoubtedly represents the close tilling of the soil; the sowing of the seed and the determination of all those vital resources without which nothing can prosper, nothing can grow."[62]

The political constitution, then, determines the fundamental rules of the political game *and* the manner in which they are likely to be made, re-made, and observed. In this respect, no doubt, the values of the social infrastructure—the 'political community'—will play their part. But, significantly, Herder included in the political constitution a *specific* type of value—civic traditions and ideals—which clearly embodies an ethos *sui generis*, a *political ethos*. In this, indeed, he saw the king-pin of politics and the leaven of a political culture; for, as we shall note, his attitude to 'government' was essentially negative.

The quoted passage also interestingly suggests a distinction between two forms or modes of interaction, between 'interaction' in the strict sense and 'interaction' in a broader and less precise sense. Interaction in the strict sense refers here to the operation of such distinctly human spheres and man-made factors as laws, political principles, ideals and civic traditions, while interaction in the broader sense refers to the diverse inter-relations between human and non-human factors, such as geographic and meteorological influences. Implicit in this distinction is the (obvious, yet occasionally overlooked) recognition that only some form of *human* activity

[59] IV, 429.

[60] XIV, 87.

[61] Herder uses the term when discussing the political culture of the ancient Hebrews in comparison with that of the Greeks. (See esp. XIV, 67). This may indeed have been the earliest use of the term, which makes somewhat surprising the omission of any mention of Herder in current literature on political culture, although attention is paid to such writers as Montesquieu, Tocqueville and Bagehot. See, for example, Lucian W. Pye and Sidney Verba (*eds.*), *Political Culture and Political Development* (Princeton: Princeton University Press, 1965), p. 514.

[62] Herder's italics. IX, 311–312.

(including thought and its product, ideas) is capable of reciprocal inter-*action*, either (as I suggested earlier) 'horizontally,' in so far as at any given time men influence each other directly or indirectly, or 'vertically,' in so far as this influence involves the historical dimension. That Herder was concerned to make and, indeed, underline this distinction is borne out further by his treatment of climate in the *Ideas*.[63]

As to the third analytical level of Easton's political system, 'government', much of what Herder had to say is extremely critical and one might say, radical, in the populist sense. While he granted that the formal expression of government through the person of the head of state might fulfill the 'functional' requirement of a collectivity for symbolic representation, being as it were "the abstracted symbol of the whole nation," as he put it,[64] he had nothing but scorn for hereditary monarchy or any form of absolute leadership.[65] Kant's dictum that man in the realm of politics is like an animal needing a master to rule and control him,[66] provoked Herder's angry retort that man is man only if he no longer needs a master in any real sense. The idea that man *needs* a strict master, Herder added, presupposes that he is weak or under age, and therefore incapable of managing his own affairs, or that he is a wild detestable creature, demanding a tamer or minister of vengeance.[67] He rejected, no less vehemently, the concept of government as the legitimate wielder of physical power. The institution of a central political power was for him not the beginning, but the collapse of politics; an obvious symptom of social decay and political bankruptcy.[68] It would seem that Herder here failed to distinguish the possession of power from its actual use, arguing as if the very existence of power in itself constituted the erosion of politics as a pursuit worthy of adult men. This complete rejection of even the *ultima ratio* variety of coercion theory may (perhaps rightly), be judged as utterly unrealistic. At the same time it should be borne in mind that Herder did not rule out power other than physical force, by his stress on collective values and customs and on solidarity sentiments derived from participation. Nor did he reject power in the sense of 'influence,' provided it is widely diffused, in keeping with his pluralist scheme.

Herder's attitude to bureaucracy was no more favorable. He had little faith in it as an instrument for nation-building. Its hierarchical structure, based on seniority rather than talent or vision, its endemic conservatism and hostility to change, its arrogance and conceit stemming from the belief that the bureaucrats as experts know best what is good for a polity, did not enamour it to him as a desirable 'government.'[69] This viewpoint is not quite so unrealistic as it may seem, neither within the context of eighteenth-century Germany, nor with reference to the contemporary scene in some ex-colonial countries.[70] Indeed, the example of the United States where, in Joseph La Palombara's words, "an enormous amount of national development took place" before bureaucracy had to any appreciable extent been established, supports Herder's low estimate of bureaucracy as an agent in nation-building.[71]

Politics, in short, did not mean 'government' for Herder in any of the senses we just sketched. Politics is not an 'instrument,' but an *activity*, and it is not confined to the 'state.' As soon as there is a family, he declared, there is some form of politics.[72] And since he viewed a polity as but an extension of the family—the same sort of plant, only with more branches[73] —'family' and 'state' are not differentiated in kind but merely in degree. While this denial of a fundamental difference between the family and the state involves Herder in a number of perplexities (which I have discussed elsewhere),[74] it also signals an important departure from the still common identification of the political with one specific type of organization such as the state. For the rejection of the Aristotelian 'state

[63] See particularly the section entitled "Concluding Remarks about the Conflict between Climate and the Genetic Force." (XIII, 284–289).

[64] XIII, 453.

[65] XIII, 377 and 383.

[66] Kant, *Idee zu einer allgemeinen Geschichte in weltbürgerlicher Absicht*, 1784; *Kant Schriften*, Preuss. Akademie der Wissenschaften edition (Berlin, 1923), Vol. VIII, p. 23.

[67] XIII, 383–384.

[68] XIII, 319–322.

[69] XIII, 384–385; see also XVI, 48.

[70] See, for example, Ralph Braibanti, "The Civil Service of Pakistan," *South Atlantic Quarterly*, 58 (1959), 258–304, and his chapter on "Public Bureaucracy and Judiciary in Pakistan" in Joseph La Palombara (ed.), *Bureaucracy and Political Development* (Princeton: Princeton University Press, 1963), pp. 360–440.

[71] La Palombara, *ibid.*, p. 55.

[72] IX, 313.

[73] XIII, 384. Aristotle by contrast, is emphatic in stressing that the distinction between a family and a state is not a simple, numerical difference, but a difference in kind. (*Politics* I, chs. 1, 3 and III, ch. 4).

[74] See F. M. Barnard, *Herder's Social and Political Thought, op. cit.* ch. IV, esp. pp. 84–86.

and non-state' distinction as *the* criterion of what is or is not political introduces a new and potentially fruitful dimension into political theorizing, which makes it possible to view the political development of diverse cultures in terms of a continuum.

What, then, was, for Herder, the essence of *political* development? Let us assume he were confronted with the ten definitions of political development cited by Lucian Pye in *Aspects of Political Development*. In view of what Herder had to say about social welfare, education and the need for maximizing occupational opportunities for everybody—he referred to the provision of such conditions as 'humanization'[75]—it is fair to presume that he would have concurred with the importance attached to economic development in most of them. But it is usually fair to presume that he would have regarded only two definitions as specifically referring to *political* development: mass participation and the building of democracy. For his own notion of political development was essentially the realization of a democratic ethic, even though he conceived of 'participation' in a sense different from both its 'western' and 'eastern' expression, in that he visualized it as free from elitist centralism. Its aim is the fusion of free co-operation with a high degree of diversity and competition, particularly in the realm of ideas.[76] What Herder chiefly had at heart was political freedom, and, as for others before and after him (and Hannah Arendt in particular in our own day)[77] political freedom meant for him first and foremost popular participation.

This stress on participation evidences once again the measure of Herder's almost prophetic vision. At least this is what the modern reader is bound to feel when he is told by contemporary authorities on political culture and political development that if there is one aspect of the "new world political culture" discernible, it is "a political culture of participation." Gabriel Almond and Sidney Verba indeed speak of a "political revolution going on throughout the world," which they call "the participation explosion":

> In all the new nations of the world the belief that the ordinary man is politically relevant—that he ought to be an involved participant in the political system—is widespread.[78]

But like most concepts in the vocabulary of politics, the notion of participation is rich in ambiguity. In order, therefore, to delineate somewhat Herder's participatory vision, it should be pointed out that he thought of two stages or levels in the realization of the envisaged democratic ethic. These can be distinguished as 'transfer' and 'goal' culture respectively.

The transfer culture corresponds to Herder's 'aristo-democracy,' that is to say, to the period of transition from what Almond and Verba call the "subject political culture" to what they call the "participant political culture."[79] Herder envisaged this transformation from elitist tutelage to increasing popular participation as an educational process oriented towards the gradual innovation of societal values which would provide an alternative to abrupt socio-cultural discontinuities. The task of guiding this 'educational' transformation Herder assigned to popular leaders whom he called aristo-democrats. Like Plato's guardians, the aristo-democrats are expected to be men of intellectual excellence rather than men of property. But unlike Plato's philosopher-kings, Herder's popular leaders are only a transitional expedient, for their function is not to rule, let alone to perpetuate their rule, but rather to help all members of the nation to achieve that degree of political maturity which will render their future leadership dispensable.[80]

The assumption underlying this conception of political leadership is perhaps somewhat less than realistic. For Herder assumed that these leaders, in their desire to relinquish power at the earliest possible opportunity—presumably in order to return to their preferred scholarly pursuits—would do everything to hasten the attainment of universal civic consciousness within the nation. Until such a stage is reached, until, that is, the state can dispose of elitism however popularly based, it remains *qua* state a mechanical contrivance.[81] But he had no illusions about the difficulties that may attend this political transformation. He granted that there may be "some disorder and a good deal of disagreement," that "men are not always honest," and that "the institutions as they emerge may be far from perfect," but, he asked,

> is it healthier or more beneficial for mankind to

[75] IX, 407; see also *ibid.*, 401–408; XVI, 601 and XXX, 234.

[76] Herder's advocacy of freedom of thought and expression in his treatise *On the Reciprocal Influence of Government and the Sciences* forcibly recalls Milton's pleas in the *Areopagitica*, as it also foreshadows Mill's central arguments in his essay *On Liberty*. (See esp. IX, 357–361).

[77] Hannah Arendt, *On Revolution*. (New York: Viking Press, 1963), p. 221.

[78] Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1968), p. 4.

[79] *Ibid.*, p. 19.

[80] XIV, 217.

[81] XIII, 455; see also *ibid.*, 385.

produce only the lifeless cogs of a huge, wooden, machine . . . in which men are forced to rot and decay during their lifetime, or to arouse and activate living energies?[82]

Clearly, political legitimacy (understood here as stemming from maximum popular participation in government) mattered more to Herder than effectiveness, to speak in current terminology. At the same time, Herder questioned the necessary correlation between absolutism and effective government. "Those who feel they are accountable to no-one," he wrote towards the end of the century, "must shoulder the blame for our political disorders. . . . A state cannot be a healthy polity unless all its members share a sense of civic responsibility."[83] Thus while in some respects Herder's conception of political leadership seems frankly utopian, it is not wholly devoid of sober realism. In particular the importance which it attaches to 'grass roots' politics and the socializing function of education has an undoubted relevance to current concerns over political participation in developing areas.

The goal culture that is to emerge from this political transformation is to witness the disappearance of the state as an administrative machine and its replacement by a pluralist diffusion of government, where 'government' is virtually reduced (or elevated perhaps) to what Herder in the *Ideas* called 'joint endeavor' (*Zusammenwirken*), which would dispense with coercion and central direction of any sort.[84] It is not important for our purpose to decide whether this almost eschatological vision of the ultimate goal of political development is utopian or not. But what is highly significant is the paradigm of participation which it projects. Perhaps a comparison with Rousseau's vision of political participation may bring this paradigm into sharper relief.

The paradigms of Rousseau and Herder each typify a distinctive interpretation of the democratic ethic. They share a belief in a high measure of equality based on achievement principles, but they differ about the conditions conducive to political participation. Rousseau's paradigm favors participation within a closely knit and highly centralized framework. Herder's paradigm favors participation within a diffuse framework operating from diverse and autonomous bases of power. For Herder the group rather than the individual constituted the nuclear unit of socio-political association. Hence, in contrast to Rousseau's undisguised hostility to any form of group interest, Herder envisaged the socio-political structure of a polity on pluralistic lines. The constituent groups are seen as power bases (in the sense of foci of influence) in their own right; there is no single focus of power. Diffusion rather than concentration of authoritative functions characterizes Herder's scheme. Its structure is intended to combine a high degree of horizontal differentiation with a minimum of vertical differentiation: all are "masters' in some spheres but none are masters in all spheres. The chief participants in this scheme are the representatives of the diverse groups and institutions that make up the polity. They are chosen by their peers on purely achievement criteria: they are the best men for the job. But their sphere of competence and range of power (influence) starts and ends in carrying out the limited task that has been allotted to them.[85]

Herder's starting points, then, are plurality and diversity. Consensus is not a prerequisite of participation but only the operational goal of a process that admits of conflict. Consensus is potentiality seeking realization, not *a priori* actuality. It is neither the decree of a transcendental will, nor the unanimous voice of a monolithic *macro-anthropos*. With Rousseau, on the other hand, the chief emphasis is on the primacy of unity and a highly integrated and centralized political structure. Factions or sectional interests are *a priori* inimical to a common purpose. They may reflect the 'will of all,' but, in so doing, they militate against the 'general will.' While Rousseau's scheme, no less than Herder's, presupposes voluntary and spontaneous participation, and co-ordination rather than sub-ordination, its practical operation has, so far at least, proved compatible only with forms of political organization that procedurally deny, or severely inhibit, the expression of dissent or diversity.

Doubts about the viability of either paradigm in its pure or extreme form seem justifiable enough. However, one important corollary of Herder's paradigm is worth noting. By focussing on the diffusion of political power rather than vesting it in a totality, the 'sovereignty of the people,' Herder revealed a truer understanding of what political analysis is essentially about.[86]

[82] V, 516; see also XIII, 340–341. J. S. Mill argues similarly in *Representative Government*, Ch. 8.

[83] XVIII, 309.

[84] XIV, 227.

[85] XIII, 376.

[86] Lasswell and Kaplan make this point rather well: "To speak of society as a whole as a power holder is to miss the whole point of political analysis. Power is distributive, and the aim of political science is to determine how and on what basis it is distributed." Harold D. Lasswell and Abraham Kaplan, *Power and Society* (New Haven: Yale University Press, 1950), p. 96.

Rousseau's paradigm also appears the more precarious under any but emergency situations. But I fully admit that many new nations may view their gigantic tasks in terms of emergency operations and that they therefore may rightly feel prompted to choose the Rousseauian rather than the Herderian model of political participation and political development. I realize also that the former corresponds more closely to the philosophy of the positive state which is judged by many to meet more satisfactorily the urgent requirements of modernization. I am not certain, though, whether or not the Herderian model necessarily presupposes some form of *economic* liberalism; experience with pluralist systems so far would seem to suggest that it does.[87]

The idea of maximal participation as the cornerstone of democracy has, to be sure, been questioned by contemporary political scientists. It is considered a useful myth, not an idea politicians really believe in. Voting studies have raised serious doubts not only about the actual degree of political participation but also about the desirability of intense participation. And yet, to a growing number of people—as we remarked earlier—political participation is not just a lofty ideal, beautiful to behold though impossible to realize, but a tangible goal within man's attainable reach. Disagreements about procedural means, they feel, there might well be. But that it is more, not less, necessary today, is a conviction few would easily surrender.

To speak of 'the crisis' of this or that has become rather fashionable these days; but to refer to the ambivalence over political participation as a crisis symptom is perhaps not unduly melodramatic. For it may well be that the style political participation is to assume is a problem that will defy purely pragmatic tinkering and involve instead a searching re-appraisal of the very content of the democratic ethic. In such a re-appraisal Rousseau's no less than Herder's paradigm of participation could prove of more than academic import.

VII. CONCLUSION

Modernization, nationalism, and popular participation, we are told, are the distinctive characteristics of a 'world culture.' To admit this is not, however, to refute the need for raising the formidable unknowns that are involved. Herder's gospel proclaiming the nation state as the most natural unit for men to live in has indeed become a world religion. Likewise the idea —which it enshrines—that the nation state, and only the nation state, can provide the proper arena for popular participation has indeed gained well-nigh universal currency. Yet the determination of what constitutes a 'nation,' its 'proper' territorial boundaries, the 'equitable' representation of ethnic, religious, regional, or economic interests, are by no means settled questions in even highly 'developed' polities.

Moreover, it can scarcely be gainsaid that modernization, nationalism and popular participation embrace values the reconciliation of which have proved, and may continue to prove, exceedingly problematical. The logic of nationalism, whatever it may be, is not necessarily the logic of economic rationality or administrative efficiency. Considerations relevant to the one need not therefore be relevant to either of the others. A nationalist does not ask himself whether or not he is more competently governed, or economically better placed, in one political setting rather than another. Sudeten-German nationalists in pre-war Czechoslovakia did not, and Arab nationalists in present-day Israel do not, entertain such 'rational' calculations. Max Weber was undoubtedly right in maintaining that no regime can rely on coercion alone;[88] but to grant the validity of this observation does not warrant the conclusion that a democratic regime based on consent rather than force, self-evidently induces a sense of participation in the citizen, especially if he feels alienated from the prevailing political culture. Multi-ethnic polities, incapable of integrating particularist sentiments, are continuously threatened from within until or unless they can resolve the problem of minorities one way or another. Nation building can hardly be expected to proceed if minorities unwilling to identify themselves with a state are treated as if they did not exist.[89]

In short, political development does not *mean* technological innovation, military prowess, administrative efficiency, national self-determination or, for that matter, popular participation. And even if it is maintained that it does, it is more than unlikely that aspirations towards all these can be met at one and the same time.

[87] See La Palombara, *Bureaucracy and Political Development, op. cit.*, pp. 30 and 57.

[88] Max Weber, *The Theory of Social and Economic Organization* (New York: Free Press, 1957), p. 382.

[89] See, for example, I. Wallerstein, "Ethnicity and National Integration in West Africa," *Sociologie Politique de l'Afrique Noire. Cahiers d'Etudes Africaines* 3 (1960), 129–139; Fred R. von der Mehden, *Politics of the Developing Nations, op. cit.*, pp. 33–47; Clifford Geertz, "The Integrative Revolution: Primordial Sentiments and Civic Politics in the New States," in Welch, *Political Modernization, op. cit.*, pp. 167–188; and Pye, *Aspects of Political Development, op. cit*, p. 23.

Choices have to be made. That is why political development, in the last analysis, is an act of will. The refusal to face up to admittedly difficult, and often distressingly bitter, choices is at the heart of what is known as the crisis of rising expectations, the hub of many current discontents.

Most themes touched upon in this article clearly bristle with difficulties. I believe, however, that Herder offered a number of extremely suggestive insights, particularly into problems of culture diffusion, continuity, socialization and socio-political cohesion and solidarity, and that these insights may be of interest to students of political culture and political development.

POLITICS, IDEOLOGY, AND BELIEF SYSTEMS*

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The word ideology points to a black box. As a philosopher puts it, ideology "signifies at the same time truth and error, universality and particularity, wisdom and ignorance."[1] Likewise, for the political scientist the term ideology points to a cluster concept, i.e., belongs to the concepts that bracket a variety of complex phenomena about which one tries to generalize; and the growing popularity of the term has been matched, if anything, by its growing obscurity.[2] All in all, one is entitled to wonder whether there is any point in using "ideology" for scholarly purposes.[3] And my specific question will be whether there is a technical meaning, or meanings, of "ideology" which constitute a necessary tool of enquiry for a science of politics.

Discussions about ideology generally fall into two broad domains, namely, *ideology in knowledge* and/or *ideology in politics*. With respect to the first area of inquiry the question is whether, and to what extent, man's knowledge is ideologically conditioned or distorted.[4] With respect to the second area of enquiry the question is whether ideology is an essential feature of politics and, if so, what does it explain. In the first case "ideology" is contrasted with "truth," science and valid knowledge in general[5]; whereas in the second case we are not concerned with the truth-value but with the functional value, so to speak, of ideology. In the first sense by saying ideology we actually mean *ideological doctrine* (and equivalents), whereas in the second sense we ultimately point to an *ideological mentality* (also called, hereinafter, ideologism).

The distinction between ideology in knowledge and ideology in politics is not necessarily neat, for a number of issues draw from both domains. For instance, if one asks whether the various "isms" of politics—e.g., liberalism, so-

(*) This is an abridged draft of a paper prepared for the meeting on "Ideology and Politics" of the *Institut International de Philosophie Politique*, Chatillon, June 1967.

[1] Remo Cantoni, *Illusione e Pregiudizio* (Milano: Mondadori, 1967), p. 103.

[2] Concerning the popularity it is symptomatic that while the 1930–1935 *Encyclopaedia of the Social Sciences* did not include the item ideology, its successor, the 1968 *International Encyclopedia of the Social Sciences*, contains two articles on ideology. For the bibliography see Norman Birnbaum, "The Sociological Study of Ideology 1940–1960: A Trend Report and Bibliography," *Current Sociology* (Oxford: Blackwell, 1962); and Kurt Link, "Bibliographische Einfürung," in *Ideologie, Ideologiekritik und Wissensoziologie* (Neuwied: Luchterhand, 1961).

[3] See the conjecture of Arne Naess that "the movement of the term 'ideology' into social science, social psychology and political science will, within a generation, be followed by a movement in the other direction. It will continue to be used in headlines, in summaries and popularizations, but scarcely in statements intended to express . . . theories, hypotheses or classifications of observations." *Democracy, Ideology and Objectivity—Studies in the Semantics and Cognitive Analysis of Ideological Controversy* (Oslo: Oslo University Press; and Oxford: Blackwell, 1956), p. 171. The book also reviews many current definitions of ideology, esp. pp. 141–198.

[4] Marx and Mannheim are the obvious references, and the literature is extensive. See esp. I. L. Horowitz, *Philosophy, Science and the Sociology of Knowledge* (Springfield: Thomas, 1961); and Jacques J. Maquet, *Sociologie de la Connaissance —Etude Critique des Systèmes de K. Mannheim et de P. A. Sorokin* (Louvain: Nauwelaerts, 1949). A particularly brilliant criticism is Robert K. Merton, *Social Theory and Social Structure* (Glencoe: Free Press, rev. ed. 1957) chaps 12 and 13. See also Raymond Aron, *La Sociologie Allemande Contemporaine* (Paris: P.U.F., 2nd ed., 1950) pp. 74–94. My own position (*Democratic Theory*, New York: Praeger, 1965, pp. 455–460) is that the Mannheim type of sociology of knowledge attacks the consumer's end of the problem, thereby explaining the success, the spread of mental products. How mental products are produced is, however, an entirely different matter.

[5] This is actually the major and more persistent controversy. The theme has been especially pursued by analytical philosophy. See Theodore Geiger, *Ideologie und Warheit: Eine Soziologische Kritik des Denkens* (Stuttgart: Humboldt Verlag, 1953); Gustav Bergmann, "Ideology," now in *The Metaphysics of Logical Positivism* (New York: Longmans Green, 1954); E. Topitsch, "Begriff und Funktion der Ideologie," in *Sozialphilosophie zwischen Ideologie und Wissenschaft* (Neuwied: Luchterhand, 1961).

cialism, nationalism, etc.—are *only* ideologies (there is no quarrel that in some sense of the word they also are ideologies), it can be readily conceded that our treatment of the "isms" of politics will be more perceptive if it presupposes a stand vis à vis the sociology of knowledge. But the distinction is neat when one is exclusively concerned with the question: what does ideology explain about the nature of politics?

For the sake of brevity a number of preliminary points will have to be laid down axiomatically.

First, there is no object in adopting new terms unless they are employed to cover new phenomena or new sets of observations. I take it, therefore, that the word ideology has been increasingly adopted in response to, and as a pointer of, the development of politics—if not the development of an unprecedented aspect of politics. To be sure, one may choose to apply "ideology" to any time and place; but not without loss of conceptual substance.

Second, either we dispose of a notion of ideology that lends itself to falsification, or we do not dispose of an empirically usable term. We are logically required, then, to declare what ideology is not, i.e., to qualify the notion *a contrario*. In other terms, we are required to conceive ideology as a dimension or an aspect of politics which may, or may not, be found to apply to the real world. For this purpose "ideological politics" will be opposed here to "pragmatic politics," i.e., pragmatism will be used as a designation for non-ideology. To be sure, I am not saying that we *must* oppose ideological to non-ideological politics. My argument is, rather, that if no such opposition is justified. then the notion of ideology loses much of its interest and has little explanatory value.

Third, I take the methodological view that *awaiting contrary proof* (subsequently called the a.c.p. clause) no concept should be used as a synonym for any other concept. Ideology is generally qualified by how it relates to idea, belief, opinion, creed, myth, utopia, ethos, and similar or derivative concepts. However, I shall not be content with implying that the aforementioned concepts are largely interchangeable and tend to overlap. This way of handling conceptual problems I call "superfluous coextensiveness," and represents an intolerable waste for the economy of language and clarity of thought. The more a set of concepts is closely interlinked, the more their meanings need to be specified and distinguished. And the point is that under the a.c.p. (awaiting contrary proof) clause, it is the equivalence between two concepts, not their difference, that requires demonstration. Therefore, unless it can be demonstrated that "ideology is nothing other than x,"[6] I shall aim at using ideology to signify something that no other neighboring concept signifies.

Fourth, the relation between ideology and "idea" (i.e., conceptual thinking) bears on the genesis of ideological doctrines, on how they originate and are born, and is eventually conducive to an epistemological discussion. It will suffice to note here that ideologies are no longer ideas, in the sense that ideological doctrines no longer fall under the jurisdiction of logic and verification. When we pass on to consider ideologies we are confronted with "the conversion of ideas into social levers,"[7] i.e., with a *persuasive treatment* (not a logical treatment) of ideas leading to action-oriented ideals.[8] However, for the present enquiry ideological doctrines are givens—they exist in their distinctiveness, whatever their *genesis* in relation to "idea." I propose, therefore, to investigate a different relationship, namely, the relation of ideology to "belief" and belief systems, under the assumption that this is the pertinent focus for discussing the *structure* and *function* of ideologism. By saying structure I refer to *how* one believes; and by speaking of function I shall be concerned with the *efficacy*, or effectiveness, of belief systems.

A final clarification is in order. Given the fact that we are confronted with a black box, it will be necessary to begin with a pure and simple *semantic explanation*.[9] My first assignment is, then, to clarify the various meanings of the term ideology as they stand in actual usage.

A term may have a "notational use," i.e., it may be a useful shorthand, an economizing device, and yet explain nothing.[10] This is notably

[6] This is called by Abraham Kaplan the "pattern model of explanation," as against the "deductive model of explanation": *The Conduct of Inquiry* (San Francisco: Chandler, 1964), esp. pp. 332–341. The pattern model of explanation does not necessarily coincide with Hempel's "reduction to the familiar": *Aspects of Scientific Explanation* (New York: Free Press, 1965), pp. 430–433.

[7] Daniel Bell, *The End of Ideology*, New York: Collier Books, 2nd rev. ed., 1962, p. 400.

[8] See C. J. Friedrich: "It is confusing . . . to call any system of ideas an ideology . . . Ideologies are action-related systems of ideas . . .": *Man and his Government* (New York: McGraw-Hill, 1963), p. 89. In a similar vein Z. B. Brzezinski qualifies ideology as "essentially an action-program suitable for mass consumption": *Ideology and Power in Soviet Politics* (New York: Praeger, 1962), pp. 5–6.

[9] Abraham Kaplan, *op. cit.*, p. 327.

[10] Kaplan contrasts *notational* and *substantive* terms as follows: "Substantive terms cannot be eliminated without loss of conceptual content, but

the case with the authors that are satisfied with saying that ideology amounts to the views of any and all social groups, or that everybody has political beliefs which amount to everyman's ideology. To abridge "social views" or "political beliefs" into "ideology" is perhaps convenient, but we are simply left to describe what people believe or have to say in political matters. If the term ideology were not employed, no loss of conceptual content would follow. Moreover, the foregoing definitions are not falsifiable: they simply lead to the conclusion that everything (or, conversely, nothing) is ideology.

This is not to say that the notational use of the term ideology is necessarily trivial. Not only do we need notations, but we equally need "explanation stoppers," i.e., terms defined in such a way as to avoid endless regression of enquiry. Ideology is, however, an ambitious term; it does belong to the concepts that are supposed to have broad and farreaching causative significance. Hence, if the eminence attributed to the notion of ideology is justified, it must be justified because the term *explains,* not merely because it abridges and/or provides a regression stopper. Therefore, as the discussion proceeds the focus will be progressively shifted to the *explanatory value* of the various meanings of ideology, for my ultimate purpose is to probe and single out the conceptualizations that are cognitive instruments, that do have explanatory-causative potency.[11]

I. THE IDEOLOGICAL MENTALITY

If ideology is linked to "belief," it is readily apparent that the general class is "belief systems" and that ideology is the narrower conceptualization. Properly speaking, "a person's belief-disbelief system is really a political-religious-philosophic-scientific-etcetera system,"[12] i.e., a total and diffuse framework; whereas ideology indicates only the political *part* of a belief system. Whatever the psychological functions of a belief system, for the present discussion it can be simply defined as the system of symbolic orientations to be found in each individual. Correlatively, a *political* belief system consists of the set of beliefs according to which individuals navigate and orient themselves in the sea of politics.

It is not sufficient to say, however, that ideology is the political slice, or part, of a belief system. Under the a.c.p. (awaiting contrary proof) clause, I assume that ideology indicates a particular state, or structure, of political belief systems. By definition, then, not all political belief systems are ideological.

Two corollaries follow. First, pragmatism is also a state of belief systems. Indeed to contrast ideologism and pragmatism as representing, respectively, a belief versus a belief-less orientation toward politics is to preempt the issue from the outset. The first corollary is, therefore, that *both* ideologism and pragmatism are possible states of belief. Second, and conversely, the presence of beliefs does not suffice to qualify, per se, the ideological nature of such beliefs: the pragmatic actor also is belief-oriented. The second corollary is, then, that beliefs are, as such, a *common,* not a discriminating element. While not every polity need contain ideological publics, no polity can exist without publics that have beliefs.

A number of authors seemingly agree to the effect that not all political belief systems are ideological, but they founder on the reefs of definition by failing to provide a discriminating element. According to Talcott Parsons, for instance, ideology is "a system of beliefs, held in common by the members of a collectivity . . . which is oriented to the *evaluative integration* of the collectivity."[13] But the discriminating power of the notion of evaluative integration, and, in general, of having recourse to the value dimension, is almost nil.[14] Beliefs are inextricably value-laden—they precede the analytical distinction between value and fact—and any belief system serves the purpose of integrating (axiologically or otherwise) the belief-collectivity.[15] It is not without reason, therefore,

notational terms are fundamentally abbreviations, and could be replaced": *op. cit.,* p. 49.

[11] I purposely avoid saying "explication" on account of the technical meaning attributed to the term by Carnap followed by Carl K. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Chicago: The University of Chicago Press, 1952), pp. 11–12. For the sake of simplicity I equally neglect the more sophisticated test suggested by K. R. Popper with regard to the "informative content" of scientific statements: *The Logic of Scientific Discovery* (London: Hutchinson, 1959), pp. 119–121. For an introductory overview see Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace, 1961), chap. 2: "Patterns of Scientific Explanation."

[12] Milton Rokeach, *The Open and Closed Mind* (New York: Basic Books, 1960), p. 35.

[13] *The Social System* (New York: Free Press, 1964 ed.), p. 349 (My italics).

[14] This applies also to the attempt, notably pursued by Bergmann and Geiger, to qualify ideology (vis à vis scientific truth) as any value judgement mistaken for, or disguised as, statement of fact. This view makes ideology far too broad.

[15] Let alone the fact that the "integration" of a belief group may well be a "disintegration" vis à vis other groups. We are forcefully reminded of this

that I propose to search for structural elements of differentiation bearing on *how* one believes.

Speaking of belief systems, both the notions of "belief" and "system" must be taken seriously. Under the awaiting-contrary-proof clause a *belief* is neither an opinion nor an idea. Opinions include and characterize the more ephemeral and superficial level of discourse, and can be safely set aside, therefore, with reference to belief systems. On the other hand, in the strict sense ideas are *thought of,* they typically belong to the more self-conscious dimension of discourse, to reasoning and theorizing. If the sentence is understood *cum grano salis,* beliefs can be defined as "ideas that are no longer thought,"[16] to signify that beliefs are idea-clusters that routinize the cost of decisions precisely because they are taken for granted. Beliefs are *believed*—not explored, tested and held under the searchlight of consciousness.[17]

As for the notion of *system,* a first obvious caution is that the system may have properties which are not exhibited by its parts. But it is more important to underline that a "belief system" points to a state of *boundedness,* to the fact that beliefs hang together. This belief-linkage is presumably what a number of authors have in mind when they define ideology as a more or less coherent set of ideas. Now, logical (or rational) attributes such as "coherence," "consistency," and the like, are hardly applicable to a belief-linkage. Indeed the single belief-elements can be logically contradictory. But there is no question that beliefs are "bound together by some form of constraint or functional interdependence."[18] Beliefs, then, cluster in *systems,* though not in rationally congruent and organized systems.

Having issued the caveat that the system should be taken seriously, no harm follows if one decomposes belief systems in a number of ways. Following Rokeach, one may distinguish between 1) primitive beliefs, 2) intermediate, and 3) peripheral regions of belief.[19] I propose to dwell briefly on the intermediate region, and then to decompose the more peripheral regions of belief into single belief-elements.

The first problem is to pinpoint the discriminating element, the mentality (*forma mentis*) that qualifies an ideological structure of belief in its difference from a pragmatic structure of belief. Following Rokeach again, it appears that the crucial single factor resides in so-called "authority beliefs," and more precisely in the beliefs concerning cognitive authority: the beliefs that tip us off to what is true or false about the world and its events.[20] More concretely one may say that the crucial factor is "the authorities," those on whom we rely for information.

Since nobody can avoid reliance on cognitive authorities, the difference must reside in how authorities are chosen and how the instructions emanating from these authorities are assessed. It is on this basis, in effect, that Rokeach draws the distinction between the *closed* and *open* mind. His initial basic definition is as follows: "A person's [belief] system is open or closed . . . [to] the extent to which the person can receive, evaluate and act on relevant information . . . on its own intrinsic merits."[21] The closed mind is defined, accordingly, as a cognitive state in which a person does not discriminate substantive information from information about the source. Hence the more closed one's belief system, the more he is unable to evaluate relevant information on its own intrinsic merits. In substance, the closed mind relies on, indeed yields to, absolute authority, and is hardly in a position to select and to check its authorities.

Doubtless the association of the ideological mentality with the closed mind can be accused of representing an anti-ideological bias. However, ideological closedness is bad and pragmatic openness is good only according to an intellectual yardstick—and one could well say an intellectualistic prejudice. If we are reminded that the pertinent yardstick is "efficacy" the evaluation could be reversed, for reliance on absolute authorities does obtain the kind of efficacy praised by the man of action and is surely in keeping with the purpose served by ideologies.

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other side of the coin by Ben Halpern: "The function of ideologies . . . is . . . to segregate and consolidate competing groups around rival ideas": "Myth and Ideology in Modern Usage," in *History and Theory,* 1 (1967), p. 136.

[16] I paraphrase from Wladimir Weidlè, "Sur le Concept d'Ideologie," *Le Contrat Social,* (March 1959), p. 77. The author speaks of ideology, but the sentence applies more directly to beliefs.

[17] *Contra,* among others, Clyde Kluckhohn, "Values and Value-Orientations in the Theory of Action," in T. Parsons and E. Shils (eds.), *Toward a General Theory of Action* (Cambridge, Mass.: Harvard University Press, 1951), p. 432: "*Belief* refers primarily to the categories, 'true' and 'false,' 'correct' and 'incorrect'." This intellectualistic conceptualization overlooks the difference between idea and belief.

[18] Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in D. Apter (ed.), *Ideology and Discontent* (New York: The Free Press of Glencoe, 1964), p. 207.

[19] *Op. cit.,* pp. 39–51.

[20] *Ibid.,* esp. p. 44.

[21] *Ibid.,* p. 57, and *passim* pp. 54–67.

In short, the ideologist cannot have it both ways, he cannot claim at the same time an intellectual and a practical primacy.

The foregoing underpins nicely the common opinion according to which the ideological mentality represents a typically *dogmatic*, i.e., rigid and impermeable, approach to politics. On the other hand, it should be noted that a cognitive closed structure fails to justify the other characteristic generally imputed to the ideological mentality, namely, its typically *doctrinaire* bent. Should this characterization be dropped? Or does it draw from another source? I shall abide by the latter suggestion, thereby shifting the focus to the notion of *ideological culture.*

When we speak of ideology as a "culture"—or a cultural pattern—we are more or less implicitly referred to the anthropological notion of culture. Perhaps we may say that we are referred once again to the intermediate regions of belief, though no longer to the "authority-beliefs" but to the "processing-coding-beliefs." The simplest and closest analogy appears to be, however, the *Gestalt* analogy: a cultural pattern is characterized by the forms, or the matrixes, with and within which our mind stores and orders whatever it apprehends. For our purposes these processing-coding *Gestalten* will be labelled "rationalism" and "empiricism," and the assumption will be that these cultural matrixes help explain why only certain polities characteristically display, over time, an ideological patterning.

What strikes me in this connection is the extent to which the typically ideological *isms* of politics—marxism being the outstanding current example—were born and have developed (before being exported) in the cultural area qualified by the notion of *rationalism,* hardly in other cultural contexts, and surely not in the cultural area of *empiricism.*[22] I find it equally striking that only the "rational ideologies"—I mean, the ideologies drawn from rationalistic philosophies and nurtured in a rationalistic soil—travel easily throughout the world.[23] Hence my hypothesis is that ideology and pragmatism qua "political cultures" are related, respectively, to the "cultural matrixes" rationalism and empiricism.

Rationalism and empiricism are generally associated, respectively, with a "coherence" theory of truth as against a "correspondence" theory of truth. Oakeshott equally makes a good point when he writes that "Rationalism is the assertion that . . . practical knowledge is not knowledge at all."[24] But, however sweepingly, let us attempt to qualify rationalism and empiricism in more detail, with the understanding that the following characterizations represent a syndrome, so that the full enumeration is required to appreciate the meaning of the separate assertions.

The *rationalistic* processing-coding tends to approach problems as follows: i) deductive argumentation prevails over evidence and testing; ii) doctrine prevails over practice; iii) principle prevails over precedent; iv) ends prevail over means; and v) perceptions tend to be "covered up," doctrine-loaded, typically indirect. Hegel's famous sentence "the rational is real" goes to the very heart of the rationalistic mind,[25] for the rationalistic attitude is to argue that if the practice goes astray, there must be something wrong with the practice, not with the theory.

Conversely, and symmetrically, the *empirical* processing-coding can be described as follows: i) evidence and testing prevail over deductive argumentation; ii) practice prevails over doctrine; iii) precedent prevails over principle; iv) means prevail over ends, and, therefore; v) its perceptions tend to be more "direct." Consequently the empirical attitude is to argue that if the practice goes astray, something is likely to be wrong with the theory, not with the practice.

[22] If "liberalism" is conceived as an ideology, I suggest that it represents the ideological apex attained by the empirical mind; and surely liberalism has been a poor competitor, ideologically speaking, of socialism, communism, equalitarianism and the like.

[23] In this connection it should be noted that the ideologies of the developing nations and, in general, of the third world have been hardly nurtured and taught in London and Oxford, and even less in the United States. A perusal of Paul E. Sigmund (ed.), *The Ideologies of the Developing Nations* (New York: Praeger, 1963), suffices to confirm, in spite of nebulous and bizarre melanges, their unmistakably rationalistic Western source.

[24] *Rationalism in Politics* (New York: Basic Books, 1962), p. 11. I cannot follow Oakeshott, however, in his positive identification of rationalism with technical knowledge. ("The sovereignty of 'reason,' for the rationalist, means the sovereignty of technique," *ibid.*) It seems to me, rather, that technical knowledge represents the point at which rationalism and empiricism converge. On its own premises, rationalism looks down at technical knowledge as an inferior knowledge.

[25] To be sure, Hegel's sentence had a dialectical circular formulation. But "the rational" is the subject: it is rationality that qualifies reality, not the reverse. Hegel's philosophy was a realistic *rationalism,* hardly a rationalistic *realism.*

The *ad hoc* implications for our subject are that the rationalistic *Gestalt* is characterized (relatively speaking, of course) by openness to deductive axiomatization and deafness to empirical evidence, by low *practical* problem-solving flexibility and by high *theoretical* problem solving ability. Also, and consequently, the rationalistic mind soars at a higher level of explicitness and especially of abstraction than the empirical mind. This implies, in turn, that rationalism is able to embrace and to cover in terms of *Weltanschauung* a comparatively broader "space," either with long deductive chains or with acrobatic leaps—a quality that I shall call comprehensiveness.

The foregoing can be easily translated into a "cultural" characterization of the ideological mentality. If we assume ideologism to result from a rationalistic cultural matrix, it follows that to the extent that the ideological mentality is "open," it is open to rational, deductive argument, hardly to evidence: the sentence "experience proves" proves nothing to an ideologically-minded actor. An ulterior implication is that the ideological mentality identifies *par excellence* with highly "abstract" and "comprehensive" belief systems. In particular an important feature of the ideological mentality that neatly flows from its rationalistic matrix is that the central elements of an ideological belief system are necessarily "ends," not "means."

I shall revert to these characteristics later. For the moment let us simply retain that to the extent that the ideological mentality is characterized by the rationalistic cultural matrix, to the same extent it can be legitimately qualified as a typically principled and doctrinaire way of perceiving political problems and of constructing their solution.[26] It should be clear that I am *not* saying: given a rationalistic cultural matrix, the ideological mentality necessarily follows. I am simply saying that a rationalistic culture is particularly vulnerable on this score. It should be equally clear that a number of individuals react to the culture to which they belong. Therefore, an ideological culture will contain non-ideological minority groups, just as an empirical culture will breed ideological minorities.

Bringing our two threads together, the ideological mentality now results in both a *personality trait* and a *cultural trait*, and the following conclusion appears reasonably warranted: ideologism can be legitimately understood to mean not only a rigid and dogmatic approach to politics, but also a principled and doctrinaire perception of politics. Needless to say, the two characterizations reinforce one another but can exist disjointly. However, for the simplicity of the argument in the following section, the ideological mentality will be reduced to a "closed" cognitive structure, and cognitive closedness will be defined as a state of dogmatic impermeability both to evidence and to argument. Conversely the pragmatic mentality will be simplistically identified with an "open" cognitive structure, and cognitive openness will be defined a state of mental permeability.

II. A FRAMEWORK FOR ANALYSIS

Thus far the analysis has been confined to a single dimension—cognition. But beliefs and belief systems vary not only along a cognitive but also along an emotive dimension. Along the former a belief system can be, as we know, closed or open. Along the emotive dimension beliefs can be intense or feeble, passionately or weakly felt. Possibly, in each individual cognition and affect are tightly interlinked, but with reference to mass phenomena—e.g., abrupt transitions from "hot politics" to "cool politics," and vice versa—cognition and affect seem to vary independently. Assuming cognition to be constant, we can still have formidable oscillations of emotional intensity.

The difference between cognitive structure and emotive status underpins two different conceptions of ideology: *ideologism*, i.e., the mentality on the one hand, and *ideological passion* on the other hand. Per se the ideological mentality is not necessarily conducive to an active involvement and thereby to "ideological activism." Thus whenever we find a mobilized polity displaying a high degree of political activism, reference should be made to the emotional component, and thereby to the notion of ideological affect, or passion. This is also to say that the *effectiveness* of ideology, its capacity of activation, of arousing and unleashing energies, does not reside in the ideological mentality as such but requires, in addition, "ideological heating." Likewise, whenever politics is depicted as a matter of faith, as a religion or even as a mystique, reference is made more often than not to a particular intensity of feeling, of emotional involvement, and we should speak, therefore, of ideological passion.

[26] The rationalist matrix also explains the "logicality of ideological thinking" forcefully highlighted—perhaps in an overly speculative vein—by Hannah Arendt, "Ideology and Terror: A Novel Form of Government," in *The Review of Politics* (July 1953) 303–327.

FIG. 1. Scheme of Analysis.

		EMOTIVE STATUS: *Strong*	EMOTIVE STATUS: *Weak*
COGNITIVE STATUS	*Closed* (Neither subjected to evidence nor argument)	I	II
	Open (Subjected to evidence and/or argument)	III	IV

The overall scheme of analysis is thus the one recapitulated in Figure 1.[27]

Let us now enter the peripheral regions of belief and decompose belief systems into distinguishable belief-elements which are generally expressed in a slogan form. As Dahl introduces the argument, "it is convenient to think of the content of a belief system as made up of identifiable elements or components (e.g. beliefs expressed by propositions like "Democracy is the best form of government," "Bourgeois democracy must be replaced by a dictatorship of the proletariat," "A capitalist economy is more efficient than a socialist economy.")[28] With reference to the four combinations afforded by the scheme of Figure 1, the belief-elements can be broken down, following the roman numerals of the previous table, as follows.

(I) Closed and strongly felt. These elements will be called *fixed.*

(II) Closed but weakly held. These elements may be called *inelastic.*

(III) Open but strongly felt. These elements will be called *firm.*

(IV) Open and weakly felt. These elements will be called *flexible.*

The scheme of Figure 1 can thus be filled with the typology of belief-elements represented in Figure 2.

Some additional but in no way exhaustive qualifications are as follows. (I) The *fixed* elements are rigid, dogmatic, impermeable to argument and evidence. One may also call them "adamant" elements. As such, they are subject only to traumatic change under conditions of great stress. Given the fact that they are emotively participated in, the dynamic, activistic potential of the adamant elements is high. (II) The *inelastic* elements are still impermeable to argument and to evidence, but display a low dynamic potential, for they are not passionately felt. Furthermore, even though they tend to be persistent over time, they can fade away, or be dismissed, without traumatic consequences. (III) The *firm* elements are firmly held, but are open to evidence and/or to argument. Even though they tend to be persistent, they are not impermeable and are, therefore, changeable, at least in principle. On the other hand, firm ele-

[27] The scheme is largely inspired by Robert E. Dahl, *Ideology, Conflict and Consensus: Notes for a Theory* (mimeographed), a paper prepared for the panel on "Consensus and Dissent," VII IPSA World Congress, Bruxelles, September 18–23, 1967, p. 2. This essay is particularly indebted to Dahl's intellectual stimulation.

[28] *Ibid.*

FIG. 2. Typology of Belief-Elements (*).

		AFFECT: Strong	AFFECT: Weak
COGNITIVE STATUS	Closed	*Fixed* elements	*Inelastic* elements
	Open	*Firm* elements	*Flexible* elements

(*) This table repeats, in substance, Dahl's scheme on page 3, *op. cit.*

FIG. 3. Patterns of Belief Systems.

I	II
Adamant "fixed-strong" elements prevail	*Resilient* "fixed-feeble" elements prevail
III	IV
Firm "flexible-strong" elements prevail	*Flexible* "flexible-feeble" elements prevail

ments have a stronger dynamic potential than the inelastic elements. (IV) The *flexible* elements are feebly held, open to argument and/or to evidence and, furthermore, to convenience. They are changeable by definition. On the other hand, their dynamic potential is very low.

It can be easily seen that the foregoing typology of belief-*elements* can be turned into a typology of belief-*systems* tentatively labelled as in Figure 3.

Conceivably, the simplest way of establishing the nature of a pattern is provided by the following criterion: which type of element prevails in terms of intensity. However, we are also reminded by Converse that "the idea-elements within a belief system vary in a property we shall call centrality."[29] Whenever possible, therefore, one should use centrality as a criterion.

The individuals imbued with an *adamant* belief system are impenetrable vis à vis external influence and are strongly motivated toward outward expansion, either in the form of proselytism or of overt aggression. The individuals imbued with a *resilient* belief system resist change and support the internal *status quo*, but lack dynamism and outward orientation. The individuals sharing a *firm* belief system are both open to change and motivated toward outward expansion. Finally, the individuals sharing a *flexible* belief system easily accept changes but lack outward dynamism.

Be that as it may, we are now in a position to perceive clearly the *placement* of the ideological and pragmatic varieties of belief systems, and to *define* them accordingly (regardless of *what* one believes).

Having reference to our initial scheme (Figure 1), qua ideal types or, better, qua polar opposites, a "perfectly ideological" belief system falls in quadrant I, whereas a "perfectly pragmatic" belief system falls in quadrant IV—as shown in Figure 4.

Definition: Whenever ideology and pragmatism are confronted dichotomously, and thereby conceptualized as polar types, *ideology* is a belief system based on i) fixed elements, characterized by ii) strong affect and iii) closed cognitive structure. *Pragmatism* is, conversely, a belief system based on i) flexible elements characterized by ii) weak affect and iii) open cognitive structure.

Granted that pure types seldom, if ever, are approximated in the real world, this means only that we are also required to define ideology and pragmatism as "concrete" systems. It does not follow that it is logically erroneous, and even less that it is logically superfluous, to define concepts *ex adverso*. I would argue, on the contrary, that the logical error is to hold that if ideology and pragmatism are conceived as blends of a same continuum, they should not be defined as opposites. A continuum of what? Unless the ends of a continuum are defined, the continuum itself remains undefined. Hence, even if one abides by the "continuum language," polar definitions remain the *sine qua non* condition for having a continuum at all.

The usefulness of these mappings, and the extent to which crucial distinctions are often bypassed, can be illustrated with reference to the

[29] "The Nature of Belief Systems in Mass Publics," *loc. cit.*, p. 208.

FIG. 4. Ideology and Pragmatism as Polar Opposites.

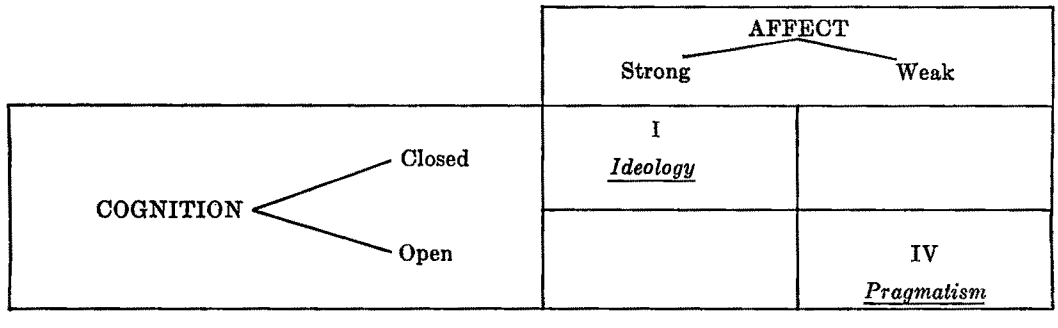
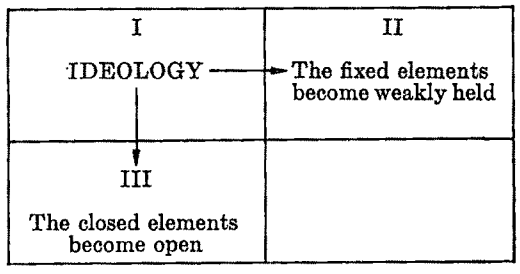


FIG. 5. Decline of Ideology Hypothesis.



decline or end of ideology debate.[30] If the question is, What do we mean by *decline of ideology,* Figure 5 shows that we are liable to confuse, in this connection, two very different processes.

A shift from box I to box II merely reflects a decline of emotive intensity and amounts to a relatively easy and easily predictable transformation. On the other hand, this shift does not necessarily indicate a point of no return. At least in short run terms (from one to two generations), returns from box II to box I, i.e., from cooler to warmer politics, are only to be expected and not difficult to obtain. Moreover, in this respect the argument largely hinges on the point in time chosen as parameter. E.g., with respect to the base 1918–20 the thirties represented a decline of ideology, but the 1946–50 period represented an increase of ideology with respect to a pre-war base; and it seems to me that the late sixties reveal no decline, but resurgence, with respect to the late fifties. A real decline of ideology is safely indicated, then, by the passage from box I to box III. However, a transformation of fixed into firm belief-elements is not an easy transformation, for it involves a radical change in the mental posture, a shift from closedness to openness.[31]

The expression *end of ideology* pushes the matter further, at least in forecasting terms. If "end" is meant seriously, it calls for an overall transformation of an ideological belief system into a pragmatic belief system. If so, Figure 6 underpins how many conditions need to be satisfied and, by implication, the hazardous nature of the prediction.

Let us now turn to the problem of defining ideology and pragmatism as *concrete* belief systems. In the real world, an ideological pattern can be identified as such when the overall distribution of the belief-elements gravitates—with reference to the scheme utilized so far—around quadrants I and II. Conversely, when the overall distribution of the elements gravitates around quadrants IV and III, the polity can be identified as being pragmatic.

This is not to deny the possibility of spill-overs in other boxes as well. Varieties and variations within each concrete belief system can be very great, as suggested by the following rule of thumb: an ideological polity will be *less ideological* the lesser the elements contained in quadrant I; conversely, a pragmatic polity will be *less pragmatic,* the lesser the elements contained in quadrant IV. The two patterns fall wide apart with reference to their respective centers of gravity; but the feebler the gravitational attraction of each *noyeau,* the greater the diffusion, and therefore the overlaps. This is, admittedly, very vague. But one can hardly be more specific until an additional set of distinctions is brought to bear on the discussion.

If belief systems are broken down into elements, they can be classified with respect to the following properties: a) their relative articulation, or richness; b) their correlative constraining power; c) their divisibility in belief strata corresponding to belief publics.

a) With reference to the first property, a belief system can be *rich* (articulate) or *poor* (inarticulate).[32] A rich belief system is necessarily explicit and contains a relatively large number of elements. Conversely, a poor belief system has a low degree of explicitness and consists of relatively few elements. b) With regard to the second property, a belief system may be *strongly constraining* or *feebly constraining*: in the former the elements are tightly related in a "quasi-logical" fashion, while in the latter the elements are loosely connected and follow, at best, an "idiosyncratic" syntax.[33] c) As for the

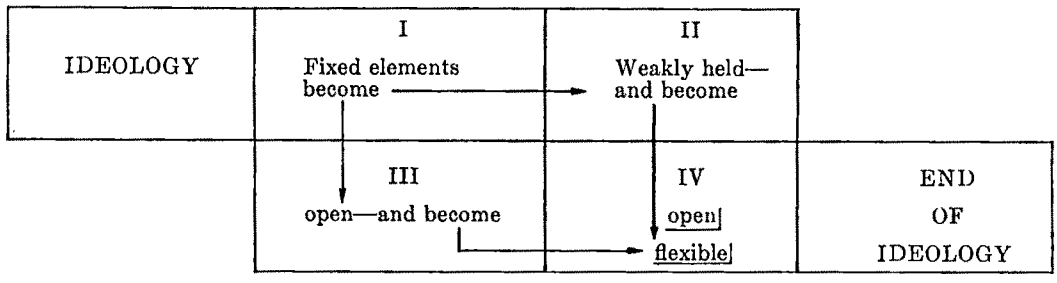
[30] Jean Meynaud's book on the *Destin des Ideologies* is actually a review and a discussion of the decline of ideology literature. See it. transl. *Destino delle Ideologie,* (Cappelli ed., 1964), esp. pp. 37–112. Raymond Aron, Daniel Bell and S. M. Lipset are the standard references on the subject.

[31] Another possibility is that the decline of ideology amounts—all other conditions remaining equal—to a convergence among different ideologies, either in the sense that the opposed "disbelievers" come to share a greater number of beliefs in common, or in the sense that the distinctive elements which oppose the various belief systems become feebly or more feebly held. This is the suggestion perceptively set forth by Dahl (*op. cit.,* pp. 5–8). However, in this case the "decline" would be an optical illusion, for the process described is, in reality, a mere process of growing "affinity" or of diminishing distance between two or more ideologies.

[32] Dahl, *op. cit.,* p. 3.

[33] As Converse puts it, *loc. cit.,* especially pp. 210–211 and 241.

FIG. 6. End of Ideology Hypothesis.



stratification aspect, the various "belief strata" can be identified by the amount of political information received and absorbed by each belief-public.

These properties, or aspects, appear highly correlated. The correspondence between the richness of a belief system and various levels of belief publics is hypothesized by Dahl as follows: "In every country the number of identifiable elements ('richness') in the political belief system of different individuals is most highly related to (1) the amount of political activity an individual engages in; (2) the level or extent of his political interest, and (3) the amount of formal education he has had."[34] The hypothesis is highly plausible on the ground of its sheer logical force, and is supported on empirical grounds by the factual evidence reviewed by Converse. Furthermore, if the argument of Dahl is combined with the "constraint argument" of Converse, two conclusions appear reasonably warranted:

a) A rich, articulate, quasi-logical—and therefore constraining—belief system corresponds to an elite belief system.[35]

b) In comparison, mass publics are likely to display, in whatever country, a poorly explicated, inarticulate, disconnected—and therefore relatively unconstraining—belief system.

If the foregoing is correct, a number of consequences follow. First, investigations are rewarding only if the public under investigation is clearly identified, if we deal with one belief public at a time, and only if each stratum is measured in accord with its standards. Second, the order of investigation suggests that precedence should be given to the rich elite belief systems. Third, mass belief publics appear to be dependent variables of elite belief publics.

The last suggestion is not only crucial, but also appears, at first, hazardous. Yet the thesis is largely implied in the basic finding of Converse that at the lower levels individuals "lack the contextual grasp of the [belief] system to recognize how they should respond to it without being told by elites who hold their confidence."[36]

On the one hand, belief systems are "diffused in 'packages' which consumers come to see as 'natural' wholes"; i.e., they are constraining in that they are presented in such terms: "If you believe this, then you will also believe that, for it follows in such-and-such ways."[37] On the other hand, however, what "follows in such-and-such ways" is not easy to follow—I mean, the argumentative chain is grasped only by the attentive, articulate citizen. The inarticulate public not only lacks, without guidance, the grasp of *what goes with what* in the deductive chain of a highflown, abstract argument; it equally and especially lacks the information and the inductive capability of deciding on his own how a *specific* event relates to a general principle, and specifically to *which* principle.

The thesis is, then, that a poorly articulated belief system becomes constraining if and when subjected to "linkage-guidance." This means that elite rich belief systems tend to be *self-constraining,* whereas poor and poorly articulated belief systems are basically *hetero-constraining.* The first provide a self-steering, inner-directed system of orientation; the latter require, *at least for dynamic purposes,* other-direction. The implication is that elite publics are largely in a position to manipulate mass publics. It should be clear, in this latter connection, that "elites" also include so-called counter-elites; the term applies to whichever authorities happen to be recognized by a distinct belief-group.

The argument can be recapitulated as in Figure 7.

I propose to elaborate on the relevance of

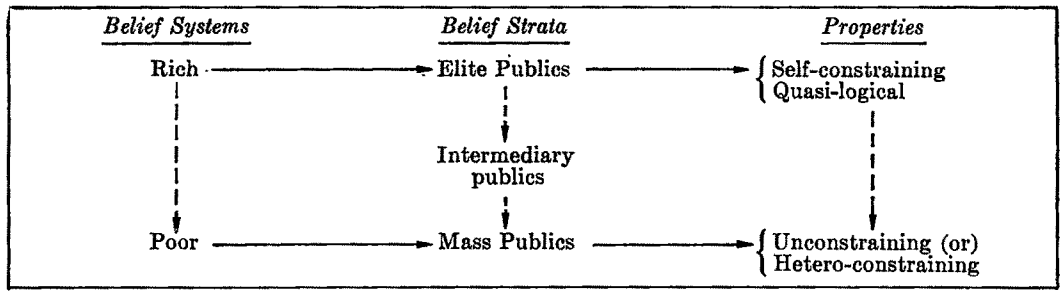
[34] Dahl, *op. cit.,* p. 4.

[35] See Converse, *loc. cit.,* p. 248: "Ideological constraints in belief systems decline with decreasing political information, which is to say that they are present among elites at the 'top' of political systems . . . and disappear rather rapidly as one moves 'downward' into their mass clienteles."

[36] *Ibid.,* p. 216.

[37] *Ibid.,* p. 212.

FIG. 7. Breakdowns and Properties of Belief Systems.



these points in the final section. Meanwhile I only wish to make the point that unless one distinguishes among the various belief strata he is hardly in a position to discuss ideology and pragmatism qua concrete belief systems. Indeed I would go as far as to say that this discussion edges on meaninglessness unless the distinction is sought at the elite belief level.

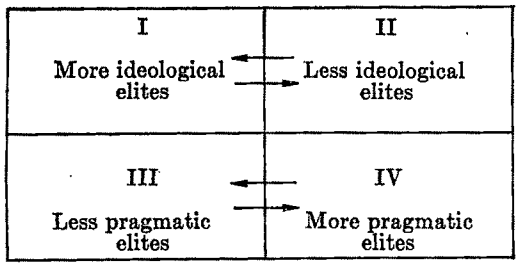
My conjecture is, in fact, that under unexposed "rest conditions" any mass public would display at best a latent political belief system. If so, and always assuming a hypothetical "non-exposure," mass belief systems are likely to be largely undifferentiated, largely amorphous with respect to the ideology-pragmatism distinction. This is to suggest, then, that the ideologic or pragmatic qualification of mass publics—of the *latent* beliefs—is largely decided by the *forensic* beliefs, by the elite belief systems to which mass publics are exposed.[38]

The argument should be placed, however, in perspective, in the sense that the ideology-pragmatism bifurcation should be traced back to the original impetus with which a belief system was launched by the founding fathers. Assuming that the take-off point of an ideological elite is likely to be situated in quadrant I, whereas the take-off point of a pragmatic elite is likely to be in quadrant III, *over time* an ideological elite public is likely to vary across a mix of "closed" strong-weak elements (boxes I and II); whereas a pragmatic elite public is likely to vary across a mix of "open" strong-weak elements (boxes III and IV). Therefore, to the extent that each elite remains as it is—either ideological or pragmatic—the respective areas of variation can be represented as in Figure 8.

By contrast, the *independent* area of variation of mass belief publics is more likely to be the one suggested in Figure 9. According to the hypothesis, at the lower belief stratum there is no reason to assume that in response to a situation of stress shifts from quadrants II to I necessarily represent, in themselves, an increase of ideologism: they are more likely to signify a sheer growth of intolerance. Likewise, shifts from quadrants II to III do not necessarily indicate, in themselves, the acquisition of a pragmatic mentality: they may simply mean a loss of beliefs and thus increased apathy and indifference. As for the blank of quadrant III, the suggestion implicitly conveyed by the table is that an "open-firm" political belief system typically represents an elite achievement. For a latent state of belief left to itself, this appears to be a very unlikely pattern.

[38] The distinction between "latent" and "forensic" is borrowed from Robert E. Lase, *Political Ideology* (New York: The Free Press, 1962), p. 16. In line with my preoccupations, I would say that Lane's "latent ideology" can either become, at the forensic level, an ideological or a pragmatic type of political belief system.

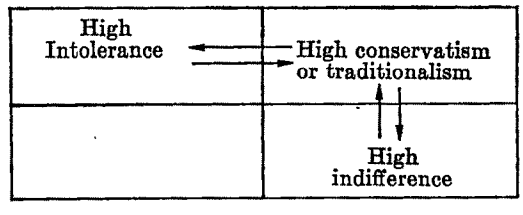
FIG. 8. Mixes and Variations of Elite Belief Publics.



III. IDEOLOGY AND CONFLICT

It should be acknowledged that so far we have been explaining "ideology" more than

FIG. 9. Mixes and Variations of Mass Publics.



using "ideology" to explain. Hopefully we have thrown light in a compartment of our black box. Yet the question remains: having explained the term, what does the term explain? I suggest that the question can be squarely met at least in two respects. First, I shall argue that ideology is an important variable in explaining conflict, consensus and cohesion. Second, I shall argue that ideology is the decisive variable in explaining mass mobilization and manipulation.

With regard to the problem of conflict and consensus the question is: how do two or more belief systems relate to one another? If belief systems are compared among themselves, some belief-elements may be shared, and they are the *common elements;* whereas the elements that differentiate one belief system from another are the *distinctive elements.*[39] It is also convenient to assume that the belief-elements that really matter are the "central" ones. On these premises our earlier distinctions between fixed, firm and flexible belief-elements[40] immediately outline three typical patterns of interrelation between different belief systems and belief groups.

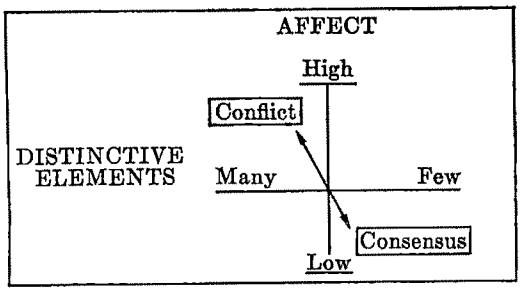
i) If the distinctive elements are *fixed* (i.e., closed and strongly held) two belief systems are *incompatible* or mutually exclusive, and the relations between the corresponding belief groups will definitely be *conflictual*: conciliation is impossible. However the intensity and scope of conflict may vary greatly, for the more numerous the (central) distinctive elements, the greater the hostility; the less numerous, the lesser the occasions of conflict.

ii) If the distinctive elements are *flexible* (i.e., open and weakly held) two belief systems are *coalescent* or fusible, and the relations between the corresponding belief groups will be *consensual*: cooperation is likely. Of course, the fewer the distinctive elements, the greater the amalgamation and the convergence.

[39] Dahl, *op. cit.*, p. 2.

[40] *Supra,* Figure 2: "Typology of belief-elements." For the present discussion the "inelastic elements" will be neglected.

FIG. 10. Conflictual versus Consensual Politics.



iii) If the distinctive elements are *firm* (i.e., open but strongly held) two belief systems are *compatible,* that is, capable of peaceful coexistence, and the relations between the corresponding belief groups will be of the *bargaining* type: mutual adjustment is possible. Of course, the fewer the (central) distinctive elements, the easier the coexistence.

In essence, then, political conflict (in its autonomy vis à vis economic conflict, or conflicts of interest) largely depends on *which* distinctive elements are distributed *how* within a national community or across nations. With regard to the pure and simple distribution—i.e., disregarding for the moment the nature of the elements—political conflict reflects the rise of a controversy which taps the distinctive elements of two (or more) belief systems. Conversely, the sharing of common belief-elements indicates the area in which we obtain political consensus. This preliminary argument can be reformulated also along the lines suggested in Figure 10.

However, Figure 10 accounts for two variables—the numerical magnitude and the emotional intensity of the elements—but fails to account for a third, crucial variable: the nature of the distinctive elements. We are thus referred to the question: which is the distribution of *which* elements? If the central distinctive elements are "closed," the controversy will be ideological; if they are "open" it will be pragmatic. In either case controversy is inevitable, but the chances and the ways of conflict resolution are conspicuously different.

At one end, if the distinctive elements are not only closed but also passionately held we shall have "ideological warfare," the relation is incompatibility and conflict is unmanageable. At the other end, the more the distinctive elements are open and feebly felt, the more we shall obtain "pragmatic transactions" and relations of mutual adjustment.

The same applies to the shared elements. At one end, a belief group whose common elements are closed and strongly held will display "ideological cohesion," that is, strong and lasting solidarity ties, discipline and active dedication to the whole. At the other end, a belief group whose common elements are open and feebly held will display "pragmatic consensus," which means low cohesion, ephemeral and feeble solidarity ties, and a tendency to dissolve into multiple loyalties.

Thus far we have hypothesized relations among homogeneous belief systems, i.e., between two belief systems which are both ideological or both pragmatic. We can also obtain, however, heterogeneous interrelations between pragmatic and ideological belief groups. In this case the

additional complication is a serious communication lag. The two mentalities simply do not fit: their very logic, their *Gestalten*, are different. On the other hand, each belief group is inevitably prompted to project its own *forma mentis* on the opposing group. A blind game results in which misinterpretation, misperception, frustration and a spiral of distrust play the major roles.

For instance, the pragmatic actor tends to assume that interests and conflicts of interests—along a continuum ranging from total coincidence of interest to zero-sum conflict—suffice to explain and to predict political behavior.[41] But in the ideological actor the "logic of interest" combines with a "logic of principles." In fact, ideological politics represents a situation in which the utility scale of each actor is altered by an ideological scale. Hence, and much to the bewilderment of the pragmatist, in this case the logic of interest no longer suffices to explain, and even less to predict, political behavior.

In conclusion, unless we are sensitized to the existence of distinctly ideological publics and belief systems we are likely to miss the very nature of "big conflict." For instance, we are easily misled into believing that ideological conflicts can be reduced to underlying economic conflicts which can be cured with economic medicines. Likewise, we are likely to miss the fact that the dialogue of politics may well be a dialogue among deaf men. On the other hand, the foregoing equally alerts us to the fact that ideological consensus is not the same as pragmatic consensus, and that the in-group cohesion of an ideological community is a far cry from the in-group solidarity of a pragmatic community.

IV. IDEOLOGY AND MASS MANIPULATION

Before turning to the explanatory value of "ideology" vis à vis the unprecedented scale of contemporary mass manipulation—which is also my major and most comprehensive point—it will be necessary to recapitulate, however sweepingly, the route travelled thus far.

Having reference to the more basic features, belief systems vary, as we know, along the following dimensions: i) closed and open cognition, ii) emotive intensity, iii) rich-poor articulation, and iv) constraining power.

With respect to their cultural matrixes belief systems vary, in addition, with respect to the following characteristics: i) accessibility either to argumentative demonstration or to factual evidence; ii) centrality of the belief-elements; iii) level of abstraction; and iv) comprehensiveness.

It is unnecessary to elaborate further on the first set of basic characteristics. As for the additional underpinnings, attention should be called to the following hypotheses.

With regard to *accessibility*, the hypothesis is that to the extent that the ideological mentality receives external communications, it is receptive to rational demonstration, hardly to empirical proofs. Conversely, the pragmatic mentality is open to evidence far more than to the "reasons of reason."

With regard to the *centrality* of the belief-elements, the hypothesis is that in ideological belief systems the "ends" constitute the central elements; whereas in pragmatic belief systems the "means" tend to be more central.

With regard to the *abstraction* ladder, ideological belief systems soar on a far more remote, abstruse and highflown level of discourse than the pragmatic belief systems.

Correlatively, and finally, with regard to *comprehensiveness*, the coverage, or the space for expansion, of ideological belief systems is far more universal and "totalistic" than the coverage of the pragmatic belief systems.

The import of these qualifications is readily apparent in the light of the most crucial aspect of belief systems, namely, their *constraining power*. It was suggested earlier that mass publics are, in general, easily hetero-constrained, in the sense that poorly articulated believers need guidance not only for the horizontal inter-belief linkage, but also for the vertical event-principle linkage.[42] But now the argument can be pinned down, according to the following three points.

First: The greater the centrality of the belief elements designating *ends*, the more a belief system will elicit normative, goal-oriented, if not futuristic or even chiliastic responses and behavior.

Second: The more *abstract* a belief system, the more "what follows from what" (inter-belief linkage), and "which event goes with which principle" (event-principle linkage) escape the grasp of mass publics and require elite guidance. Hence, the more abstract a belief system, the more it allows for elite manipulation and maneuvering.

Third and correlatively: The more a belief

[41] Interest is understood here as the utility scale of each individual, as perceived by the interested party according to the culturally accepted standards of economic rationality.

[42] The need, if not the inevitability of guidance is also the conclusion implicitly conveyed by the literature on the closed and open mind. In particular Rokeach brings out neatly the extent to which cognitive closedness is exposed to manipulation from the authorities.

system transcends common sense spatial and temporal boundaries, that is, the more it obtains a totalistic *comprehensiveness,* the more it calls for elite interpretation and facilitates elite control.

The foregoing forcibly suggests, then, that the hetero-constraining potentiality of belief systems increases the more the system is ideological, and diminishes the more the system is pragmatic. In short, ideologies are the *hetero-constraining* belief systems par excellence. And this is the same as saying that ideologies are the crucial lever at the disposal of elites for obtaining political mobilization and for maximizing the possibilities of mass manipulation. This is, it seems to me, the single major reason that ideology is so important to us. We are concerned about ideologies because we are concerned, in the final analysis, with the power of man over man, with how populations and nations can be mobilized and manipulated all along the way that leads to political messianism and fanaticism.

The focus on ideology as a lever of political mobilization, as an instrument of mass manipulation, satisfies the requirement of causal explanation. At this stage we are not merely saying, "this is *what* the term indicates;" we are also saying "this is *why* we have ideological politics." Yet the reader may feel that this is a too narrow conclusion, and surely a conclusion that requires some justification.

At the outset of this exploration I had two queries in mind. One was: What should we seek to explain? The other one was: What is it that remains unexplained? Clearly these are the two sides of a same question. But the first query is virtually limitless, whereas the second narrows the problem, thereby providing the guideline that I have resolved to follow. By now we know a great deal about why individuals act and react in politics the way they do. Nevertheless we are still at a loss when we come to macro-phenomena and try to understand why the distribution of the "open" and "closed" minds, of high or low affect, does not follow any statistical logic and varies as it does. If so, the concept of ideology still has an important explanatory role to play—under the condition, however, that we sharpen it for the purpose of explaining what other convergent conceptualizations and disciplines leave unexplained. My view is, then, that ideology is crucial to an empirical theory of politics because, and to the extent that, it is conducive to the understanding of *variations* and *varieties.*

Politics is not a monotonic phenomenon. At times it is a mystique, a matter of faith, a secular religion; at other times the logic of politics is no less practical, no less "matter of fact," than the logic of economics. Some political systems display a high extractive capability and succeed in eliciting enthusiastic, passionate and trustful allegiance; other political systems display a low mobilizational and extractive capability. In certain instances we are confronted with monolithic political units characterized by extraordinary cohesiveness; but in other instances we find equivalent political units characterized by a hopeless lack of solidarity ties. Finally, the dialogue of politics is confronted with very different types of "deafness."

Does political science need the variable ideology to explain, at least in part, these varieties and variations? My reply has been in the affirmative, provided we do not overload the concept in pursuing ambitious attempts to explain (worse) that which other disciplines or conceptualizations explain better; and provided, therefore, that the use of "ideology" is restricted to the meanings that appear to have a *unique* explanatory value.

WHO PAYS FOR DEFENSE?[1]

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I. THE OPPORTUNITY COSTS OF DEFENSE

Theories of the economic causes of war are at least as old as capitalism, and have in recent years appeared in myriad forms. Around the turn of the last century J. A. Hobson and Lenin developed their famous arguments about the economic driving forces behind imperialist expansion; American opponents of their country's entry into World War I blamed the lobbying of munitions makers; more recently we have had C. Wright Mills and the New Left. The assertions of these theorists are not always susceptible to scientific examination, but to supplement them there have in the past few years been a number of sound and well-documented studies locating in the national economy the groups that benefit most from military expenditures.[2] Such studies show very effectively which industries, and which states, *gain* disproportionately from defense spending and hence develop some special interest in maintaining or increasing those expenditures. One need not accept Marxist or other extreme positions on the causes of war to find such information relevant to identifying political pressure groups that must be countered or compensated in any effort to reduce the level of military spending.

A question closely related to "Who benefits from defense spending?" is, of course, "Who *pays* for it?"; but curiously this second problem has received very little attention. Nothing comes free, and defense is no exception. In this paper we shall examine some evidence about what segments of the economy and society sacrifice disproportionately when defense spending rises. What kinds of public and private expenditure are diminished, or fail to grow at previously established rates, when military expenditures take a larger proportion of a less than infinitely expansible economy? The exercise will use economic data to address some critical political questions. It should point to particular interests, or pressure groups, that are relatively strong (or weak) and able (or unable) to maintain their accustomed standards of living during periods of international adversity, or to seize the pecuniary opportunities presented by the reduced defense effort that may accompany a relaxation of global tensions. Furthermore, it will enable us, in a sense, to do a "cost-benefit" analysis of war or preparedness, to identify the opportunity costs in the kind and amount of social benefits that are likely to be foregone. Such costs may be entirely in the form of current benefits foregone or, if the nation's resource base is eroded, they may be paid largely by future generations.

The basic question has two prongs, of which only one can be considered here. One part would require detailed data on tax incidences and on wage and price changes. Expanded defense needs are usually financed by a combination of increased taxation and deficit spending. Tax rates would tell us what income or occupational groups suffered disproportionately from assessment increases. Deficit spending in the absence of adequate tax increases normally produces inflation. The wage and price data would show which groups saw inflationary pressures diminishing their real income more sharply than was happening to the average consumer. Gener-

[1] Research for this paper was supported by the World Data Analysis Program of Yale University under grant #GS-614 from the National Science Foundation, and the comparative sections were done under contract #N-0014-67-A-0097-0007 from ARPA, Behavioral Sciences, monitored by the Office of Naval Research. I am grateful to Kenneth Boulding, Peter Busch, John Sullivan, and Murray Weidenbaum for comments. Of course no person or agency is responsible for errors or for the opinions expressed.

[2] See, for example, Walter Isard and James Ganschow, *Awards of Prime Military Contracts by County, State, and Metropolitian Area of the United States, Fiscal Year 1960* (Philadelphia: Regional Science Research Institute, 1962) and Walter Isard and Gerald Karaska, *Unclassified Defense Contracts: Awards by County, State and Metropolitian Area of the United States, Fiscal Year 1962* (Philadelphia: World Friends Research Center, 1962). Murray L. Weidenbaum, "Problems of Adjustment for Defense Industries," in Emile Benoit and Kenneth E. Boulding (eds.), *Disarmament and the Economy* (New York: Harper & Row, 1963) presents data on the distribution of contracts by industry. See also James L. Clayton, "Defense Spending: Key to California's Growth," *Western Political Quarterly*, 15 (June 1962), 280–293. *Science*, 161 (1968), p. 448 reports the receipts of various universities for Department of Defense-sponsored research in the physical, biological, and social sciences. On Canada, see Gideon Rosenbluth, *The Canadian Economy and Disarmament* (New York: St. Martin's Press, 1967).

ally, one would expect owners of common stocks and land, and union laborers with cost-of-living clauses in their contracts, to suffer least. White collar workers and, especially, pensioners and poor unorganized laborers, have the most nearly fixed incomes and the greatest vulnerability.

While there is some information on these matters, a high degree of precision and specificity is required before the data can yield reliable answers, and I have chosen not to develop that side of the problem here. What I have done is to examine information on *expenditures* by G.N.P. categories, by function, and by governmental unit to see what kinds of alternative spending bear the brunt of heavy military spending. For the United States we have this data for the period 1939–1968 or 1938–1967. This allows us to see the effects of two earlier wars (World War II and the Korean War) as well as the burdens of the current Vietnam venture. Toward the end of the paper we shall compare the American experience with that of several other developed Western states, albeit for a shorter time span.

First, an overview of the changing level of defense expenditures may be helpful. For 1939, in what was in many ways the last peacetime year this nation has experienced, defense expenditures were under $1.3 billion. They rose rapidly with the new preparedness, to a still-unsurpassed peak of $87.4 billion in 1944. The 1968 figure (annual estimate based on the first three quarters) was by contrast $78.4 billion, reflecting a build-up, for the Vietnam war, from levels of around $50 billion in the early-to-mid 1960's. The raw dollar figures, however, are deceptive because they reflect neither inflation nor the

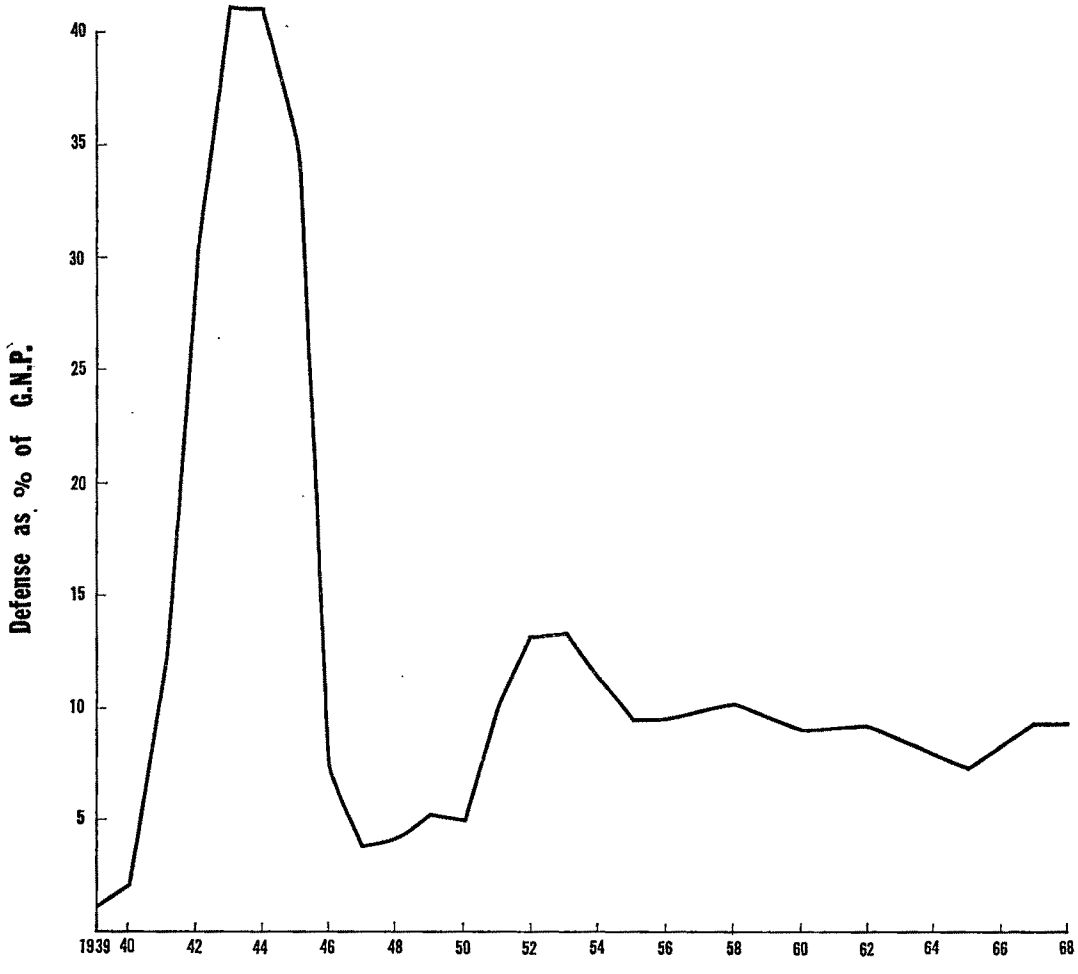


FIG. 1. Defense Expenditures as a Percentage of U.S. G.N.P., 1939–1968.
Source: See Table 1.

steady growth in the economy's productive capacity that makes a constant defense budget, even in price-adjusted dollars, a diminishing burden. Figure 1 shows the trend of military expenditures as a percentage of gross national product over the past 30 years.

We immediately see the great burdens of World War II, followed by a drop to a floor considerably above that of the 1930's. The cold war, and particularly the Korean action, produced another upsurge in the early 1950's to a level that, while substantial, was by no means the equal of that in the Second World War. This too trailed downward after the immediate emergency was past, though again it did not retreat to the previous floor. Not since the beginning of the cold war has the military accounted for notably less than five per cent of this country's G.N.P.; not since Korea has it had as little as seven per cent.[3] Finally, we see the effect of the Vietnam build-up, moving from a recent low of 7.3 per cent in 1965 to 9.2 per cent in 1968. This last looks modest enough, and is, when compared to the effects of this nation's two previous major wars. At the same time, it also represents a real sacrifice by other portions of the economy. The 1968 G.N.P. of the United States was well in excess of $800 billion; if we were to assume that the current war effort accounts for about 2 per cent of that (roughly the difference between 7.3 per cent and 9.2 per cent) the dollar amount is approximately $16 billion. That is in fact too low a figure, since some billions were already being devoted to the war in 1965, and direct estimates of the war's cost are typically about $25 to $30 billion.[4] The amounts in question, representing scarce resources which might be put to alternative uses, are not trivial.

[3] This repeated failure to shrink the military establishment back to its prewar level is a phenomenon of some interest to students of the dynamics of international arms races and/or Parkinson's Law. It shows up even more clearly in the data on military personnel, and goes back almost a century to demonstrate the virtual doubling of the armed forces after every war. From 1871 to 1898 the American armed forces numbered fewer than 50,000; after the Spanish-American war they never again dropped below 100,000. The aftermath of World War I saw a leveling off to about 250,000, but the World War II mobilization left 1,400,000 as the apparent permanent floor. Since the Korean War the United States military establishment has never numbered fewer than about 2,500,000 men. See Bruce M. Russett (ed.), *Economic Theories of International Politics* (Chicago: Markham, 1968), p. 521. Should the post-Vietnam armed forces and/or defense portion of the G.N.P. prove to be higher than in the early and mid-1960's, that will represent another diversion from private or civil public resources and a major indirect but perhaps very real "cost" of the war.

II. CRITERIA FOR EVALUATION

The method of analysis is straightforward. I have computed various kinds of private and public expenditures as proportions of G.N.P., and regressed them against the proportion of G.N.P. represented by defense spending. Both linear and curvilinear regressions (second order curves) were tried, but the curvilinear fit will be reported only when it improves notably on the results of the linear model. Defense is the independent variable, with the implication that the others are dependent upon it; that is, that increases in civil expenditures are made possible by relative reductions in defense spending, and that increases in defense must force relative reductions, whether deliberate or unintended, in nonmilitary items.[5] The causal chain is of course

[4] Much of the war's costs are undoubtedly absorbed by other military categories, such as the number of troops kept in other parts of the world, and reductions in new procurement and maintenance of old equipment not being used in the war zone. Even so, it is likely that our method understates the cost of the war to the civilian sectors of the system. President Johnson's State of the Union Message in January 1968 put the annual cost at $25 billion in 1967. For 1968 it was nearly $29 billion.

[5] It must be made clear that this procedure is not the only plausible way of processing these data. For instance, using another theoretical model one might compute the *differences* between spending levels from one year to the next (t_1-t_0) and run the regression on them, giving different results. The procedure I used here assumes that it is a high *level* of defense spending that gets in the way of particular civilian expenditures; the alternative assumes that it is a sharp *increase*, regardless of level, that forces civilian cutbacks. In fact, though I report only the former, I did also process the data by the other method, finding on the whole lower r^2s. In part, I hypothesize that the reason for weaker relationships with the difference model stems from varying time lags for various types of expenditure. For example, many purchases of suppliers may be sharply reduced simultaneously with the new military demands; reductions in tenured personnel, or liquidation of construction commitments, may take one or two years. To the degree that this is true, analysis of the aggregates with any *particular* time relationship specified (e.g., simultaneous differences, or

not fully demonstrable with regression analysis, but seems generally plausible.

I assume that defense spending has to come *at the expense* of something else. In the formal sense of G.N.P. proportions that is surely true, but it is usually true in a more interesting sense as well. Economics is said to be the study of the allocation of scarce resources; despite some periods of slack at the beginning of war-time periods (1940-41 and 1950) resources have generally been truly scarce during America's wars. Major civilian expenditures have not only lost ground proportionately (as would nevertheless happen from a military spending program financed entirely out of slack) but have failed to grow at their accustomed rates, have lost ground in constant dollars as a result of inflation, or even have declined absolutely in current dollars. During World War II, for example, such major categories as personal consumption of durable goods, all fixed investment, federal purchases of non-military goods and services, and state and local expenditures all declined sharply in absolute dollar amounts despite an inflation of nearly 8 per cent a year.

Some observers might argue that high levels of military spending are introduced to take up the slack and maintain demand in an otherwise depression-prone economy. If this were the case, opportunity costs would be minimal. But there is little evidence for that proposition in the American experience of recent decades. Certainly the Vietnam experience does not support it. I assume, *pace* "Iron Mountain," that with the demonstrable public and private needs of this society, and with modern tools of economic analysis and manipulation, full or near-full employment of resources would be maintained even in the face of major cuts in military spending.[6] Because of the skills with which economic systems are now managed, in modern economies defense expenditures are much more likely to force tradeoffs than they were some 30 years ago. Thus the original formulation, "Who pays for defense?" is not inappropriate.

Defense expenditures are not necessarily without broader social utility. Spending for military research and development produces important technological spill-overs into the civilian sector. The education, skills, and physical conditioning that young men obtain during service in the armed forces are likely to benefit them and their society, when they return to civilian life. Rarely, however, is the achievement of such benefits as spillovers the most efficient way to obtain them. While scientific research may be serendipitous, the odds are better that a new treatment for cancer will come from medical research than from work on missile systems. Hence we must still consider the tradeoffs that appear in the official functional categories as real costs, though not quite as heavy costs as a literal interpretation of the dollar amounts would imply.

Finally, we must recognize that some civilian expenditures, for health, for education, and for research, have been stimulated by cold war and ultimately military requirements. Various pro-

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one-year lag) would show only a weak relationship.

Also, the r^2's and regression coefficients are sensitive to the *particular* set of *years* examined. I also analyzed the World War II years and the post-war years separately, producing different and generally lower r^2's than for the entire period. Hence much of the variance found in this paper stems from differences between rather than within those two periods. Furthermore, inclusion of the war-time years with the others produces a distribution of defense percentages that would offend a purist data analyst—those for 1943 and 1944 are clear outliers nearly three standard deviations from the mean, and hence they exert disproportionate weight in the analysis. Excluding 1939–1945, however, would sharply reduce both the number of data points and the range of experience being analyzed. Thus I have left the earlier years in, but have also been careful to examine the scattergrams to be sure that the r^2's were not deceptively high or low because of any inordinate statistical effect of the wartime peaks.

Although I believe my handling of these two problems was appropriate, the caveat that the conclusions depend on these assumptions, and could be modified by others, is a serious one. The data are available on request from the World Data Analysis Program, 89 Trumbull Street, Yale University, New Haven, Connecticut 06520. Further analysis of the methodological problems with these data will appear in my contribution to volume 5 of *Mathematical Applications in Political Science* (1970).

[6] This position is adhered to by most economists. See, for example, Benoit and Boulding (eds.), Disarmament and the Economy; U.S. Arms Control and Disarmament Agency, *Economic Impacts of Disarmament* (Washington: U.S. Government Printing Office, 1965); Emile Benoit, "The Monetary and Real Costs of National Defense," *American Economic Review*, 58 (May 1968), 398–416, and Walter Isard and Eugene Schooler, "An Economic Analysis of Local and Regional Impacts of Reduction of Military Expenditures," *Peace Research Society (International) Papers*, I (1964).

grams of the 1950's fit this characterization, when a need more for long-run girding of the loins than for more immediate military capabilities was widely seen. Still, this is appropriate to precisely the kind of question we shall be asking. If the civilian and military expenditures consistently compete for scarce resources, they will be highly negatively correlated; if they both are driven by the same demands, they will be positively correlated. If they generally compete but sometimes are viewed as complementary, the negative correlation will be fairly low.

An evaluation of the experience of this and other nations requires some explicit criteria. There is room for serious argument about what those criteria should be, but I will suggest the following:

1) It is bad to sacrifice future productivity and resources for current defense or war-fighting activities; insofar as possible such activities "should" be financed out of current consumption. Such an assumption might be easily challenged if it were offered as a universal, but for the developed countries of North America and Western Europe in recent years it seems defensible. All of them are now, relative to their own past and other nations' present, extremely affluent with a high proportion of their resources flowing into consumption in the private sector. Furthermore, for the years being analyzed the demands of defense have not usually been terribly great. Since World War II ended, none of these countries has had to devote more than about 10 per cent of its G.N.P. to military needs, save for the United States during the Korean War when the figure rose to just over 13 per cent. It surely is arguable that such needs rarely require substantial mortgaging of a nation's future.

a) By this criterion one would hope to see periodic *up-swings* in defense requirements financed largely out of personal consumption, with capital formation and such social investment in the public sector as health and education being insensitive to military demands.

b) Another aspect of our criterion, however, is the anticipation that in periods of *declining* military needs the released resources would largely be *kept* for investment and education rather than returned to private consumption. In a strong form the criterion calls for a long-term secular increase in the proportion of G.N.P. devoted to various forms of investment, and with this secular trend realized through a fluctuating line made up of a series of upward slopes followed by plateaus, insensitive to rising defense needs but responsive to the opportunities provided by relaxations in the armament pace.

2) Another point of view, partially in conflict with the last comment, would stress the need for a high degree of *insulation from political shocks*. A constant and enlarging commitment to the system's social resources is necessary for the most orderly and efficient growth of the system, avoiding the digestive problems produced by alternate feast and famine. Some spending, for instance capital expenditures for buildings, may be only temporarily postponed in periods of fiscal stringency, and may bounce back to a higher level when the pressure of defense needs is eased. To that degree the damage would be reduced, but not eliminated. In the first place, school construction that is "merely" postponed four years will come in time to help some students, but one age cohort simply loses out. Secondly, boom and bust fluctuations, even if they do average out to the socially desired dollar level, are likely to be inefficient and produce less real output than would a steadier effort.

III. GUNS, BUTTER, AND STRUCTURES

Calculation of a nation's G.N.P. is an exercise in accounting; economists define the gross national product as the sum of expenditures for personal consumption, investment or capital formation, government purchases of goods and services, and net foreign trade (exports minus imports). Each of these categories can be broken down further. Private consumption is summed from expenditures on durable goods (e.g., automobiles, furniture, appliances), non-durables (e.g., food, clothing, fuel) and services (airline tickets, haircuts, entertainment); investment includes fixed investment in non-residential structures, producers' durable equipment (e.g., machinery), residential structures, and the accumulation or drawing down of stocks (inventories); government purchases include both civil and military expenditures of the federal government, and spending by state and local units of government. Except for inventories (which fluctuate widely in response to current conditions and are of little interest for this study) we shall look at all these, and later at a further breakdown of public expenditures by level and function. Table 1 gives the r^2 (proportion of variance in the dependent variables accounted for by defense), the regression coefficients, and an index of proportionate reduction. The r^2 tells *how closely* the two variables vary together, and the regression coefficient tells *the amount* in dollars by which the dependent variable changes in response to a one dollar increase in defense. The proportionate reduction index shows the damage suffered by each category relative to its "normal" base. It assumes for illustration a total G.N.P. of $400

TABLE 1. THE EFFECT OF DEFENSE SPENDING ON CIVILIAN ACTIVITIES IN THE UNITED STATES, 1939–68

	r^2 (linear reg.)	Regression Coefficient	Index of Proportionate Reduction
Personal Consumption (Total)	*.84*	*−.420*	*−.041*
Durable Goods	.78	−.163	−.123
Nondurable Goods	.04	−.071	−.014
Services	.54	−.187	−.050
Fixed Investment (Total)	*.72*	*−.292*	*−.144*
Nonresidential Structures	.62	−.068	−.140
Producers' Durable Equipment	.71	−.110	−.123
Residential Structures	.60	−.114	−.176
Exports	.67	−.097	−.115
Imports	.19	−.025	−.037
Federal Civil Purchases	.38	−.048	−.159
State & Local Gov't Consumption	.38	−.128	−.105

Source: *Survey of Current Business*, 45, 8 (August 1965), pp. 24–25, 46, 11 (November 1966), p. S-1, and 47, 11 (November 1968), p. S-1. Data for 1968 are provisional figures for first three quarters only.

billion, an increase of $25 billion in defense spending from the previous period, and that the "dependent" expenditure category had previously been at that level represented by its mean percentage of G.N.P. over the 1939-68 period. This last measure is important for policy purposes, since the *impact* of the same dollar reduction will be far greater to a $100 billion investment program than to a $500 billion total for consumer spending.[7]

In general, the American experience has been that the consumer pays most. Guns do come at the expense of butter. Changes in defense expenditure account for 84% of the variance in total personal *consumption*, and the regression coefficient is a relatively high −.420 (That is, a one dollar rise in defense expenditures will, all else being equal, result in a decline of $.420 in private consumption).

Of the sub-categories, sales of consumer durables are most vulnerable, with 78 per cent of their variance accounted for by defense. Spending on services is also fairly vulnerable to defense expenditures, with the latter accounting for 54 per cent of the variance. But the *linear* regression between defense and non-durables is not nearly so high, with an r^2 of but .04. This is not surprising, as needs for non-durables are virtually by definition the least easily postponed. The low r^2 from the linear model is deceptive, however, because a look at the plot of defense against non-durable consumer goods purchases shows that the proportion of G.N.P. spent on the latter *increased* somewhat in the early 1940's, when war costs were reaching their peak; it was as high in 1942-45 as in many prewar and postwar years. Using a curvilinear regression with a second-degree curve adds another 22 per cent of the total variance to the 4 per cent accounted for by the linear model. The explanation is fairly simple: During the war years new consumer durables such as automobiles and appliances were virtually unavailable, since the factories that normally produced them were then turning out war materiel. Similarly, due to manpower shortages almost all services were expensive and in short supply, and long-distance travel was particularly discouraged ("Is this trip necessary?"). Hence, to the degree consumers' spending power was not mopped up by taxes, or saved, an unusually high proportion was likely to go into non-durables.

Investment (fixed capital formation) also is typically hard-hit by American war efforts and, with its consequence of a smaller productive capacity in later years, diminished investment is a particularly costly loss. The r^2 of .72 is only a little less than that for defense on consumption, and the regression coefficient is a substantial −.292.[8] The slope is of course much flatter than that for defense and consumption (with a coefficient of −.420) but that is very deceptive considering the "normal" base from which each starts. Over the 30 years for which we have data, the mean percentage of G.N.P. consumption typically was about five times as great as investment. Thus in our hypothetical illustration a $25 billion increase in defense costs in a G.N.P. of $400 billion would, ceteris paribus, result in a drop in consumption from approximately $256 billion to roughly $245, or only a

[7] This index is computed by the formula

$$\text{IPR} = \frac{25 \text{ (reg. coef.)}}{400 \text{ (mean prop. of G.N.P.)}}$$

for the dependent variable. This choice of illustrative values for the G.N.P. and defense increases does *not* of course imply that the impact is thus in any *particular* build-up.

[8] The high r^2 is nonetheless a bit deceptive, as a close examination of the plot discloses. If one looks at the periods of moderate defense expenditure since 1940 one finds only a mild relationship between the two variables. Most of the variance is concentrated on the differences between the moderate and high defense groups, and *within* the latter.

little over 4 per cent of total consumption. Investment on the other hand would typically fall from $51 billion to about $44 billion, or more than 14 per cent. *Proportionately,* therefore, investment is much *harder* hit by an expansion of the military establishment than is consumption. Since future production is dependent upon current investment, the economy's *future* resources and power base are thus in a very real sense much more severely damaged by the decision to build or employ current military power than is current indulgence. According to some rough estimates, the marginal productivity of capital in the United States is between 20 and 25 per cent; that is, an additional dollar of investment in any single year will produce 20–25 cents of annual additional production in perpetuity.[9] Hence if an extra billion dollars of defense in one year reduced investment by $292 million, thenceforth the level of output in the economy would be *permanently* diminished by on the order of $65 million per year.

This position is modified slightly by the detailed breakdown of investment categories. Residential structures (housing) shows the lowest r^2 of the three, but its regression coefficient is the strongest and it takes the greatest proportionate damage. Within the general category of investment, therefore, non-residential structures and equipment usually hold up somewhat better proportionately than does housing. Doubtless this is the result of deliberate public policy which raises home interest rates and limits the availability of mortgages while trying at the same time to maintain an adequate flow of capital to those firms needing to convert or expand into military production. One other feature should be noticed, however. The curvilinear model (second order curve) adds as much as 43 per cent of the remaining variance with non-residential structures. Relative spending goes *up* with increments to very low levels of defense spending to where defense equals about 9 per cent of G.N.P. It then flattens out until, by around the 13 per cent mark, it begins to fall off sharply. This phenomenon is probably the result of construction of new factories for war production and, especially, facilities for military bases and camps when a large army must be mustered quickly from low manpower levels.

The nation's international *balance of payments* is often a major casualty of sharp increases in military expenditures; the present situation is not unusual. Some potential exports are diverted to satisfy internal demand, others are lost because domestic inflation raises costs to the point that the goods are priced out of the world market. *Imports* may rise directly to meet the armed forces' procurement needs—goods purchased abroad to fill local American military requirements show up as imports to the national economy—and other imports rise indirectly because of domestic demand. Some goods normally purchased from domestic suppliers are not available in sufficient quantities; others, because of inflation, became priced above imported goods. If the present situation is "typical," the Vietnam war's cost to the civilian economy would be responsible for a loss of more than one and one-half billion dollars in exports.

The import picture is more complicated. According to the sketch above, imports should *rise* with defense spending, but the r^2 in the table is very low and the slope of the regression line is actually *negative.* This, however, is deceptive and gives a conclusion quickly reversed by a curvilinear regression. The second order curve shows the expectable sharp *positive* relation between defense and imports up to a level of defense more or less equal to 13 per cent of G.N.P. Only then does it flatten out and then turn down dramatically. The four years of World War II show unusually low importation activity, and the reasons are obvious. A combination of enemy occupation of normal sources of goods for the U.S., enemy surface and submarine activity in the sea lanes, and the diversion of our allies' normal export industries to serve *their* war needs drastically reduced America's opportunity to import. To assess the impact of defense expenditures on imports in a less than global war one must omit the World War II data from the analysis. Doing so produces the expected positive regression coefficient, on the order of +.060. This suggests that the current effect of Vietnam may be to add, directly and indirectly, over $1 billion to the nation's annual import bill.[10] Coupled with the loss of exports,

[9] Robert M. Solow, *Capital Theory and the Rate of Return* (Amsterdam: North Holland, 1963).

[10] The costs of military procurement abroad can of course be figured more precisely and directly than we have done here, but by missing the indirect effects of inflation and diverted demand such a computation would understate the loss. Defense Department calculations of the direct cost do in fact come to but $1.5 billion; a complex independent analysis that includes the indirect effects suggests $4.0 billion, or that without the war the United States would actually have maintained a balance of payments surplus. See Leonard Dudley and Peter Passell, "The War in Vietnam and the United States Balance of Payments," *Review of Economics and Statistics,* 50 (November 1968), 437–442.

the total damage to the balance of payments on current account (excluding capital transfers) is in the range $2.5–$3.0 billion. That still does not account for the entire balance of payments deficit that the United States has experienced (recently as high as 3.4 billion annually) but it goes a long way to explain it.

IV. THE PUBLIC SECTOR

In the *aggregate* there is no very strong impact of defense on *civil public expenditures*. The linear r^2's are a comparatively low .38; the regression coefficients only −.048 for federal civil purchases and −.013 for state and local governments. For the federal government a curvilinear regression helps, however, adding approximately one-sixth of the variance left unexplained by the linear model. During the four peak years of World War II changes in federal civil expenditures were essentially unrelated to changes in defense spending, hence the curve at that end is flattened out. But it is simultaneously steepened, from the linear model, for the 1 per cent to 15 per cent defense range.[11] Samuel P. Huntington notes that "Many programs in agriculture, natural resources, labor and welfare dated back to the 1930's or middle 1940's. By the mid-1950's they had become accepted responsibilities of the government," and hence politically resistant to the arms squeeze. If so, the overall inverse relationship we do find may be masking sharper changes in some of the less well-entrenched sub-categories of central government budgeting. We shall investigate this below.[12]

TABLE 2. THE EFFECT OF DEFENSE SPENDING ON PUBLIC CIVIL ACTIVITIES IN THE UNITED STATES, FISCAL YEARS 1938–67

	r^2 (linear reg.)	Regression Coefficient	Index of Proportionate Reduction
Education—Total	*.35*	*−.077*	*−.139*
Institutions of Higher Ed.	.12	−.013	−.146
Local Schools	.34	−.053	−.125
Other Ed.	.19	−.014	−.265
Federal Direct to Ed.	.16	−.013	−.309
Federal Aid to State & Local Gov'ts for Ed.	.08	−.004	−.140
State & Local Gov't for Ed.	.24	−.060	−.124
Health & Hospitals—Total	*.32*	*−.017*	*−.113*
Total Hospitals	*.30*	*−.014*	*−.123*
Fed. for Hospitals	.25	−.004	−.130
State & Local for Hospitals	.29	−.011	−.120
Total Other Health	*.22*	*−.003*	*−.087*
Fed. for Health	.06	−.001	−.101
State & Local for Health	.45	−.002	−.078
Welfare—Total	*.54*	*.019*	*−.128*
Fed. Direct for Welfare	.13	.003	−.493
Fed. Aid to State & Local Gov'ts for Welfare	.17	−.005	−.087
State & Local for Welfare	.30	−.011	−.134

Source: G.N.P. same as for Table 1, but adjusted for fiscal year. Others: U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957* (Washington: U.S. Government Printing Office 1960), pp. 719, 723–27; *Statistical Abstract of the United States, 1963* (Washington: U.S. Government Printing Office, 1963), pp. 392, 419, 422; *Statistical Abstract of the United States, 1967* (Washington: U.S. Government Printing Office, 1967), pp. 390, 421, 423; U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957; Continuation to 1962 and Revisions* (Washington: U.S. Government Printing Office, 1965), pp. 98–101, 148–50. Budget data for 1967 from U.S. Bureau of the Census, *Governmental Finances in 1966–67, Series GF67-No. 3* (Washington: U.S. Government Printing Office, 1968), pp. 15, 17–19. Data for Institutions of Higher Education, Local Schools, Other Education, Total, Federal, and State and Local for both Health and Hospitals include 1938, 1940, 1942, 1944, 1946, 1948, 1950, 1952–67; others same except for addition of 1951.

Some restraint is required when relating state and local government expenditures to defense. There really is no relationship except *between*

[11] Although recent experience may make it seem obvious that public civil expenditures are likely to be inversely related to defense shares, this perception has not always been universal. Several years ago W. Glenn Campbell reported a strong inverse relationship for the 1953–63 period, in "Assuring the Primacy of National Security," in David M. Abshire and Richard V. Allen, *National Security: Political, Military and Economic Strategies in the Decade Ahead* (New York: Praeger, 1963), pp. 963–984. Otto Eckstein, however, noted some questionable assumptions in Campbell's analysis and followed with a stronger expression of doubt: "I think that historical experience has been that governments are either stingy, or they're spenders. And if they're stingy about defense, they're stingy about everything. I would say that the historical record suggests that the association between civilian spending and military spending is positive, not negative." "Discussion" in *ibid.*, p. 1012.

[12] *The Common Defense* (New York: Columbia University Press, 1961), p. 208. Further masking of the impact on actual programs may stem from the inability of government agencies to reduce costs for building maintenance and tenured employees, thus forcing them in dry times to cut other expenses disproportionately.

the points above and below the 15 per cent mark for defense. During World War II state and local government units did have their spending activities curtailed, but overall they have not been noticeably affected by defense purchases. Quite to the contrary, spending by state and local political units has risen steadily, in an almost unbroken line, since 1944. The rise, from 3.6 per cent of the G.N.P. to 11.2 per cent in 1968, has continued essentially heedless of increases or diminution in the military's demands on the economy.[13]

When we look at the breakdowns by function, however, it becomes clear that the effect of defense fluctuations is serious, if less distinct than for G.N.P. categories. Three major items —education, health, and welfare—were selected for further analysis, on the grounds that one might reasonably hypothesize for each that expenditure levels would be sensitive to military needs, and, for the first two, that a neglect of them would do serious long-term damage to the economy and social system of the nation.

All three are sensitive to defense spending, with *welfare* somewhat more than the others as is not surprising. In most of this analysis reductions in expenditure levels that are forced by expanded defense activities represent a *cost* to the economic and social system, but welfare is different. Insofar as the *needs* for welfare, rather than simply the resources allocated to it, are reduced, one cannot properly speak of a cost to the economy. Rather, if one's social preferences are for work rather than welfare, the shift represents a *gain* to the system. Heavy increases in military pay and procurement do mean a reduction in unemployment, and military cutbacks are often associated with at least temporary or local unemployment. The effect seems strongest on state and local governments' welfare spending. In fact, the inverse relationship between defense and welfare at most spending levels is *understated* by the linear regression model. When a second-order curve is applied to the regression of defense on total welfare expenditures, 32 per cent of the variance remaining after the linear regression is added, to make a total of more than two-thirds of the variance. At all but the highest levels of defense spending, achieved in World War II, the inverse relationship is very steep, with small increases in military needs having a very marked dampening effect on welfare costs. But manpower was quite fully employed during *all* the years of major effort in World War II, so ups and downs in defense needs during 1942–45 had little effect, and the curve flattens out.

Both for education and for health and hospitals their relationship to the immediate requirements of national defense is less powerful (lower r^2), but nonetheless important. Furthermore, the regression line is quite steep for education, and since the mean share of G.N.P. going to education is only 3.5 per cent for the period under consideration, the proportionate impact of reductions is severe.

A widespread assumption holds that public expenditures on *education* have experienced a long-term secular growth in the United States. That assumption is correct only with modifications. The proportion of G.N.P. devoted to public education has increased by three quarters over the period, from 3.0 per cent in 1938 to 5.3 per cent in 1967. But it has by no means been a smooth and steady upward climb. World War II cut deeply into educational resources, dropping the educational percentage of G.N.P. to 1.4 in 1944; only in 1950 did it recover to a level (3.6) notably above that of the 1930's. Just at that point the Korean War intervened, and education once more suffered, not again surpassing the 3.6 level before 1959. Since then, however, it has grown fairly steadily without being adversely affected by the relatively modest rises in defense spending. Actually, educational needs may have benefitted somewhat from the overall decline in the military proportion of the economy achieved between the late 1950's and mid 1960's. The sensitivity of educational expenditures to military needs is nevertheless much more marked on the latter's upswings than on its declines. Education usually suffers very immediately when the military needs to expand sharply; it recovers its share only slowly after defense spending has peaked. Surprisingly, *federal* educational expenditures are less related (lower r^2) than is spending by state and local units of government; also, local schools at the primary and secondary level are more sensitive than are public institutions of higher education (whose share has grown in every year since 1953).

Public expenditures for *health* and hospitals are only a little less sensitive to the pressures of

[13] It might be thought that this would be a case where, if there were no immediate effects of defense needs on state and local government finances, defense might nevertheless force some delayed cutbacks by the areal units. But this too is not the case, since an effort to lag local expenditures a year or two behind defense made no improvement in the fit. Apparently the federal and the areal units of government are sufficiently independent in their major revenue sources that fluctuations in the needs of the former do not seriously hamper the latter.

defense than are dollars for education. Here again the image of a long-term secular growth deceptively hides an equally significant pattern of swings. They accounted for a total of .77 per cent of G.N.P. in 1938; as with education this was sharply cut by World War II and was not substantially surpassed (at 1.00 per cent) until 1950. Once more they lost out to the exigencies of defense in the early 1950's, and bounced back slowly, at the same rate as did education, to recover the 1950 level in 1958. Since then they have continued growing slowly, with a peak of 1.23 in 1967. Thus, the pattern of health and hospitals is almost identical with that for education—some long-term growth, but great cutbacks in periods of heavy military need and only slow recovery thereafter. In detail by political unit the picture is also much the same—despite reasonable *a priori* expectation, federal spending for this item is less closely tied to the defense budget than is that by state and local governments. It should also be noted that though the r^2 is much the same, the *impact* of defense on health and hospitals is slightly less severe than on education.

It seems fair to conclude from these data that America's most expensive wars have severely hampered the nation in its attempt to build a healthier and better-educated citizenry. (One analyst estimates that what *was* done to strengthen education accounted for nearly half of the United States per capita income growth between 1929 and 1957.)[14] A long-term effort has been made, and with notable results, but typically it has been badly cut back whenever military needs pressed abnormally hard.

It is too soon to know how damaging the Vietnam war will be, but in view of past regularities one would anticipate significant costs. The inability to make "investments" would leave the nation with a smaller resource base of skill and well-being than would otherwise be the case. We can already see the effect of the war on fixed capital formation, discussed earlier in this paper. Consumption absorbed a larger *absolute* decline in its share of G.N.P. between 1965 and 1968 than did fiscal investment—from 63.3 to 62.1 per cent in the first instance, from 14.3 to 13.8 per cent in the second, but given the much smaller base of investment, the *proportionate* damage is about twice as great to the latter. In most of the major categories of public social "investment," nevertheless, the record is creditable. Despite a rise from 7.6 to 9.1 per cent in the defense share between 1965 and 1967, the total public education and health and hospitals expenditure shares went up 4.5 to 5.3 and 1.17 to 1.23 respectively. And even federal spending for education and health, though not hospitals, rose. There are of course other costs involved in the inability to *initiate* needed programs—massive aid to the cities is the obvious example. But on maintaining or expanding established patterns of expenditure the score is not bad at all.

The pattern of federal expenditures for *research and development* indicates some recent but partially hidden costs to education and medicine. From 1955 through 1966 such expenditures rose spectacularly, and every year, from $3.3 billion to $14.9 billion.[15] Obviously such a sky-rocketing growth could not continue indefinitely; not even most of the beneficiary scientists expected it to do so. Still, if one regresses R & D expenditures against G.N.P. over the entire period 1955 to 1968, the r^2 is a very high .93 and the "estimated" level for 1968 is $19.4 billion, instead of the actual level of $16.3 billion. The actual level of expenditures in fact fell below the estimated level for the first time in 1966—the first year since 1961 when the defense share of G.N.P. showed any notable increase.

Finally, we must note a very important sense in which many of these cost estimates are substantially underestimated. The entire analysis has necessarily been done with expenditure data in current prices; that is, not adjusted for inflation. Since we have been dividing each expenditure category by G.N.P. in current dollars that would not matter *providing that price increases were uniform throughout the economy.* But if prices increased faster in say, education or

[14] Edward F. Denison, *Sources of Economic Growth in the United States and the Alternatives Before US* (C.E.D., 1962) as cited in Ruth P. Mack, "Ecological Process in Economic Change," *American Economic Review,* 58 (May, 1968), 47. See also Gary S. Becker, *Human Capital: A Theoretical and Empirical Analysis* (New York: National Bureau of Economic Research and Columbia University Press, 1964) and Theodore W. Schultz, "Investment in Human Capital," *American Economic Review,* 51 (March 1961), 1–17. On this idea of educated manpower as a form of national capital see also the chapter by Kenneth Boulding in Walter Adams (ed.), *The Brain Drain* (New York; Macmillan, 1968).

[15] U.S. Bureau of the Census, *Statistical Abstract of the United States, 1968* (Washington: U.S. Government Printing Office, 1968), p. 526, and *Statistical Abstract of the United States, 1963* (Washington: U.S. Government Printing Office, 1963), p. 544. Comparable data for earlier years are not available, so this variable was not used in the computations for Table 2.

health, than did prices across the board, the *real* level of expenditure would be exaggerated. And as anyone who has paid a hospital bill or college tuition bill recently knows, some prices have increased faster than others. From 1950 through 1967 the cost of medical care, as registered in the consumer price index, rose by 86.2 per cent. Thus even though the health and hospital share of public expenditure rose in *current* prices, the *real share* of national production bought by that spending *fell* slightly, from 1.00 per cent to about .99 per cent. Presumably the difference has been made up in the private sector, and benefits have been heavily dependent upon ability to pay. Comparable data on educational expenses are less easy to obtain, but we do know that the average tuition in private colleges and universities rose 39 per cent, and in public institutions 32 per cent, over the years 1957–1967.[16] This too is faster than the cost of living increase over those years (not more than 20 per cent), but not enough to wipe out a gain for government education expenditures in their share of real G.N.P.

In evaluating the desirability of an expanded defense effort, policy-makers must bear in mind the opportunity costs of defense, the kinds and amounts of expenditures that will be foregone. The relationships we have discovered in past American experience suggest what the costs of future military efforts may be. These relationships are of course not immutable. Should it be concluded, after enlightened discussion, that certain new defense needs must be met, it is possible by careful choice and control to distribute the burdens somewhat differently. If costs cannot be avoided, perhaps they can be borne in such a way as better to protect the nation's future.

[16] Myron Brenton, "The Higher Cost of Higher Education," *New York Times Magazine,* April 28, 1968, p. 32.

V. THE EXPERIENCE OF SOME OTHER WESTERN NATIONS

We may also examine material on the relation of defense to civilian expenditures in three other major nations: Canada, the United Kingdom, and France. In each the time span is reasonably long and the average level of defense expenditures is high enough and the range wide enough to provide a reasonable test of the impact of periods of military build-up and retrenchment. The data are shown in Figure 2. The post-World War II experiences of Canada and Britain have pretty well paralleled each other, with the Canadian effort somewhat smaller. Both have drastically reduced their relative defense shares since the Korean War. France's pattern is basically similar, but complicated by special efforts for colonial wars in Indo-China and Algeria, and for the *force de frappe*. Data are not available on all the desired dependent variables for each of these countries, and the time span covered varies a little, so we

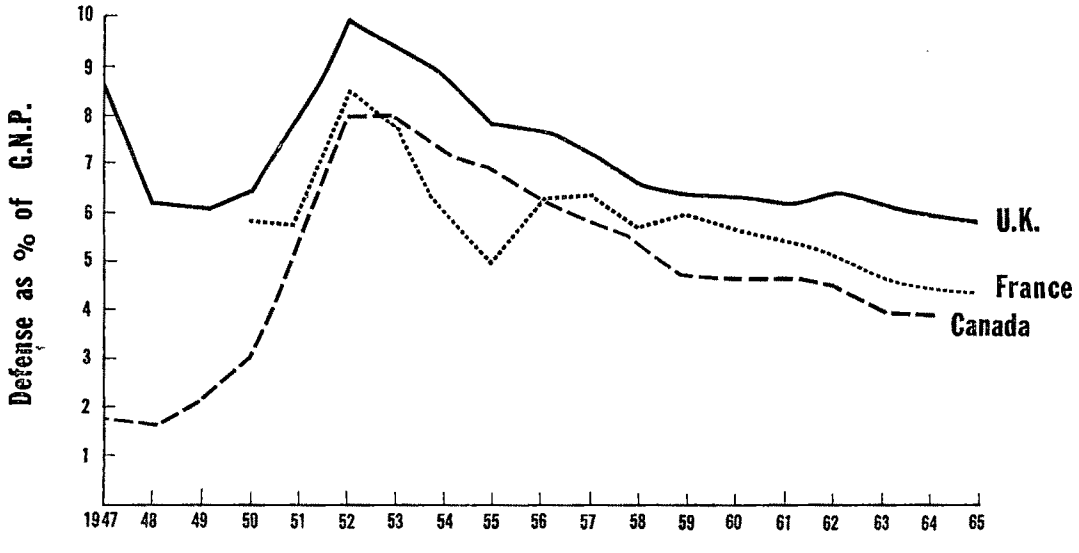


FIG. 2. Defense Expenditures as a Percentage of G.N.P. in Canada, France, and the United Kingdom, 1947–1965.

Source: See Table 3.

must be cautious in pointing out differences among them or, especially, from the United States (which includes World War II data). Nevertheless some tentative conclusions can be suggested. Most important, perhaps, is that there is no single universal pattern for the impact of military spending. Each political system absorbs the economic impact in an different way.[17]

In both Britain and France *investment* suffers substantially from high levels of arms acquisition. The regression coefficient is in every case quite high, with a slope three to four times as steep as we observed in the United States (−.29). In France the absolute amount by which investment changed actually *exceeded* the variation in defense spending. Yet the steep slope is less important because the linkage between defense and investment is much less close; even in France less than half the variance in investment can be attributed to changes in defense (as contrasted with 72 per cent in the U.S.). But investment has not suffered in Canada; on the contrary there is a mild but clearly discernible *positive* relationship. At least at the level of exertion reached by Canadians over the past two decades, military expenditures may actually serve as a net stimulus to fixed capital formation. Only in Canada does there seem to be a very direct conflict between defense and private *consumption*. There the proportion of variance in consumption explained by defense is substantially lower than in the United States (84 per cent) but the slope of the regression line is much steeper (−.814 vs. −.420). It is impossible to tell from this kind of analysis whether the tradeoff is the result of deliberate and explicit policy, but certainly the effect is strong. More than half of the additional dollar expenditures required for defense ultimately come out of personal consumption, or, when the defense proportion declines, go back into consumption.

The French and British experiences are both quite different and more complex. The years of greatest defense effort in France were 1952 and 1953, times when the French economy still had not fully recovered from World War II, and there were some slack resources that could be mobilized by an infusion of public demand. In those years the goods and services needed for the military effort came in large part out of slack, and not in any case much at the expense of consumption. Private consumption essentially maintained its fraction of G.N.P. during the build-up and then fell *after* the first push of the defense effort was largely completed, accounting for the moderate *positive* association we observe between trends in defense spending and in private consumption. This drop in relative defense spending since the early 1950's, despite the French nuclear effort and *force de frappe*, has perhaps enabled investment to rise. Certainly it is capital formation rather than consumption (which has *declined* proportionately) that has in fact increased its share of resources over the period. In Britain, on the other hand, the level of private consumption has been quite unrelated to the defense effort. Consumers were not forced to sacrifice particularly for the rearmament of the early 1950's, nor were they especially indulged when, in the late 1950's and 1960's, military spending was curtailed. Investment has grown somewhat faster than the economy as a whole, but the relationship is not terribly strong ($r^2 = .30$).

For France and Britain it is quite apparent that defense expenditures have been negatively correlated with *government spending* for *civilian* needs. In both cases the former explains roughly half the variance in the latter, and the regression coefficients are remarkably similar. Actually, the figures in Table 3 tend to understate the dependence of government spending on defense in the U.K., because they apply only to the linear model. When a curvilinear fit is attempted, it adds more than 25 per cent of the remaining variance. The curve shows a steep downward slope (inverse relationship between defense and civil government) over most of the range, but flattens out to a low level plateau for the greatest Korean War period rearmament exertions. The slope for the low-to-middle defense levels then becomes much sharper than appeared with just the linear regression model; for that range the coefficient becomes approximately −1.0. In other words, from the mid-1950's onward every decrease in defense expenditures made possible a *fully equivalent* increase in civil

[17] This was also the conclusion of Charles Wolf, Jr., from examining a limited amount of data for some Asian nations. See his *Competition and Complementarity Between Defense and Development—A Preliminary Approach,* P-1743 (Santa Monica California: Rand Corporation, 1959). Similarly, Frederick L. Pryor, in *Public Expenditures in Communist and Capitalist Nations* (Homewood, Ill.: Irwin, 1969) examined the short-run effects of defense changes on various types of expenditures in most Western developed nations for the 1950's and early 1960's. In those nations where defense spending was relatively high, typically there was an inverse relationship between defense and public civil expenditures; in the small states where the defense proportion was low, no relationship appeared. Some relationships of defense to other G.N.P. categories were found in individual countries, but there was no standard pattern.

TABLE 3. THE EFFECT OF DEFENSE SPENDING ON CIVILIAN ACTIVITIES IN SOME MAJOR WESTERN NATIONS

Dependent Variable	r^2 (linear)			Regression Coefficients		
	Canada (1947–64)	U.K. (1947–65)	France (1950–65)	Canada	U.K.	France
Private Consumption	.57	.06	.35	−.814	.494	.825
Fixed Investment	.25	.30	.49	.529	−.842	−1.135
Gen'l Gov't Civil Consumption	*.13*	*.48*	*.63*	*−.354*	*−.469*	*−.440*
Education & Research	.01	.35	.49	−.060	−.247	−.395
Health Services	.01	.05	.24	−.031	−.064	−.041
Special Welfare Services	.24	—	.71	−.180	—	−.027
Transfers for Ed. & Res.	—	.40	—	—	−.065	—
Transfers for Health	—	.00	—	—	.008	—
Social Security & Assistance	—	.34	.39	—	−.240	−.016
Total Education & Research	—	*.37*	—	—	*−.312*	—
Total Health	—	*.03*	—	—	*−.054*	—
Total Welfare	—	—	*.39*	—	—	*−1.596*

Sources: Canada: Dominion Bureau of Statistics, Information Services Division, *The Canada Yearbook, 1945* (Ottawa: S. E. Dawson, 1945) and later annual editions; Dominion Bureau of Statistics, *National Accounts Income and Expenditure, 1926–1950* (Ottawa, 1951); United Nations, *Yearbook of National Accounts Statistics, 1957* (New York: United Nations, 1958), and *Yearbooks* for *1960* and *1966;* O.E.E.C., *Statistics of National Product and Expenditure, 1938, 1947 to 1952* (Paris: O.E.E.C., 1954).

U.K.: Central Statistical Office, *National Income and Expenditure, 1946–1952* (London: H. M. Stationery Office, 1953); Central Statistical Office, *Annual Abstract of Statistics, 1950* (London: H.M. Stationery Office, 1950); U.N., *Yearbook of National Accounts Statistics, 1957, 1966.* No data for 1954 on public expenditure breakdown.

France: O.E.E.C., *Statistics of National Product and Expenditure;* U.N., *Yearbook of National Accounts Statistics, 1957, 1960, 1966;* Ministere des Finances et des Affaires Economiques, Institut National de Statistique et des Etudes Economiques, *Les Comptes de la Nation, 1949–1959* (Paris: Imprimerie Nationale, 1964); Ministere des Finances, *Annuaire Statistique de la France, 1954* (Paris: Imprimerie Nationale, 1954), and *1956, 1957, 1965, 1966;* Ministere des Finances, *Statistiques et Etudes Financieres,* "Situation Provisoire des Recettes et des Despenses de L'Etat Pour 1949," (Paris: 1949), p. 260–62, also studies No. 23 (1950), No. 49 (January 1953), No. 52 (April 1953), No. 69 (September 1954), No. 73 (January 1955), No. 100 (April 1957), No. 142 (October 1960), No. 160 (April 1962), Nos. 163–64 (July-August 1962), No. 197 (May 1965), No. 220 (April 1967).

consumption expenditures in the public sector.[18] The Canadian situation has again been different. The linear regression shows only a very small relation between military spending and public non-military consumption. These figures are deceptive, however, because a curvilinear fit adds almost half the remaining variance. Above the mark of approximately 5 per cent of G.N.P. to defense government civil consumption is sharply reduced.

Finally, we must look at the breakdowns for public expenditure. Some of the categories are subsets of the government consumption aggregates, others come under the rubric known as "transfer payments." According to the standard accounting definition, transfers are *unilateral* payments from government which are considered to provide additions to the income of their recipients, who may be households or private non-profit institutions. They are in effect subsidies rather than payments for goods or services received by government; they include pensions, unemployment compensation, public scholarships, and subventions to educational institutions. Except for social security and public assistance they are typically fairly small as compared with government consumption in Britain, for instance, the health and education transfers amount to only one-fifth to one-fourth the

[18] Note the similar conclusion of Harold and Margaret Sprout, "The Dilemma of Rising Demands and Insufficient Resources," *World Politics,* 20 (July 1968), 660–693.

amount expended under consumption for the same categories.

In Britain and France expenditures and transfers for *health* are not much affected by changes in the armed forces budget. Most of the major components of these aggregates represent long-term social commitments that at least in the European countries are not readily changed. *Welfare* and/or social security assistance responds somewhat more strongly to changes in the defense budget, but, as in the United States, this is not especially surprising. Periods of heavy defense expenditures are likely to be characterized by low unemployment and the return of some retired workers to the active labor force.

The effects of defense needs on education, however, are much more interesting. Expenditures for *education and research* suffer seriously in periods of heavy military demand or, to put it in the direction that military spending has been going in those nations over the last decade and a half, the *diminution* in relative defense costs has made possible substantial *increases* in educational efforts. Since the early 1950's both these nations have pared their defense budgets by an amount that is equal to about four per cent of their G.N.P.'s. This has been accompanied by a shift of about a third of that amount in France into educational enterprises. Since public educational expenditures amount to only 3 or 4 per cent of G.N.P. in any case, the improvement has been very great when measured against the base from which it began. Coupled with the relative increase in investment over these same years that we noted earlier for France and Britain, it is plain that they have turned their defense savings to good long-term use, substantially expanding the productive resources of their economies.

At first glance defense in Canada appears unrelated to *health* and *education* budgets. The linear regression coefficients nevertheless hide a most important development in the postwar Dominion, and a curvilinear fit accounts for about 45 per cent of the variance in each case. Since 1947 Canadian public health and education expenditures combined have risen from 3 per cent of G.N.P. to more than 8 per cent. This expansion was halted, but not reversed, during the Korean War rearmament period. Since then it has proceeded under great steam. In fact, the decline from 8 per cent to below 4 per cent of Canadian resources devoted to defense since 1953 has been matched by a fully equivalent transfer of resources into the health and education sectors. With fixed capital formation we found that Canada did not earn high marks specifically for expanding investment with the funds released by savings in military spending —capital formation in recent years has taken just the same proportion of national income that it did in the early 1950's. But its social investments in a healthier and better educated populace are impressive indeed.

VI. A COMPARATIVE EVALUATION

Normatively, it is difficult to make a clear-cut evaluation of the overall virtues and defects of each nation's experience. None of the countries examined has any monopoly of either. On the requirement that defense upswings be financed largely out of personal consumption, both the United States and Canada come out very well over most of the period; on the requirement that capital formation and public sector social "investment" in education and health be insensitive to military demands, the United States performance is mixed. During the post-World War II and Korean War rearmament periods investment pretty effectively held its own in Canada and, to a lesser extent, in the United States. For example, in the early 1950's the Canadian fixed investment percentage actually rose slightly, and health and education combined fell by only about .3 per cent of G.N.P. In the most recent American military expansion, investment has suffered proportionately more than consumption, though the health and education shares have actually risen.

Another criterion, however, is that in periods of declining military needs the released resources largely be kept for investment and education rather than returned to private consumption. Here one finds that the United States has performed rather badly with its *fixed capital formation* in the past decade. Some revival of investment occurred after World War II and the Korean War, but not during the modest 1962–65 dip in defense expenditures. Nor is there any secular trend toward increased investment. And since 1953 Canadian defense expenditures have been fully halved as a proportion of G.N.P., without any systematic increase in investment to take advantage of the opportunity thus presented.

Britain and France both fit the ideal picture rather better. The French have been very good at substituting capital formation for defense during the latter's downturns, though investment gave a bit during periods of rising defense needs; the British were very good at maintaining investment during periods of expanding military demands, though perhaps they were sometimes a little slow to take advantage of a declining arms budget.

As for *education and research* funds, the United States, Britain, and France all have been fairly sensitive to defense requirements on both the upswings and the downturns of the latter. In

the eyes of many Britons and Frenchmen, and of some American leaders too, educational needs apparently can be sacrificed or postponed when military needs are great, but will be fairly readily indulged when defense pressures are relaxed. All three countries nevertheless can point to a long-term secular increase in the proportion of resources going to education, a trend on which the fluctuations, with upward and downward slopes, are superimposed. The Canadians have most dramatically devoted a rising share of their resources to education in every year but 4 of the last 18; this long-term growth in commitment to education has brought its G.N.P. percentage from 2.1 to 5.2, an upward trend broken seriously by military needs only in 1951. In 1952 education resumed its growth despite a concommitant rise in military expenditures. The experience of other nations suggests some possible alternative policies for American decision-makers.

POLITICAL MATRIX AND POLITICAL REPRESENTATION: PROLEGOMENON TO A NEW DEPARTURE FROM AN OLD PROBLEM*

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Scholars interested in theorizing about political representation in terms relevant to democratic governance in mid-twentieth century America find themselves in a quandary. We are surrounded by functioning representative institutions, or at least by institutions formally described as representative. Individuals who presumably "represent" other citizens govern some 90 thousand different political units—they sit on school and special district boards, on township and city councils, on county directorates, on state and national assemblies, and so forth. But the flourishing activity of representation has not yet been matched by a sustained effort to explain what makes the representational process tick.

Despite the proliferation of representative governments over the past century, *theory* about representation has not moved much beyond the eighteenth-century formulation of Edmund Burke. Certainly most empirical research has been cast in the Burkean vocabulary.[1] But in order to think in novel ways about representative government in the twentieth-century, we may have to admit that present conceptions guiding empirical research are obsolete. This in turn means that the spell of Burke's vocabulary over scientific work on representation must be broken.[2]

To look afresh at representation, it is necessary to be sensitive to the unresolved tension between the two main currents of contemporary thinking about representational relationships. On the one hand, representation is treated as a relationship between any one individual, the represented, and another individual, the representative—an *inter-individual* relationship. On the other hand, representatives are treated as a group, brought together in the assembly, to represent the interest of the community as a whole —an *inter-group* relationship. Most theoretical formulations since Burke are cast in one or the other of these terms.

Current empirical studies of representation by and large make individualistic assumptions. Partly these presuppositions are rooted in the individualistic culture of democratic politics; but they are also eminently congenial to the methodology of survey research that takes the individual as the empirical unit of analysis. In concentrating on the individual, be he representative or represented, contemporary research has gained much insight into the ideology of representation and possibly into representational behavior. We know, for instance, that the representative may see himself as a "trustee" or "delegate" (or some mixture) and that such self-images serve the public official in defining political situations, in guiding his actions, or in justifying his decisions.[3] But research into the rationalizations of the representatives has not

* The larger project of which this analysis is a part, the City Council Research Project, is sponsored by the Institute of Political Studies, Stanford University, and is supported by the National Science Foundation under grants GS 496 and GS 1898.

[1] See, for instance, Heinz Eulau, John C. Wahlke, William Buchanan, and LeRoy C. Ferguson, "The Role of the Representative: Some Empirical Observations on the Theory of Edmund Burke," this REVIEW, 53 (September, 1959), 742–56; or Warren E. Miller, and Donald E. Stokes, "Constituency Influence in Congress," REVIEW, 57 (March, 1963), 45–56.

[2] For a fuller discussion of this point of view, see Heinz Eulau, "Changing Views of Representation," in Ithiel de Sola Pool (ed.), *Contemporary Political Science: Toward Empirical Theory* (New York: McGraw-Hill, 1967), pp. 53–85.

[3] We may note, in making this statement, that of 474 state legislators interviewed in the late fifties, 38 per cent failed to articulate any kind of representational role in response to an open question about how they would describe the job of being a legislator. See John C. Wahlke, Heinz Eulau et al. *The Legislative System* (New York: Wiley, 1962), p. 281. Ten years later, of 435 city councilmen interviewed in connection with this study, as many as 59 per cent failed to make any spontaneous mention of their putative representational role in response to the same question. As far as we know, no student of representational behavior has as yet examined the implications of the evidently low salience of thinking about representation among political practitioners. The matter will be treated in a forthcoming monograph by Katherine Hinckley.

led to an adequate theory that would explain the functioning of contemporary representative government. Other investigations are less "individualistic" in their presuppositions. In particular the theoretical discussions of "public interest" or "general will" suggest an understanding of representation as a relationship between collectivities. These investigations, however, have not provided the empirically grounded theory of representation we feel is needed.

A viable theory of representation, it seems to us, cannot be constructed from individualistic assumptions alone. It must be constructed out of an understanding of representation as a relationship between two collectives—the representative assembly and the represented citizenry. However, neither can a viable theory be advanced in the absence of empirical investigation into the thinking and the acting of the individuals in the collectives. What we grope toward, then, is a theoretically adequate treatment of representation as a property of the political system, a treatment tutored by systematic data.

I. A FRESH LOOK AT REPRESENTATION

Our beginning point is a highly suggestive passage in Professor Pitkin's recently published explication of the concept of representation. She elaborates on representation as something that "must be understood at the public level:"

> The representative system must look after the public interest and be responsive to public opinion, except insofar as non-responsiveness can be justified in terms of the public interest. At both ends, the process is public and institutional. The individual legislator does not act alone, but as a member of a representative body.[4]

By elevating representation from the level of individual relationships to the level of the political system, Pitkin suggests that representation is, in her own words,

> primarily a public, institutional arrangement involving many people and groups, and operating in the complex ways of large-scale social arrangements. What makes it representation is not any single action by any one participant, but the overall structure and functioning of the system, the patterns emerging from the multiple activities of many people.[5]

Having pointed out that representation is a systemic phenomenon, Pitkin goes on to note that representation may or may not emerge from whatever is the relationship between citizens and public officials. "I am not suggesting," she writes, that representation "must emerge from any particular system; there is no guarantee that it will. But it may emerge, and to the extent that it does we consider that system as being a representative government."[6]

If, as Pitkin suggests, representation is a collective and public phenomenon that may or may not emerge in a political community, and if emergence of a representative relationship is conditioned by the "over-all structure and functioning of the system," our attention should be directed to properties of the political system that either facilitate or impede representation. This we propose to do.

Some Methodological Considerations. In describing representation as an emergent property of the political system, Pitkin anticipates a course of methodological inquiry that has engaged us for a number of years and that underlies the procedures adopted in present research on the governance of cities. First, our analysis assumes that representation as well as other variables we consider are group rather than individual properties; thus we make statements about governing bodies and not individual public officials. Second, although we study 82 governments formally defined as "representative," we see representation not as something existing by definition but as something which emerges in the relationship between governing assemblies and governed citizens. Third, whether the particular relationship we might call representation does emerge is affected by the political matrix within which representatives and represented act. Finally, the analysis is configurative, not causal; we want to determine how a particular configuration of system properties, including representation, are linked together.

Of course not all properties of the political community are likely to be equally relevant to the emergence of representation. Our reading of the theoretical literature about American politics directed us to four variables usually considered germane to representation: the degree of social pluralism, the effectiveness of elections as sanctioning mechanisms, the support available to the governing group, and the recruitment processes that select public officials. For present purposes, we consider these four variables—taken together—to constitute a political matrix. The configuration of the political matrix, then, should be critical to how the governing group responds to social pressures and political demands.

[4] Hanna Fenichel Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1967), p. 224.

[5] *Ibid.*, pp. 221–222.

[6] *Ibid.*, p. 224.

Two of these properties—degree of social pluralism and election effectiveness—are first-order system variables that can be directly measured with relevant indicators. The other two properties—recruitment and support—in addition to representation, are constructed into group properties from individual data. The data base for our measures of these three variables are interviews with 423 city councilmen in 82 cities of the San Francisco Bay metropolitan region. In order to make the relevant information appropriate for systemic analysis it was necessary to convert the responses of individuals into group properties. This we did in one of two ways. In the case of representation and support we treated the councilmen as informants. We read appropriate questions in the interview schedule for the council *as a whole* and assigned the council to a code category. Because in this analysis we use only one measure of representation, no further index construction was necessary. Our measure of support is constructed from five separate indicators that are sufficiently cumulative to permit a simple summation index. Recruitment as a systemic property, on the other hand, was measured by aggregating the codings of each individual councilman's recruitment pattern into a single council measure. We describe the procedures more fully as we introduce the measures in the analysis.

To employ these coding procedures, of course, is to take certain liberties. For example, although we sometimes speak of community support for the council, we actually are inferring level of support from responses of the councilmen themselves. This is not the place to engage in a complicated defense of whether "definitions of the situation" do indeed serve as the "reality" for those who see it as such. Our intent is to explore in a tentative fashion the relationship among several variables; level of community support being one of these variables, we sought the best measure possible within the confines of our data. A second methodological assumption made is that single group measures can be constructed from the individual responses of those who belong to the group without doing violence to the data. Again, this is not the time to present an extended discussion of "group properties" and how to construct them. Our general strategy is to pursue certain theoretical questions as vigorously as possible, but to report specific findings in a tentative way, paying full heed to the data difficulties.

II. REPRESENTATION AS RESPONSE

Whether the relationship between the governing few and the governed many can be said to be a representative one depends, of course, on how the term is defined. Representation has been subject to a variety of definitions. Some studies have described a legislative assembly as representative when the social and economic characteristics of the constituency were fairly well mirrored in the assembly. Other studies, pursuing the logic of "mirror representation," have been less concerned with demographic representation and instead have examined whether in ideology or general values the assembly reflected the constituents. Yet other studies, dropping the mirror analogy, have defined representation in terms of equal access to the assembly by all members of the constituency.

The definition of representation which guides the theoretical endeavors in this paper differs from these formulations. In coding the interviews we made what to us was a surprising discovery: of the 82 councils, as many as 36 did not in any discernible manner seem to act in response to any politically organized views in the public. These 36 councils seemed to rely on their own sense of what the community needs were. This finding alerted us to Pitkin's observation that representation is an emergent property whose appearance cannot be taken for granted. The finding further suggested the importance of looking at the representative relationship in terms of whether the elected assembly acts in response to public views, especially views as delivered in some identifiable manner.

Pitkin left no stone unturned in her effort to salvage the concept of representation as a viable tool for theorizing about problems of democratic governance. After reviewing and interpreting almost any conceivable formulation of the concept's meaning, she settles for this definition: "representing here means acting in the interest of the represented, in a manner repsonsive to them."[7] We find this definition useful because it seems to conceive of representation in two related ways: first, the representative assembly defines *what* it should do—"acting in the interest of the represented"; that is, it decides on the political agenda and, in so doing, formulates community goals. Second, the assembly does so in a manner *responsive* to the sentiments of the constituents.[8]

[7] *Ibid.*, p. 209.

[8] Pitkin's treatment of responsiveness appears to stress the condition in which the representative assembly stands ready to be responsive when the constituents do have something to say. An assembly may, therefore, be responsive whether or not there are specific instances of response. Our analysis, as will be clearer shortly, stresses the actual

In studying the protocols from the councils, it became clear that, as they govern, councils can act in response to two types of politically organized and publicly voiced opinions. First, councils can consider the views and wishes of attentive publics, of fairly well-defined and permanent interest clusters in the community. These attentive publics may have differing views of how the community should be governed, in which case the council must compromise and adjust. Or the attentive publics may be more or less of the same view, in which case the council need only determine what this view is and act upon it. In either case, we say that the council, by acting in response to the viewpoints and thinking of attentive publics represents these publics. Second, the council may not concern itself with cohesive attentive publics but may, instead, act in response to ad hoc pressures and petitions. Neighborhood groups, for instance, may organize on a sporadic basis, make claim on the council for some service or benefit, and expect to be listened to by "their representatives." Under these conditions, councils placate or respond to specialized and transitory citizen groups. If in the first case the council represents attentive publics, in this case it represents issue-specific groups of citizens.

There is a third type of representative relationship with the public. As previously noted, some councils appear to be altogether immune to external pressures; no identifiable groups of citizens, permanently or sporadically organized, appear to intrude on council deliberations about community affairs. In such cases, councils may or may not be acting *in the interest* of the represented (an issue we do not explore here); they are not, however, acting *in response to* the represented. Rather, these councils entertain a self-defined image of what community needs are. It is in terms of its own image that the council tackles the problems which come to its attention.

The 82 councils divided into the three categories as follows:[9]

Councils responsive to attentive publics	20	24%
Councils responsive to ad-hoc issue groups	26	32%
Councils entertaining self-defined image	36	44%

The distribution of the councils into this three-category classification essentially sets the question. For present analytic purposes, representation is taken to mean a relationship between governed and governors wherein the governing group responds to ("represents") politically organized viewpoints among citizens, that is, responds to something *other* than its own image of what the community needs. With representation so understood, we ask: under what political conditions is a representative relationship between governors and governed likely to emerge in a community? Our analysis examines the patterns among the four variables that we have singled out as relevant for representation and examines the connections between the political matrix, as constructed from these four indicators, and the emergence of representation.

III. REPRESENTATIONAL RESPONSE STYLE AND SOCIAL PLURALISM

An assumption which frequently appears in discussions of representation and modern society might be stated as follows: increasing complexity and differentiation in modern societies makes it more and more difficult for representative bodies to respond to the variety of interests which constitute the political community. Persons who make such observations usually assume representation to be a relationship between

act of response rather than simply the potential for it. The difficulties of empirically working with a concept stressing the possibility of an act rather than the act itself dictated our decision to modify Pitkin's theoretically suggestive definition.

[9] The coding procedure used was as follows: Both investigators, reading jointly, read through all parts of the interview schedules pertinent to how councilmen defined their relations with the public for all members of any given council. If the councilmen seldom mentioned any groups or groupings in the public or if they failed to describe an actual case where they had been responsive to public pressures or if they simply asserted (a not unusual occurrence) that they knew what was best for the community and acted upon it, the council was placed in the "self-defined image" code. If the councilmen made references to neighborhood groups or to transitory groups wanting, say, a stoplight at a given corner, or to election groups and if the councilmen indicated that they responded to pressures from such groups and attempted to placate them, then the council was coded in the "responsive to issue-groups" category. If the councilmen defined for us a fairly well organized public, attentive to what the council was doing, and if the councilmen indicated (usually by citing an illustrative case) that they were responsive to these attentive publics, the council was placed in the "responsive to attentive publics" code. The procedure, then, used the councilmen as individual informants about the response style of the council. It is quite possible, though not a frequent occurrence, for a given individual councilman to not feel responsive to, say, attentive publics but to describe the council as acting in that way.

individuals. We were interested to see if representation, treated at the systemic level, was indeed less likely to emerge in larger and more complex communities. Table 1 presents relevant data.

Using population size as an indicator of a community's social pluralism,[10] it is clear that responding to interests voiced from the community is *facilitated* in the more pluralistic communities. How are we to reconcile this finding with the assumption that social complexity impedes the exercise of the representational function? It may be correct that the larger and more complex a social system, the more difficult it is for any *one* citizen to make his wishes known or for any *one* representative to respond to individual constituents. The data in Table 1 emphasize why an individualistic conception of representation is obsolete. Representation as a relationship between two collectivities—the represented and the representatives—appears to emerge more easily in the larger, more complex communities than in the smaller, more homogeneous communities. To understand representation as a systemic property we may have to re-think many of our conventional assumptions. Indeed, the responsiveness of the representative assembly is facilitated under just those conditions assumed to impede individual responsiveness.

The pattern in Table 1 makes considerable sense. It may well be that in the smaller and more homogeneous communities, the social structure is such that there is simply little opportunity for the council to respond to stable, attentive publics or to ad hoc, spontaneous issue groups. Unlike in larger and heterogeneous cities, where various social groups are present and likely to make their demands known to the city council, in the smaller, more homogeneous settings, the group life is likely to be less developed and the public pressures brought on the council are probably less frequent or urgent. If this is so, councils in small cities are unable to identify groups to which they might wish to respond, if they only could, and will have to rely on their own images of what is in the best interest of the represented. As Table 1 shows, this is in fact the case: the smaller the city, the more councils act in their self-images; the larger the city, the more councils are responsive to groups that articulate interests.[11]

[10] That size is an adequate indicator of "social pluralism" may not be self-evident. We refer the reader to Jeffrey K. Hadden and Edgar F. Borgatta, *American Cities: Their Social Characteristics* (Chicago: Rand McNally, 1965) for evidence of the correlative power of size as an indicator of a city's demographic and ecological diversity and pluralism.

TABLE 1. CITY SIZE AS INDICATOR OF SOCIAL PLURALISM AND COUNCIL RESPONSE STYLE

Representational Response Style	City Size		
	<10,000 N=32	10–50,000 N=33	>50,000 N=17
Self-defined Image	56%	39%	29%
Ad-hoc Issue Groups	31	36	24
Attentive Publics	13	25	47
	100%	100%	100%

$\frac{\chi^2}{\text{d.f.}} = \frac{8.10}{4} = 2.03$

Gamma = .38

IV. REPRESENTATIONAL RESPONSE STYLE AND ELECTORAL TOLERANCE

It is customary to entertain many assumptions, which often remain untested, about the consequences of elections for representation. Different nominating procedures (primaries vs. conventions), voting rules (list vs. transferable vote), electoral arrangements (at-large, multi-member vs. single-member constituencies), counting procedures (majority vs. proportional) election types (partisan vs. non-partisan), and so on, are said to produce very different types of elected representatives.

Even more to the point, assumptions about elections are very central in writings which consider such issues as "accountability" and "responsiveness." As Dahl has written, elections "are

[11] In Table 1 and all following tables our interpretation of the data is largely based on comparison of the distributions in "high" and "low" categories of the "independent" (column) variable. However, we are attaching to each table two statistics: the raw chi square score adjusted for degree of freedom which can tell us something about the relative order of the data; that is, by dividing chi square by the table's degree of freedom, it is possible to compare tables of different numbers of cells as long as the "N" remains the same or nearly so. Because we are not essentially dealing with a sample but with a universe (82 out of 90 cases in the defined universe), we are not concerned with the sampling problem of whether the distribution in any table is due to chance or not at some set level of confidence. Gamma is introduced as a measure of relationship because it seems especially suitable to data ordered by ordinal or weak ordinal scales.

crucial processes for insuring that political leaders will be somewhat *responsive to the preferences of some ordinary citizens.*"[12] The assumption made by Dahl and by many other writers on democratic politics is that public officials choose policies in anticipation of likely electorate response at the next election. Schlesinger employs similar reasoning and states the point even more strongly: "The desire for election and, more important, for reelection becomes the electorate's restraint upon its public officials."[13] In democratic theory, then, elections are viewed as the sanction available to the public. In the absence of this sanction, office-holders would not be accountable. And, if the governors are not accountable, it is difficult to imagine them being responsive. "The point of holding [the elected official] to account after he acts is to make him act in a certain way—look after his constituents, or do what they want."[14]

The difficulty with these assumptions about representative democracy is that they, in turn, make the assumption that elections do indeed remove or threaten to remove men from public office. We have serious doubts that this is the case. For one thing, and a fact too often overlooked by theorists who emphasize elections as a sanction, a very sizeable group of office-holders retire from office voluntarily. As Charles Hyneman sharply pointed out three decades ago, turnover in legislatures is due not to election defeats, or even to fear of same, but because men simply decide to leave public office. The real task, Hyneman wrote in 1938, "is to find out why so many legislators, senators and representatives alike, choose not to run again."[15] In his analysis of eight states covering the period 1925–33 he found that *more than 60 percent* of the retirements from both lower house and Senates were due to failure to seek reelection.[16]

Alerted by Hyneman to the question of voluntary retirement, we checked to see how frequently city councilmen retired from office for reasons other than election defeat. In 82 cities over a ten year period (5 elections per city), more than half of the councilmen retired voluntarily from office. Although a few leave the council to seek higher office, survey data indicate this number is not large. And though a few might retire out of threat of election defeat, survey data indicate that this occurs very infrequently.[17]

This high and persistent rate of voluntary retirement from elected office certainly should

[12] Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956), p. 131, italics added. At another point Dahl argues, "The effective political elites, then, operate within limits often vague and broad, although occasionally narrow and well defined, set by their expectations as to the reactions of the group of politically active citizens who go to the polls." *Ibid.*, p. 72.

[13] Joseph Schlesinger, *Ambition and Politics* (Chicago: Rand, McNally & Co., 1966), p. 2. Schlesinger's study is a very careful and ingenious examination of how the political opportunity structure in the U.S. might facilitate or impede political ambitions and thus affect the working of democracy. He does not, however, consider the consequences for democratic politics if men in public office are not ambitious.

[14] Pitkin, *op. cit.*, p. 57.

[15] Charles S. Hyneman, "Tenure and Turnover of Legislative Personnel," *Annals of the American Academy of Political and Social Science,* 195 (1938), p. 30. Hyneman also remarks that his finding "completely knocks out the supposition that the transiency of legislative personnel is due to the fickleness of the voter at the polls. . . . Only 16.1 per cent of the 1,965 House members and 14.7 per cent of the 511 senators who quit service during this period were eliminated by defeat in the general election." Pp. 25–27.

[16] Possibly one of the reasons Hyneman's findings have had such little impact on theories about elections is that he was concerned with the implications of turnover for questions of legislative experience. Students who followed Hyneman's lead also addressed themselves to this question. As far as we have discovered, no political scientist has yet considered how the high rates of voluntary retirement might affect the attention of lawmakers to voter preferences. This question is addressed more systematically in Kenneth Prewitt, "Democratic Elitism From Another Angle," paper presented at the Midwest Political Science Association Meetings, Ann Arbor, Michigan, April 24–26, 1969.

[17] The mean percent of voluntary retirements is .53; the standard deviation is .18. The rate of voluntary retirement is not related to any major demographic characteristic of the city, not to size, population density, per cent of the working force in manufacturing occupations, nor to median income. The stability of this rate across all types of cities suggests that it is a very permanent, even institutionalized, feature of nonpartisan city politics in the Bay Area. By the way, only 3 of the 82 cities studied have limitations on tenure. The survey data which help us understand the reasons for the high rates of voluntary retirement are presented and analyzed in Kenneth Prewitt, *The Recruitment of Political Leaders: A Study of Citizen Politicians* (Indianapolis: Bobbs-Merrill, in press).

caution us against the easy assumption that "elections make public officials responsive" and thus guarantee representative government. For if the representative body plans to depart from office in any case, why should it be concerned with voter approval of its policies? To explore the relationship between elections and representative response style, we need to determine whether the voting public does indeed ever remove incumbents from office.

We constructed an index of "forced turnover" based on the number of incumbents who won, divided by the number of incumbents who sought reelection over a ten-year period (five elections). (The index does not include incumbents who were appointed to office in the expired term.)[18] In 21 cities no incumbent seeking reelection was defeated. On the other hand, in only four cities did as many as half of the incumbents suffer defeat. The distribution, therefore, is highly skewed toward success in being reelected. Nevertheless the spread of election defeats in sufficient to permit us to classify cities into three theoretically useful groups: those in which a bid for reelection never failed, those in which it sometimes failed but less often than 25 percent of the time, and those in which it failed for at least one of every four incumbents.

We derive from general democratic theory a simple hypothesis. Cities with the highest rate of forced turnover, where electoral tolerance is relatively low, should have councils tending to act in response to public pressures and petitions. Conversely, councils which govern in a milieu where elections never force anyone from office will act in response only to their own image of what the community needs. This, as Table 2 shows, is the case.

Thus, on the one hand, conventional democratic theory appears to be confirmed. The presence of an electorate which removes men from office leads to more acts of response by the representative body. On the other hand, conventional theory also holds that councils not responding to political groups would be ousted

[18] Appointed incumbents were excluded because of the high rate of appointment to the councils—24 per cent for all cities averaged over the ten year period. Appointment can be a strategy designed, in this context, to assure election. Omitting these appointed incumbents therefore strengthens the index of forced turnover. The aggregate election data which were used in constructing these analyses were initially collected by Gordon Black, now at the University of Rochester, in collaboration with William Hawley, Institute of Governmental Studies, Berkeley. We are indebted to both Black and Hawley for their help.

TABLE 2. FORCED TURNOVER AND REPRESENTATIONAL RESPONSE STYLE

Representational Response Style	Per Cent of Forced Turnover		
	None N=21	1–24 N=39	25+ N=22
To self-defined image	71%	41%	23%
To ad-hoc issue groups	19	31	45
To attentive publics	10	28	32
	100%	100%	100%

$\frac{\chi^2}{\text{d.f.}} = \frac{10.96}{4} = 2.74$

Gamma = .44

from office. If we read Table 2 in reverse it is evident that this does not happen; elections do not necessarily remove councils which respond only to their own image of community needs. The inability of conventional theorizing about elections to help us explain Table 2 is due to an oversight in much of the contemporary literature about representative democracy. Nowhere that we could find have scholars systematically examined the implications for representation *if elections are not used to force turnover* on representative bodies. But the reluctance, or inability, of the electorate to remove public officials is something we must take into account if our theory of representation is not to make unfounded empirical assumptions. Just why non-responsive councils are seldom removed from office is a question to which we turn momentarily, but first we take a quick look at the relationship between social pluralism and electoral tolerance.

We expect that the electorate uses its voting powers to force turnover more often in larger cities, where pressures on the governing bodies are diverse and frequent, than in the more homogeneous and politically quiet environment of the smaller community. Table 3 shows this to be the case. The medium-sized and larger cities are characterized by higher rates of forced turnover than are the smaller cities, though even in the latter election defeats are not uncommon. Interpreting Tables 2 and 3 together, it seems that electoral tolerance is negatively related to the degree of representational response. Where councils need not fear the vote of the people, they are also less likely to act in response to the voice of the people.

V. REPRESENTATIONAL RESPONSE STYLE AND COMMUNITY SUPPORT

For purpose of our theoretical exploration, let us assume that the first three tables provide

TABLE 3. CITY SIZE AND FORCED TURNOVER

Per Cent of Forced Turnover	City Size <10,000 N=32	10–50,000 N=33	>50,000 N=17
None of incumbents	37%	25%	7%
1–24	41	45	64
25 or more	22	30	29
	100%	100%	100%

$\frac{x^2}{d.f.} = \frac{6.26}{4} = 1.57$

Gamma = .28

TABLE 4. FORCED TURNOVER AND COMMUNITY SUPPORT

Level of Community Support	Per Cent of Forced Turnover None N=21	1–24 N=39	25+ N=22
Relatively high	81%	49%	45%
Relatively low	19	51	55
	100%	100%	100%

$\frac{x^2}{d.f.} = \frac{7.14}{2} = 3.57$

Gamma = −.42

findings on which we can build. Governing bodies with self-defined images of the public interest are found in smaller communities and are less subject to electoral sanctions than councils which act in response to public pressures. This is a paradox for representational theory. In order to solve the paradox, it is necessary to consider another aspect of the political matrix that may be related to a council's representational response style—a system property we characterize as "community support." We expect, for instance, that where a council is generally supported in what it does by the citizenry, it is relatively free to define the community interest in its own image. And if this is so, it helps explain why a council, though non-responsive, faces little risk in being ousted from office.

In order to characterize the citizenry of the 82 cities as more or less supportive of the council, we assumed that support was forthcoming if, according to council reports, any one or all of the following conditions were met: (1) that the public held a favorable and respectful image of the council; (2) that the public was in general agreement with the council on its duties; (3) that the public did not include disruptive elements; (4) that there were not many groups steadily critical of the council's policies. We combined these items into a single index of community support.[19]

The measure of community support allows us to investigate the paradox that councils acting on the basis of their own images are less exposed to electoral sanctioning than councils which placate ad-hoc issue groups or are attentive to more stable interest groups. As Table 4 shows, communities in which the electorate does not force incumbents from office are seen as overwhelmingly supportive of their councils; in communities where some or relatively many incumbents are forced from office, the citizens are seen as almost evenly split in the support they are giving their councils.

In communities, then, in which the citizenry is on the whole satisfied with council operations and policies and is apparently giving the council its support, we can assume that citizens do feel "represented" by the council even though the council follows its own definition of what is in the best interests of the governed. If this inference is sound, it helps explain why councils which do not act in response to community groups are yet free of threats of election defeats. It is precisely because it does not hear from the public that the council is able to rely on its own judgment. As Table 5 indicates, there is a very significant difference in councils' representational response styles between the more and less supportive political milieux, and the relationship is exceptionally strong. The table and our analysis suggest that, in dealing with representation as a

[19] Four of the five items used in the support index were coded by using the informant procedure described in footnote 9. Councils were classified according to whether they reported 1) the public to have a respectful view toward councilmen, 2) the public to be in agreement with the council's definition of its duties, 3) the public to include disruptive and unfriendly elements, and 4) the public to be generally supportive in its behavior toward the council. The fifth item, whether there are critical groups in the community, was initially an aggregate measure of individual responses to a question about the number of critical groups. Councils were ranked in terms of this aggregate measure and those above the median were said to have supportive publics, those below, to be operating in a non-supportive environment. Each council was given a score of 1 for each plus on the five items. The support scores were then dichotomized to provide the "relatively high" and "relatively low" classifications used in the analysis.

TABLE 5. COMMUNITY SUPPORT AND REPRESENTATIONAL RESPONSE STYLE

Representational Response Style	Community Support	
	Relatively High N=46	Relatively Low N=36
To self-defined image	63%	19%
To ad-hoc issue groups	17	50
To attentive publics	20	31
	100%	100%

$\frac{\chi^2}{\text{d.f.}} = \frac{16.52}{2} = 8.26$

Gamma = .54

system property, we must at all times keep in mind that it is embedded, as Pitkin so well put it, "in the complex ways of large social arrangements." It remains, therefore, to ascertain in what type of community support is forthcoming. From all that we know about small, relatively homogeneous cities we expect that they are more likely to have a supportive citizenry than will larger, more heterogeneous cities. Table 6 shows that our expectations are met: the relationship between support and social pluralism is as strong as between support and response style. Small communities, it appears, generate relatively high levels of political support that, in turn, leave the governing body free to pursue community interests as it sees fit.

VI. RECRUITMENT AND REPRESENTATION

The study of political representation must, at some time, confront the naked fact that in any political community a handful of men are chosen to govern over a very large number of citizens. As Lord Bryce observed:

> In all assemblies and groups and organized bodies of men, from a nation down to a committee of a club, direction and decision rest in the hands of a small percentage, less and less in proportion to larger size of the body, till in a great population it becomes an infinitesimally small proportion of the whole number. This is and always has been true of all forms of government, though in different degrees.[20]

TABLE 6. CITY SIZE AND COMMUNITY SUPPORT

Level of Community Support	City Size		
	<10,000 N=32	10–50,000 N=33	>50,000 N=17
Relatively high	72%	58%	23%
Relatively low	28	42	77
	100%	100%	100%

$\frac{\chi^2}{\text{d.f.}} = \frac{10.59}{2} = 5.30$

Gamma = −.53

The phenomenon that so impressed Bryce alerts us to the fascinating research problem of linking political recruitment and political representation. Since it is clear that a few men are chosen to govern the many and since, at least under democratic rules, the few are charged to "represent" the many, it is important for a theory of representation that we investigate how the few are chosen. This directs our attention to political recruitment—the process or set of processes by which in a city of, say, 30,000 inhabitants, the population is narrowed to only five men who, as councillors, assume formal authority to govern the remaining 29,995 citizens.

In spite of the obvious logical connection between how the governors are recruited from the people and how they represent the people, the linkage has received little attention in empirical political studies. Some years ago, one of us had occasion to point out that "the relationship between the process of selection of legislators and the modes and consequences of legislative behavior . . . offer wide and fertile fields for empirical research,"[21] but the relevant questions were not pursued in *The Legislative System* or, as far as we can tell, in any other subsequent work.

One of the main reasons for this inattention to the link between recruitment and representation is the tendency of scholars to treat recruitment as an individual characteristic. There are studies of the political career, of the selective effect of personality on political success, of nominations of candidates, of the ascent and descent of political leaders, and so on.[22] Although these studies are productive in their own right, the preoccupation with recruitment at the individual level blocks theorizing and empirical research which could connect recruitment and representation. If the position taken in this paper that representation is a systemic property be accepted, however, it is evident that recruitment must also be conceptualized at that level.

[20] James Bryce, *Modern Democracies* (New York: The Macmillan Co., 1924), p. 542.

[21] Wahlke, Eulau, *et al, op cit.*, p. 269.

[22] The literature, of course, is quite large. Representative studies are reviewed in Prewitt, "Political Socialization and Leadership Selection," *Annals of the American Academy of Political and Social Science*, 361 (September, 1965), pp. 96–111.

We should note that there are studies which treat recruitment and representation at least as aggregate variables. These are the studies which examine the socio-economic attributes of elected officials and compare them with a demographic profile of their constituents.[23] The difficulty with this research design is that it locks the analyst into a very narrow definition of both recruitment and representation. As to recruitment, it means studying who is selected but not the processes by which this happens. For representation it means that the analysis is limited to a "mirror theory" wherein the very complex process of representation is reduced to a very simple formula of statistical "representativeness." This approach can yield only limited understanding.

Our own conception of recruitment centers in the problem of "sponsorship"—the degree to which the recruitment process is open or unsponsored and the degree to which it is closed or sponsored. Where sponsorship is highly developed, persons already in established political positions exercise considerable control over who will sit on the council. Sponsorship implies that there are fairly determined or even institutionalized pathways to office. The route to the council might be through an apprenticeship on the planning commission or by being an officer of the Chamber of Commerce or by being active in a local ethnic association. For a man to gain a council seat in a community where sponsorship dominates political recruitment, it is important and maybe even necessary for him to first join the inner circles. Sponsorship does not mean, however, that restrictive criteria are being applied, at least as we normally think of restriction in political recruitment. Our analysis is not concerned with whether persons with the "wrong" social traits are eliminated from consideration. In addition, sponsorship is not a notion which masks some conspiracy theory. A community can rely heavily on sponsorship as a means for recruiting political talent without there being manipulation by powerful persons behind the scene.

Sponsorship is an issue of considerable theoretical interest to students of political recruitment. A problem for most elected governing groups is how to maintain some policy continuity despite personnel turnover. This is an especially difficult problem for city councils where, as we have seen, the rate of voluntary retirement is high and where the average tenure is fairly low. (The average number of years of service is 6.5). Continuity of policy viewpoint can be maintained despite turnover if control can be exercised over successive recruits. A procession of like-minded men through office is equally as effective in stabilizing city policies as is low turnover. If indeed sponsorship aids a governing group to maintain control over both its members and its policies, sponsorship should also relate to how the group defines its representational function. A reasonable hypothesis is that sponsored recruitment insulates the council from certain political experiences and that this insulation will in turn lead to a representational response style that minimizes the impact of organized demands from the public on council thinking.

The measure of sponsorship used here was derived by aggregating individual recruitment patterns at the council level. Just as any one councilman could, on the one extreme, be an "outside challenger" who initiated his own career and attained a council seat with minimum prior contact between himself and those already in office, so the council group as a whole could have followed this career line. At the other extreme, just as an individual might have been asked to run for office by current incumbents or even appointed to office, so the council as a group could have had this experience. Between these extremes more or less prior involvement with city affairs could be characteristic of individual councilmen or the council as a whole. Sponsorship is admittedly a highly multidimensional measure as it pertains to the council, but it undoubtedly captures at the group level something of the rich variety of recruitment patterns that are possible.[24]

[23] See, for instance, the collection of articles in Dwaine Marvack (ed.), *Political Decision-Makers* (Glencoe: The Free Press, 1961); Donald R. Matthews, *The Social Background of Political Decision-Makers* (Garden City: Doubleday & Co., 1954); and the chapter by Thomas R. Dye in Herbert Jacob and Kenneth N. Vines (eds.), *Politics in the American States* (Boston: Little, Brown & Co., 1965). This issue is explored with the city council data in chapter 2 of the book by Prewitt cited in footnote 17.

[24] The measure of sponsorship is particularly problematic since we are summing not just individual experiences to get a group score but individual experiences which took place over a considerable span of time in some cases. It may be that the aggregation of individual career experiences into a council recruitment measure disguises more variance in the original data than the index should be burdened with. Councilmen enter the council at very different points in time and recruitment, as a system property, may have undergone major changes since the entry experiences of the older members. For the present, however, we are trapped by our own data; when we began the study we still were thinking of recruitment as an

TABLE 7. FORCED TURNOVER AND POLITICAL RECRUITMENT

Amount of Sponsorship	Per Cent of Forced Turnover		
	None N=21	1–24 N=39	25+ N=22
Little	5%	36%	27%
Some	38	51	59
Much	57	13	14
	100%	100%	100%

$\frac{\chi^2}{\text{d.f.}} = \frac{18.59}{4} = 4.65$

Gamma = −.44

TABLE 8. COMMUNITY SUPPORT AND POLITICAL RECRUITMENT

Amount of Sponsorship	Community Support	
	Relatively High N=46	Relatively Low N=36
Little	19%	33%
Some	48	53
Much	33	14
	100%	100%

$\frac{\chi^2}{\text{d.f.}} = \frac{4.50}{2} = 2.25$

Gamma = .38

Recruitment patterns, of course, are part of the political matrix and are likely to be related to other system properties. In communities where elections are not a sanctioning mechanism and where there is, as a result, little or no forced turnover, the recruitment pattern is likely to be characterized by considerable sponsorship, while in politically volatile systems relatively little sponsorship is likely to be practiced. As Table 7 reveals, these expectations are reasonably well substantiated by the data.

Similarly, we expect that sponsorship is more likely to appear in a supportive political environment than in a critical one. In a community seen as supportive, where the council is relatively free to do as it pleases precisely because what the council does pleases the citizenry, incumbents are likely to bring into the council men who have already had experience in local community affairs and who can be counted on to continue the policies which seem so satisfactory. On the other hand, and the relatively weaker relationships shown in Table 8 underline the point, a council more exposed to criticism and social pressures may *also* seek to perpetuate its policy views by bringing like-minded and trusted members into the council fold. On balance, then, either alternative is possible. Support as a system property and recruitment through sponsorship are moderately related.

Recruitment practices might also be related to the degree of social heterogeneity or homogeneity of a community. In small communities where men are more likely to share each other's characteristics, know each other better and are more likely to be of similar mind, sponsorship should be practiced more frequently than in more pluralistic settings. Again the data suggest, as Table 9 shows, that this seems to be the case, though a good deal of sponsorship is evidently also practiced in the larger and medium-sized cities.

We noted in connection with Table 1 that the response style of a council depends in some important measure on the actual presence of groups or publics to which the representative group can respond. We assumed that the same reasoning would hold for recruitment practices; a socially diverse community would sustain a relatively open recruitment process. Although the reasoning is not incorrect, Table 9 indicates that recruitment practices may be less affected than response style by community characteristics. This being the case, the relationship between recruitment and representation is perhaps more problematical than our initial theoretical reasoning might suggest.

Where a council is free to recruit its own suc-

individual attribute and thus mainly collected data about individual careers. Despite the relatively weaker nature of our sponsorship measure, we are reluctant to give up our theoretical posture. We simply note, then, that the weaker relationships in tables using the sponsorship measure may be traced to these methodological difficulties. A council was given a sponsorship measure by computing the mean of six alternate paths to office. The "sponsorship continuum" ranged from the case in which an outside challenger initiates his own career and attains a council seat with minimum contact between himself and those already in established positions, to the case in which a councilman was deliberately selected—either asked to run or appointed to the council—by those already in office. The means were then ranked and, for present purposes, the lowest quartile in the rank constitutes the low sponsorship councils; the highest quartile constitutes the high sponsorship councils; the remainder we assigned to the middle group.

TABLE 9. CITY SIZE AND POLITICAL RECRUITMENT

Amount of Sponsorship	City Size <10,000 N=32	10–50,000 N=33	>50,000 N=17
Little	19%	30%	29%
Some	37	61	53
Much	44	9	18
	100%	100%	100%

$\frac{\chi^2}{d.f.} = \frac{11.14}{4} = 2.79$

Gamma = −.34

TABLE 10. POLITICAL RECRUITMENT AND REPRESENTATIONAL RESPONSE STYLE

Representational Response Style	Amount of Sponsorship Little N=21	Some N=41	Much N=20
To self-defined image	38%	37%	65%
To ad-hoc issue groups	33	36	20
To attentive publics	29	27	15
	100%	100%	100%

$\frac{\chi^2}{d.f.} = \frac{4.85}{4} = 1.21$

Gamma = −.25

cessors, with a view toward maintaining continuity in policy leadership by like-minded and trusted men, it should also be free from those public pressures which would force it to consider views other than its own when selecting policies. But, it seems, sponsorship is practiced to some degree in the larger, more pluralistic cities where councils are exposed to a politically more active environment. It is not surprising, therefore, that, as Table 10 shows, the relationship between recruitment and representation is relatively weak. Although the data point in the theoretically expected direction—councils practicing "much" sponsorship seem more likely to follow their own image of the community interest, while councils characterized by "little" sponsorship are more likely to respond in one way or another—the relationship between the two variables is lower than that between any other unveiled in the total configuration of the analysis.

Because of the small number of cases, we are generally reluctant to subject the data to a multivariate analysis which would permit us to untangle the relationship between recruitment and representation. However, since councils in the larger cities reported more sponsorship than expected, we decided to control the relationship between recruitment and representation by city size, collapsing however both the size and the representation variables. Table 11 reports the results.

It appears that where *much* sponsorship is practiced in a city's recruitment process, size does not have an independent effect on representational style. In large and small cities, councils practicing a great deal of sponsorship tend to follow their own image of community interests. But where sponsorship is less frequently practiced, councils in large cities are more responsive to issue groups or attentive publics than councils in the smaller cities. In the latter, one half of the ten councils with open recruitment practices yet are able to pursue their own conceptions of the community interest. This may be due, of course, to the fact, suggested in regard to Table 1, that in smaller cities there may just

TABLE 11. POLITICAL RECRUITMENT AND REPRESENTATIONAL RESPONSE STYLE, CONTROLLED BY CITY SIZE

Sponsorship	City Size <25,000			>25,000		
Representational Response Style	Little N=10	Some N=27	Much N=15	Little N=11	Some N=14	Much N=5
To self-defined image	50%	41%	67%	27%	29%	60%
To issue groups & publics	50	59	33	73	71	40
	100%	100%	100%	100%	100%	100%
	Gamma = −.25			Gamma = −.32		

TABLE 12. POLITICAL RECRUITMENT AND REPRESENTATION RESPONSE STYLE, CONTROLLED BY COMMUNITY SUPPORT

Sponsorship	Community Support					
	Relatively High			Relatively Low		
Representational Response Style	Little N=9	Some N=22	Much N=15	Little N=12	Some N=19	Much N=5
To self-defined image	78%	50%	73%	8%	21%	40%
To issue groups & publics	22	50	27	92	79	60
	100%	100%	100%	100%	100%	100%
	Gamma = .00			Gamma = −.52		

not be any groups or publics sufficiently active to be paid attention to by the council. In general, we can conclude that the uncontrolled results of Table 10 do not give too distorted a picture of the relationship between recruitment and representation.

Because community support seems strongly related to the kind of representation that emerges, whereas recruitment seems less critical, we controlled the recruitment-representation relationship by our support variable. Table 12 shows some interesting outcomes. Where the community is supportive, sponsorship has no effect at all on representational style. Regardless of whether much or little sponsorship occurs, councils in supportive communities are pursuing their own notions of the public interest. There is, of course, one very plausible interpretation of why sponsorship is so unrelated to response style in such communities. In small cities, recruitment processes need not be controlled to insure a succession of like-minded men through office. In the socially homogeneous and politically satisfied community, anyone who presents himself for office is acceptable. Selection of public leadership by some random method is as likely to produce men with roughly the same views as does selection by sponsorship. Thus the high support but low sponsorship communities do not present an anomaly at all.

However, in communities where support from the citizenry is relatively low, sponsorship has the expected consequences for representational behavior. Councils with little sponsorship are almost always those which respond to public pressures, while those with more sponsorship apparently are those less likely to respond (but the small number of cases does not permit us to be confident that this result is sufficiently stable to allow a firm inference). While recruitment does seem to play a role in representation, the degree of support that a community is willing to give is clearly a more important factor in shaping representational style.

Support, in turn, is attenuated by the political system's electoral tolerance. As Table 13 indicates, in a relatively supportive environment where retirement is forced upon incumbents by

TABLE 13. FORCED TURNOVER AND REPRESENTATIONAL RESPONSE, CONTROLLED BY COMMUNITY SUPPORT

Representational Response Style	Community Support					
	Relatively High			Relatively Low		
% Forced Turnover	None	1–24	25+	None	1–24	25+
To self-defined image	82%	58%	40%	25%	25%	8%
To issue groups & publics	18	42	60	75	75	92
	100%	100%	100%	100%	100%	100%
	Gamma = .55			Gamma = .42		

electoral defeat, councils tend to be attentive rather than self-sufficient in their representational behavior. In less supportive communities forced turnover accentuates the tendency for councils to act in response to community groups. Eleven of twelve councils with high forced turnover rates indicated responsiveness to issue groups or attentive publics, 15 per cent more than reported in the original Table 2. The fact that our measure of forced turnover is truly independent of our representational style and support measures makes this result all the more significant.

VII. SUMMARY AND CONCLUSIONS

The analysis suggests that the four components of the political matrix—the complexity of the social environment, the impact of elections in forcing incumbents from office, the degree of public support perceived by the council, and the amount of sponsorship in political recruitment —may explain a great deal about whether a responsive relationship between governors and governed will emerge. Since the analysis has been configurative, on the assumption that representational "responding to" emerges, if it does, in the context of the "over-all structure and functioning of the system," we do not assess the relative impact of any given variable. Rather we stress two things: first, there seems to be a theoretically meaningful cluster of political phenomena that is strongly related to the response style adopted by the governing body; and second, under certain, very identifiable political conditions the governing body may remain indifferent to any views of the public good except its own and yet not suffer at the hands of an antagonistic electorate. What then do we make of these findings for a theory of representation?

In order to pursue a theory of representation we have chosen a particular concept of representation. The concept with which we work stresses that political representation is a relationship between two collectivities—i.e., representation occurs when the few who are chosen to govern respond to some organized demands or preferences of the many who permit themselves to be governed. The theory to which this concept directs us is one which emphasizes that a politically responsive relationship may or may not emerge, and that the type of community in which it does emerge is characterized by an identifiable matrix of properties.

The analysis led to some unexpected findings; we summarize them here, but remind the reader that the "prolegomenon" in the title is a word to be taken seriously. What we have attempted is to break new ground in theorizing about representation in modern society; the specific findings to which our theorizing activity give rise will undoubtedly be modified as additional empirical work along these lines takes place. The findings, then, are presented as suggestive of what we may expect from a re-thinking of "representation" rather than as confirmed facts about the political world.

1. By the definition we introduced, an elected assembly is representative when it acts in response to publicly expressed and more or less organized viewpoints from the citizenry. We found that the governing councils often act in response to interest groups or attentive publics in the larger, more diverse communities. In the smaller, homogeneous cities, councils tend to rely on their own image of what the community requires. Thus, as an initial observation, we suggest that as a system property representation is more likely to emerge under just those conditions often presumed to impede responsiveness at the individual level.

2. A theory of representation as an emergent property may have to be adjusted in the light of certain facts about elections too long ignored. We are not sure, as one author has recently stated it, that "elections are thought of as providing the great sanction for assuring representative behavior."[25] The patterns uncovered in our analysis suggest a considerably more complex relationship between elections and representative government. Where an aroused electorate does, from time to time, unseat incumbents, the governing groups do tend to be attentive. However, it does not follow that elected assemblies concerned only with their own definition of the political situation will be turned out of office, for such self-images may indeed coincide with public preferences.

3. A council tends not to be responsive, as we use the term, where the public is viewed as being most supportive. This finding suggests a major qualification to the assumption that "elections force representativeness." Representative government can function quite independently of elections, though only under certain conditions. A politically satisfied community is apparently unconcerned about whether the council acts in response. If the job is being done, the citizens would just as soon not be bothered. It appears then not to be the inattentive council which is thrown out of office but only the council whose performance is suspect.

[25] J. Roland Pennock, "Political Representation: An Overview," in Pennock and John W. Chapman (eds.), *Representation: Nomos X* (New York: Atherton Press, 1968), p. 8.

4. A fourth finding is one which validates the observations just made; communities in which the council sees itself as generally supported tend to leave to the council itself the selection of its successors. The pattern of sponsored recruitment, in which candidates are insulated from the competitive struggle for a council seat, tends to be associated with a representational style also suggesting a certain insulation from public pressures.

Students of democratic politics are deeply interested in the processes available to the public for controlling and holding accountable the political leadership; the analysis presented here is closely related to an understanding of these processes. It appears that members of the public dissatisfied with their representative assembly can intrude into its deliberations and force attentiveness in two ways at least, (1) by playing a role in determining who is selected to the representative body, and (2) by defeating incumbents when they stand for reelection. Put differently, when members of the public control the constituting of the representative assembly, they also influence how that assembly will define its representational role. When, however, citizens do not exercise that control, allowing the assembly more or less to determine its own members and seldom unseating an incumbent, they thereby permit the representative group the privilege of defining for itself the goals and programs of the community.

WILBUR D. MILLS: A STUDY IN CONGRESSIONAL INFLUENCE*

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All leaders are also led; in innumerable cases, the master is the slave of his slaves. Said one of the greatest German party leaders referring to his followers: "I am their leader, therefore I must follow them."

Georg Simmel[1]

Political scientists studying Congress have shown the same disinclination for the study of individual leaders as that of the profession as a whole. Whatever the reasons for avoiding an analysis of social and political processes from the perspective of an individual—and there are several good ones[2]—it is difficult to ignore, if not discount, the extreme emphasis placed on personalities by experienced participants and observers of the congressional process. One may decide, with Fenno, to underplay references to specific individuals in an effort "to show how much generalization is possible short of a heavy reliance on personality data."[3] But the fact remains that those closest to the legislative process do see it in terms of individuals and personalities; more important, much can be learned, as evidenced by Huitt's work, by focusing on individual legislators and the contexts within which they function.[4] Whether it is Lyndon Johnson as Senate Majority Leader searching for the man who is the "key" to a particular bill, the differences between a Rayburn and a McCormack, or the skill of a Judge Smith, the individual looms large on Capitol Hill.

Among the reasons why political scientists have shied away from individual or personality-centered studies of Congress are, first, the extreme variability of personalities, which stands as a barrier to generalization; and, second, the fact that it is hard to say how much of the legislative process is due to individuals and how much is due to the situational factors that confront them. Faced by these old problems, there is an understandable tendency to downplay individuals and personalities, and, implicitly or explicitly, to treat the situation and the group as the primary determinants of behavior. Before taking this step, however, it might be wise to consider a warning made by Homans apropos of Durkheim's social mold theory (which overemphasized the impact of society on individuals): "Intellectually the descent into hell is easy. One false step, and logic will do the rest."[5]

Durkheim erred, according to Homans, because he thought of the individual-society dichotomy in terms of cause and effect and not in terms of mutual dependence.[6] Norms, for example, are the result of human interaction but they may also be the cause of such interaction if, once established, they govern future behavior. The same kind of mutual dependence exists between individual leaders and the group. "Neither individual character structure nor the contextual configuration alone," Edinger writes, "can explain a leader's behavior, but careful analysis of their interaction—in as many instances as possible, may reveal certain patterns and facilitate understanding."[7]

Thus, for certain analytical purposes, one may take either a group-sociological or an individual-personality approach to the study of

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[1] "On Superordination and Subordination," *Theories of Society,* eds. Talcott Parsons, Edward Shils, Kasper D. Naegele, Jesse R. Pitts (New York: Free Press, 1965), p. 542.

[2] Edinger discusses many of them. See Lewis J. Edinger, "Political Science and Political Biography: Reflections on the Study of Leadership (I)," *Journal of Politics,* 26 (May, 1964), 423–39.

[3] Richard F. Fenno, Jr., *The Power of the Purse* (Boston: Little, Brown & Co., 1966), p. xxiii.

[4] Ralph K. Huitt, "The Morse Committee Assignment Controversy: A Study in Senate Norms," this REVIEW, 51 (June, 1957), 313–329; "Democratic Party Leadership in the Senate," this REVIEW, 55 (June, 1961), 333–344; "The Outsider in the Senate: An Alternative Role," this REVIEW, 55 (September, 1961), 566–574.

[5] George C. Homans, *The Human Group* (New York: Harcourt, Brace and World, 1950), p. 318.

[6] *Ibid.,* p. 319.

[7] *Op. cit.,* p. 437.

Congress, but the two approaches may also be treated as complementary. Indeed, in the case of the Ways and Means Committee, Chairman Wilbur D. Mills stands out as one of the most influential committee chairmen in recent years, if not in history, and, like all leaders, he also follows. The paradox of leadership is that it always involves followmanship; and, as a leading House liberal once said of Mills, "He's an expert in followmanship."[8] The group that forms the base of his influence in the House and that he is most likely to follow is the Committee.

Mills's leadership is here conceived not solely in terms of the traits he possesses which enable him to lead others, a view of leadership which has been rejected for many years,[9] but as a function of his relationship to his colleagues on the Committee. "There can be no leadership," as Gibb says, "in isolation, it is distinctly a quality of a group situation."[10] We will be concerned with Mills as he interacts with the Committee, a concern which tends to stress the group-sociological side of leadership. A balance is sought between Mills as an individual leader and the nature of the group he leads, but the approach to the former is primarily through the ways in which Mills is seen by the rest of the Committee: their perceptions of his leadership, what they like and dislike about the way he runs the Committee, the scope and limitations of his leadership as they see it, why they respond to him in certain ways, and how it is that he is allowed to exert influence on them. The justifications for this emphasis are the conception of leadership discussed above and, secondly, the fact that much better data have been collected on Mills vis-a-vis the Committee members than on Mills the inscrutable personality who intrigues, and frustrates, those in Washington whose view is restricted to him as a particular kind of individual.[11]

Part I describes and analyzes Mills's instrumental or task leadership of the Committee and ties it to his socio-emotional or affective leadership. The main finding is that, with qualifications, he is both the task leader and the socio-emotional leader of the Committee, and that the two kinds of leadership are inextricably linked in the Committee. Part II discusses Mills's influence in the Committee—as distinct from power—and explores the bases and scope of his influence with the members. The theory of exchange is employed as a useful way to conceive of and explain such problems. In Part III the Chairman is discussed in terms of the voting coalitions in the Committee and his central position as a swing vote is analyzed as a basic factor in explaining his influence.

I. MILLS AS INSTRUMENTAL AND AFFECTIVE LEADER

Studies of small groups, as is well-known, commonly report a tendency for two types of leaders to function in groups: the instrumental or task-oriented leader, and the socio-emotional or affective leader.[12] Instrumental leaders are primarily concerned with realizing the goals of the group and directing the group toward the completion of its tasks. Task-oriented direction may give rise, however, to intra-group conflict and disintegrative pressures, creating a need for some way to relieve tension. The affective leader helps to cool internal tensions and to stimulate a harmonious working atmosphere. Theoretically, the activities of the two leaders complement each other and the stable group achieves a state of equilibrium internally and in relation to its environment. Experiments with small groups

[8] Quoted in Julius Duscha, "The Most Important Man on Capitol Hill Today," *New York Times Magazine*, February 25, 1968, p. 78.

[9] Seligman wrote one of the obituaries in 1950. See Lester G. Seligman, "The Study of Political Leadership," this REVIEW, 44 (December, 1950), 912–914.

[10] Cecil A. Gibb, "The Principles and Traits of Leadership," *Small Groups: Studies in Social Interaction*, eds. A. Paul Hare, Edgar F. Borgatta, and Robert F. Bales (New York: Alfred A. Knopf, 1955), p. 91. Likert comments: *"To be effective and to communicate as intended, a leader must always adapt his behavior to take into account the expectations, values and interpersonal skills of those with whom he is interacting."* Rensis Likert, *New Patterns of Management* (New York: McGraw-Hill, 1961), p. 95 (italics his).

[11] A reporter who is very close to Mills and has studied him for ten years once observed to me, "I know Wilbur Mills better than anyone in this town, and I don't know him at all." For a more complete account of the nature of Ways and Means, and some fragments on Mills which are used in the present study, see John F. Manley, "The House Committee on Ways and Means: Conflict Management in a Congressional Committee," this REVIEW, 59 (December, 1965), 927–939.

[12] Robert F. Bales and Philip E. Slater, "Role Differentiation in Small Decision-Making Groups," *Family, Socialization and Interaction Process*, Talcott Parsons, et al., (London: Routledge & Kegan Paul, 1956), pp. 279–306. See also Philip E. Slater, "Role Differentiation in Small Groups," in Hare, Borgatta and Bales (eds.), *op. cit.*, pp. 498–515.

TABLE 1. TASK AND SOCIO-EMOTIONAL LEADERSHIP IN THE WAYS AND MEANS COMMITTEE

	Task Leadership			
	Mills	Mills and Byrnes	No One Stands Out	Other and No Response
Democrats	9	2	2	0
Republicans	1	4	2	0
	Socio-Emotional Leadership			
Democrats	6	2	1	3
Republicans	4	1	1	1

have found that one man may perform both task and maintenance functions, but by and large the opposing requirements of these functions lead to a division of labor within the group.

The author's initial research on the Ways and Means Committee led to no evidence that the task and affective leadership functions were separated in the Committee; in fact, Mills appeared responsible for guiding the Committee's task efforts and his leadership seemed to be the main reason that the affective tone of the Committee was high. In order to test this hunch, the members of the Committee were asked two questions:

1. When you come to particularly difficult problems, does any member of the Committee stand out as being the one who most often comes up with a way out?
2. During the course of discussing legislation things can get rather tense from time to time. I know that this happens even in Ways and Means at times. Does anyone stand out as the peacemaker in these situations?

Table 1 categorizes the responses to these questions by party. The results on the task leadership question show a greater tendency among Democrats to assign this function to Mills than the Republicans. For the Republicans, Mills and the ranking Republican, John Byrnes (Wis.), together and in that order, are the instrumental leaders in the sense posed by the question. The results on the affective leadership question are more mixed but again Mills is most often seen as the man who stands out in this regard, for Republicans and Democrats alike. Although the data are more suggestive than conclusive, it is interesting to note that the Republicans were more inclined to cite Mills as the affective leader by himself, while they linked Mills and Byrnes as the instrumental leaders.

The great qualification that must be made on the responses summarized above is that the complexity of leadership in the Committee far transcends the scope of the two questions that were asked. Suggesting ways out of difficult problems, for example, is only one form of task leadership, and perhaps not the most important form at that. Acting as the peacemaker in tense situations is only one form of affective leadership, and it is probably more important to Mills's leadership that the mark of the meetings is the absence of tension, not its relaxation.

What is not shown in the table is that although the members named Mills as the task and affective leader of the Committee, many members qualified their answers with the comment that Mills and Byrnes are the dominant figures on the Committee but any member is free to participate and contribute if he so desires. Leadership is both more diffuse in the Committee than the task-affective dichotomy implies, and there is a relationship between affective relations in the Committee and the way Mills performs the taskmaster function. Task leadership, in short, is more collegial in the members' eyes than the conspicuousness of Mills in the public eye might lead one to expect, and because this is true Mills also rates high as the affective leader of the Committee.

Such a finding should not be altogether unexpected. Simply because leadership functions are separated in some groups is no reason why they cannot be joined in others. There is no reason why a skillful leader cannot operate in such a way that although he is the primary instrumental leader of the group he guides the group in performing its tasks so that he is the main reason why the job gets done, and without much tension. Thibaut and Kelley comment on this:

> The dichotomous separation of task and maintenance roles may not be necessary under all conditions. If . . . there are clear norms as to what each person should do on the common task and when he should do it, instigation to conform to these norms may come from many members, and no person will need to generate much hostility toward himself if he uses the norm. . . . This is an important part of the pattern of behavior referred to as 'democratic' leadership. The democratic leader encourages the members to influence one another in their work on the task. . . . By group decision and participation procedures, he attempts to distribute many of the task functions among the membership at large, and in this manner is enabled to perform a maintenance role himself.[13]

[13] John W. Thibaut and Harold H. Kelley, *The*

Mills is a democratic committee chairman par excellence. In this way, he guides the Committee on tasks and maintains good affective relations in the process.

Seen in this light, one familiar observation on Mills's style of leadership, i.e., that he is a consensus-seeker, an observation made by almost everyone who has ever looked at the contemporary Committee, is of great interest. Mills's search for a consensus in the Committee is another way of saying that he is a democratic chairman of the Committee, a task leader who encourages the members to influence one another, who shares the quest for solutions to problems with the members even though at the end of the process he may articulate and legitimate the conclusion they reach together, a leader who directs the members along lines he may favor but who does not attempt to force his predilections on others. Mills's task leadership of the Committee, subtle and indirect, is an important source of the high esteem in which he is held by the members and a major factor in explaining why the members like the Committee as well as they do.

Several members (six Republicans, three Democrats) touched on this in the course of discussing who suggests solutions to the Committee's substantive problems, but no one put it better than a Republican,

> I don't really understand our mark-up. . . . Many times we don't have a bill. The first two-thirds is like a rudderless ship, flopping all over, anyone brings up anything on his mind. The drafters are right there, we never draft anything, they take their notes and write it up. Mills will say, 'Well, I guess we can agree on this, draft it up.' He gives direction, yes, but anyone can jump in. He's quick to see an idea that will have appeal in the House.

When the members say that the Committee is like a rudderless ship, loosely directed by the Chairman, they are expressing the feeling that the hand of leadership weighs lightly on them in the Committee. "When you get to the [crucial] stage it's Mills and Byrnes. But we all participate. I do. Less than they, but if I have something to contribute, or if anyone else does, there's no problem." A Republican concurred, "I'd say the final legislation is never very far from the way Mills wanted to go, the general direction. He consults with Byrnes: 'Here we have a piece of legislation, where do we go?' They decide the general direction. Now when we get into the Committee there is general participation. A number of us give suggestions." "If the members start from points apart," a Democrat summed up, "[Mills] puts them together to points similar. But I'd say it's *one* committee with a head."

That the members of Ways and Means tend to blur the distinction between Mills's task leadership and his affective leadership within the Committee is evident from the last-quoted Democrat. He was answering, interestingly enough, the peacemaker question, not the problem-solving question, and he depicted Mills as the peacemaker because the Chairman brings the members together on substantive issues. The close relationship between the two leadership functions became even clearer when the members talked about Mill's general style of leadership. When left free to discuss leadership and conflict in the Committee as they pleased, most of them spoke favorably of Mill's consensus-seeking style and many of them linked it in a causal way to the quality of relations among the members. First we will look at Republican perceptions of Mills, then at the Democrats.

To reach a consensus in the Committee Chairman Mills will compromise, bargain, cajole, swap, bend, plead, amend, coax and unite until as much of the controversy as possible is drained from the bill, and as many members of the Committee as possible support it. "Mills, although he may feel strongly about things, is not by nature an uncompromising man, you know, even on some principles he may have. . . He likes to be a leader, to be in front of the troops, and if he sees that the troops are ahead of him then he'll circle around and make sure that he gets out in front again." Other Republicans say that if Mills "finds out my backers have a lot of votes, he'll give in, he'll compromise," "he encourages you to participate and the first 95 percent of the approach is nonpartisan," "our committee is a little unique in that we have a chairman who doesn't let the political dominate," and

> If we had a partisan chairman the Committee would become partisan overnight. In some ways, for political purposes, it might be better to have a partisan chairman. This would unite the minority, we could come out with our political position, and perhaps get some votes in the next election. But I don't think on our committee it should be like that.

Republican appreciation for the way Mills runs the Committee is probably heightened by

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Social Psychology of Groups (New York: John Wiley & Sons, 1959), p. 282. See also Verba's observation that in ongoing groups the development of a legitimate leadership structure replaces dual leadership. Sidney Verba, *Small Groups and Political Behavior* (Princeton University Press, 1961), p. 171.

their awareness that there is no absolute necessity for Mills to treat them well. In fact, a few of them commented on this point. "Some committee chairmen just use their authority and ram things through. Wilbur won't do this." A second Republican refuted an erroneous popular image of the Chairman which he felt was propagated by the newspapers,

> Although consensus is a bad term since Johnson screwed it up, he is a consensus seeker. He never pushes things to votes, we reach a compromise. Nothing bothers me more than to read, as you do in the newspapers, that he's an authoritarian—the 'little authoritarian from Kensett, Arkansas.' That's not it, he's no authoritarian. The reason the tax bill is not going through is because they have to cut expenditures. . . . It's not that Mills is a dictator, the newspapers are all wrong.

A third Republican put the matter strongly, but well, "Anyone who has ever been on another committee appreciates the way Mills conducts Ways and Means. There's almost a reverence among the members about him because they know how it is on other committees."

Democratic members of Ways and Means are no less unanimous in their agreement on Mills's fairness, the considerate way he treats all members, and that, above all, he directs the Committee's task operations with consensus uppermost in mind. Few members of either party would disagree with the Democrat who said, "He's evasive, aloof, coy and he's not a stern oak that stands in the wind and splits rather than bends. He'll bend in the wind." As for sensing the strength of the winds, another Democrat expressed a common view, "He has developed the ability, if two sides are opposed on some issue, to sense what may bring them together in an acceptable compromise." "He *leads* the Committee, he doesn't *drive* it," was a good summary statement of the general view of the way Mills runs the Committe, as was the same man's comment that the Chairman has enough skill as a leader to "retreat with dignity." "He counts the heads in the Committee and he counts the heads in the House, he's always counting." And, while counting, he is compromising. Questions about Mills drew the inevitable comparison, "Now you take Education and Labor, they never try for a consensus, they battle it out. But on Ways and Means we try to reach a consensus. . . . He always seeks a consensus and he's a genius at getting one on complex problems."

If, as one Democrat said, "Mills loves harmony," and will sometimes "abandon his position" to get it, it is also true, as another Democrat said, that the search for harmony is not the easiest way to proceed. "It's long, hard, tedious work, but he seeks a consensus." In the course of this work the Chairman guides but does not continually dominate the Committee's discussion. He does participate more than other members but much of his participation involves commenting on the comments of others, raising searching questions if not objections about various ideas, and, most important, stepping in to legitimize a position which the active members have arrived at under his guidance, not under his urging. This kind of leadership requires a flexible approach to controversies, and an inclination to accommodate the opposing views of the members. In this regard, the comment of an HEW official is instructive,

> Mills is an eminently successful opportunist. He does not announce his position and force it through. He sits and listens to the members and knows what will go. I'd say 80 percent of it is consensus, 20 percent Mills, but certainly not 50 percent Mills.

It could be argued, of course, that this is not real leadership, that all Mills does is sense what the members will accept, fix his position accordingly, and take a firm stand in favor of a sure thing. But this interpretation—which is sometimes made—seriously distorts a complex form of leadership because it does not conform to one aspect of leadership: getting other people to accept one's position. Leadership, as Simmel says, always involves a certain amount of followmanship. On this point, Gouldner argues that one of the most important elements of leadership is the ability to legitimate decisions,

> It is not . . . the individual who first develops an idea or makes a suggestion (not the man 'ahead of his time') who is necessarily a leader, in the sense proposed. The leader would be, rather, that individual who is able by his support or espousement of the proposal to legitimate it. He transforms it into something to which group members are obliged to orient themselves.[14]

There is no question but that Gouldner's description of legitimacy is precisely the kind of leadership Mills exercises in the Ways and Means Committee. Simply because it does not involve early commitments on proposals and dramatic efforts to convince others of the soundness of his position, is no reason to discount the importance of Mills's style of leadership for the policy decisions made by the Committee.

Perhaps the major obstacle to easy and un-

[14] Alvin W. Gouldner, *Studies in Leadership* (New York: Harper & Bros., 1950), p. 19.

qualified generalizations about the scope of Mills's leadership is that one of the prerequisites for consensus-seeking leadership is the ability not only to be flexible on policy but to keep one's position unclear to others. On many issues Mills is inscrutable to the members, they don't know if he has a position or, if he does, what it is. A Democrat praised Mills in these words, "I always said if he were a gambler he'd be the world's best poker player. He plays his cards close up. He'd sit there and you'd never know what card he had until he showed it—and maybe not then!"

The leadership of the Chairman is, then, a process in which Mills "leads but he does it by compromising." Committee members see Mills as the legitimate leader of the Committee, as the central figure on the Committee, as the man who gives the Committee direction and who shapes the decisions of the Committee. But they also see him as a shaper of decisions, not a dictator. *To them he is an extremely skillful leader who responds to them in such a way that his conclusions, drawn from their discussion, become their conclusions.* It appears to matter little to them if in the abstract, before the Committee goes to work, Mills would favor or oppose many of the proposals made to the Committee. On many questions they do not know where he stands and, in any event, there is no such thing as leadership in the abstract. Leadership is part and parcel of the group process and in this process the members concur that Mills is a leader without parallel. One of the primary reasons Mills holds this position is that regardless of what his initial views "really" are he will be responsive to the views of the members, and the members know it.

By stepping in at the right time and suggesting that a particular line of action may be acceptable to the members, and thereby resolving whatever tensions arise, he is seen by the members as "powerful," "smart," "expert," "clever," and a "good synthesizer," qualities that are associated with instrumental leadership, and as "fair," "considerate," "pleasant," "patient," and the "peacemaker," qualities that go with affective leadership.

In concluding this initial look at the way in which Mills runs Ways and Means it is well to remember that in some ways the Committee is jointly directed. Mills and Byrnes together, not just Mills alone, are responsible for the way the Committee goes about its business. Although Mills is the most salient member of the Committee a great many of the policy decisions taken by the Committee (e.g., excise tax reduction and the scope of medicare in 1965) can be traced to Mills and Byrnes. If Mills and Byrnes disagreed over the way the Committee should function the amount of conflict in the Committee would be appreciably higher than it is. As implied, the Chairman and the ranking Republican are agreed on the nature of the Committee's job and how it should be done, if not on all major policy questions.

It is difficult to exaggerate the importance of the Mills-Byrnes relationship to Ways and Means. The Democrats were more inclined to reserve Committee leadership to Mills than the Republicans, but the fact that the Republicans feel that Mills and Byrnes together are the leaders of the Committee helps explain the nature of partisanship in the Committee. Far from being cut off from influence in the Committee the Republicans feel that because of Mills and Byrnes they have as much if not more say than the Democrats. One Republican observed that the Committee under Byrnes would be about the same as it is under Mills. Asked why, he said, "Because we Republicans get so much in now."

Mills's efforts to reach consensus are often directed primarily at Byrnes and the Republicans. Although only two Democrats assigned task leadership to Mills and Byrnes they were two of the most perceptive Democrats interviewed. One was asked why he replied Mills and Byrnes to the problem-solving question, and he answered, "Because they have locked arms on everything, that's why." The second Democrat said, "They get together once in a while but it's not like a group of corporations conspiring to fix prices. With them it's a conspiracy with no conspiring." He explained that, having worked together long enough to know each other's position almost automatically, they take the consensus of the Committee together. This man feels —with strong reasons—that when Mills moves toward a decision, he has communicated, explicitly or implicitly, with Byrnes, and that much of what Mills does is in fact a joint proposal by Mills and Byrnes.

Mills's Perception of His Job

Up to now the analysis of Mills has been based on how the members of the Committee see and respond to his leadership in the Committee. Little has been said about the way Mills himself sees the Committee and its tasks. As expected, there is a close fit between the way Mills sees the Committee's job and defines his role as chairman and the perceptions of the other members of the Committee. To put the matter briefly, Mills thinks that the Committee, in addition to reporting technically sound bills, should report bills which can pass the House,

and the best means of insuring passage in the House is through compromise in the Committee. His reputation and that of his committee are at stake every time a Ways and Means bill comes before the House, and if there is one thing Mills tries hard to avoid it is defeat in the House.

Mills believes that part of the Committee's job is to examine carefully all of the policy proposals it actively considers. Given the complexity of tax law and fiscal policy, and the need to protect the actuarial soundness of the social security system, he makes sure that the Committee is painstakingly thorough in the mark up stage of the legislative process, that it studies the alternatives before reaching conclusions, and that it proceeds cautiously so as to lessen the chances of adversely affecting the economic status of the country, corporations, or individuals. It is up to the Committee to perfect bills before sending them to the House for final approval, and he sees no sense in spending two or three days on the House floor amending Ways and Means bills. Hence the argument that bills reported by the Way and Means Comittee are so "intricate" and "complex," reported only after weeks of concentrated study, that no floor amendments should even be considered unless they are first approved by the Committee.

A workmanlike job on the legislation, however, is only half the task. The other half is to get enough votes in the Committee and in the House to pass the Committee's product. "As I see it," Mills has said, "our job is to work over a bill until our technical staff tells us it is ready and until I have reason to believe that it is going to get enough support to pass. Many of our bills must be brought out under a closed rule, and to get and keep a closed rule you must have a widely acceptable bill. It's as simple as that."[15]

Six years later, in another interview, Mills revealed that his conception of the Committee's job had not changed. "I was always taught by Mr. Rayburn that our whole system was to settle disputes within the committees. It's a waste of time to bring out a bill if you can't pass it. I just don't like to have a record vote for the sake of having a vote."[16]

If Mill's main objective is to report sound, passable legislation the means to this end are clear. In 1967 the Chairman said that reports of his consensus-seeking were highly exaggerated. Perhaps so. But during the course of his remarks he revealed that consensus-seeking is exactly what he does in the Committee. Mills's world is a very practical one in which compromise is the most widely-accepted form of decision-making, and the perfect compromise is a consensus. As the Chairman remarked on the practical nature of the Committee's work,

> Oh yes, there is no place for just the idealist because we're dealing with very practical matters. We're living in a very practical world, we have to find practical answers; generally those answers have to be a compromise of the judgment of the 25 men on the committee, and the 435 members of the House, the members of the Finance Committee and the hundred members of the Senate. So it's a very practical world in which we live and a very practical . . . area within which we operate in the Ways and Means Committee.[17]

Moreover, Mills observed that, in his opinion, reaching a consensus in Ways and Means is not a mysterious skill which only a few can understand: you sit, listen and, in the end, compromise.

> Oh to get a consensus, I think of the Ways and Means Committee or of any other group is a relatively easy matter. If you sit and listen long enough, you don't have to talk, you can pretty well find out what the 25 members are thinking about, what their primary interests are with respect to the particular issue, how they would like the legislation dressed a little differently from the way it's introduced maybe. What particular provision might satisfy a group of the members, another provision that might satisfy another group. It's the easiest thing in the world, I think, to get a consensus.[18]

It need hardly be said, as Mills himself observed, that not everything connected with a particular piece of legislation can be compromised. "But there're so many areas of any bill that are less important than the main issue of the bill that there's always a way to change some little method of doing something."[19] And, underlying this approach to the legislative process in Ways and Means, is the following belief: "I think if I can get a vast majority of the membership of the Ways and Means Committee to agree upon something, that I've got a vast majority of the

[15] Quoted in Charles B. Seib, "Steering Wheel of the House," *New York Times Magazine*, March 18, 1962, p. 146.

[16] Duscha, *op. cit.*, p. 76.

[17] "Interview with Congressman Wilbur Mills," for "Operation Government," Westinghouse Broadcasting Company, October 11, 1967, p. 3. I am indebted to Stephen Horn of the Brookings Institution for a copy of the transcript.

[18] *Ibid.*, p. 3.

[19] *Ibid.*, p. 4.

House agreed upon the same thing. Because our committee is a cross section of the membership of the House. Just as it is a cross section of the people of the United States."[20]

Mills's theory about the Committee as a microcosm of the House has yet to be tested, but at present the importance of his conception of legislation is that it leads him to look for a "vast majority" of the votes in the Committee. As a result, Ways and Means bills, in the words of an HEW specialist, are all "negotiated items so they aren't so partisan. Mills will work it around so that 80 percent of the Committee will support it." A Treasury Department official agrees, "Sometimes he'll give up and settle things by party votes but he prefers not to do this." A member of the Committee's staff has the impression that Mills is "happiest when all 25 members agree and the House is unanimous." To achieve this end Mills will, in this man's opinion, "take a tentative position and then if he sees that the Committee is not likely to go along he'll make it clear that this isn't really what he wants anyway." Not surprisingly, the staff man also believes that the Chairman "has the most acute antennae of anyone I've ever seen. He can gauge the sentiment of the House and the Committee extremely well." Why all this? "He doesn't like to lose." At the risk of belaboring the point, one more view—from another HEW employee—will be cited:

> Mills wants more than anything not to have a minority report. He wants at least 20 votes and one way he does it is drop out anything controversial. I don't mean just major policy questions that may be controversial but anything. If they come across a provision and some member raises an objection, he'll drop it out. They go through it once, make tentative decisions, again, and drop out anything controversial. If Burke were chairman it might be different but Mills is amazing. If he started by saying I'm for the Administration position then all the Republicans would gang up, but he doesn't do it like this.

Because Mills is the chairman of an enormously important committee, and because he has acquired a position of legitimate leadership to go with his formal status, many people in the Washington community enjoy trying to explain why Mills leads the Committee as he does. Some dismiss him as a man who has a built-in aversion to taking chances, others see him as a man who does not have strong feelings about any policy in particular and so is willing to hold back until the feelings of others make victory certain if policy is formulated according to the least common denominator. Mills's admirers—most notably all the members of the Committee save one—see him as "the most capable man in the House of Representatives." His detractors, like one House liberal, explain his influence in the House as a "myth that he always has his Committee under control," and argue that even if he does it is because he has no real competition for leadership in the Committee.

Amidst the conflicting opinions of Mills and the reasons which ostensibly explain why he is influential, none is more common than the observation that he will do practically anything to avoid losing a bill on the floor of the House. Several members of the Committee noted that Mills lost one of the first major bills he brought to the floor and this loss is supposed to have had a permanent effect on the Chairman. It made him cautious.

But this explanation ignores the possibility that Mills might have expected—indeed planned—his first defeat. Mills claims that his first so-called defeat was in fact part of the strategy he used to pass a controversial unemployment compensation bill.[21] By bringing a two-part bill to the floor Mills contends that he was willing to sacrifice one part in order to get the other passed. The controversial matter thus acted as a lightning rod, drawing criticism away from the other section which, on its own, would have had trouble in the House. A recommittal motion extracted the controversial title and the bill itself was passed. If this is true Mills's humilating loss was in truth a clever—if little-known—victory.

It would be naive not to admit the possibility that Mills's explanation of his first defeat might be a post hoc rationalization (at least one reporter who was on the scene in 1958 thinks it is), but whether it is fact or fiction is, for our purposes, irrelevant. Even if the Chairman's first loss had no lasting impact on him, as it probably did not, his behavior since 1958, and his view that policy disputes should be resolved in the Committee, make it highly probable that the tenuousness of power in the House, which is less often stressed than the concentration of power in the committee chairman, is an important constraint on how he conducts the Committee. Mills is probably well aware that underneath the "cocoon of good feeling" which envelops House members the House is sentimental, even maudlin, about everything but who has power. His reputation in the House is on the line every time he commits himself to the passage of a bill and, although one defeat is not necessarily disastrous, a few defeats and it rapidly becomes known that he has lost his grip. To guard

[20] *Ibid.*, p. 5.

[21] *Ibid.*, p. 7.

against this Mills tries for support from both sides of the Committee table. Although he can win with just the 15 Democrats behind him it is much more comfortable to have bipartisan support. In any event, his leadership is conducive to the maintenance of harmony in the Committee and to the completion of a quality job on the complex legislation considered by the Committee. When rough spots appear in the interaction of members he tries to soothe them just as he tries to iron out rough spots in the bills. Whether Mills is conscious of it or not, this is what he does and this is why his leadership is effective.

II. MILLS: INFLUENCE AND EXCHANGE

Influence, not power, is the concept to use in thinking about the leadership of individuals such as Chairman Mills. Power means many different things to many people but intuitively it conjures up the notion that individual A who "has" power, can through the expression of his will, activate obligations on the part of B which stimulate B to do something he would not otherwise do.[22] Despite the general agreement that power is a relational or transactional concept, of some presumed utility for describing and analyzing political relations, the tendency to speak of it as a commodity which some persons have and others do not will not die. So too with influence, another relational concept, which for purposes of shorthand is spoken of in commodity terms when, in fact, it refers to relationships among people. Thus it should be kept in mind that in using the term influence in connection with Mills all that is meant is a relationship between him and the other members of the Committee, not something he has and they do not. This, of course, is in keeping with the notion of leadership used in Part I.

The reason why influence is a better concept to use in analyzing Mills than power is not that it has fewer theoretical problems. Nor is it that it is easier to devise an operational definition of influence, and measure it, than of power. The main reason is that the theoretical and empirical meaning of influence is much closer to the kind of relationship between Mills and the Committee than the meaning usually given to power. Influence, as Parsons points out, may be distinguished from power in the sense that influence is, in essence, a means of *persuasion* which involves giving reasons or justifications for doing certain things and avoiding others, whereas power may be taken to mean the communication of decisions which activate obligations.[23] Power has a much more direct connotation than influence. When one thinks about power between A and B there is a tendency to view the relationship as unidirectional, $A \rightarrow B$; with influence, the relationship is more apt to be seen as a mutual process of stimulation, $A \leftrightarrow B$. The essence of a consensus-seeking form of leadership, the kind practiced by Mills, is mutual dependence, a relationship between Mills and the Committee which involves the flow of influence from the Committee to Mills just as much as the flow from Mills to the Committee.

Interestingly enough, two Committee members, in response to a question about Mills's power in the House, drew their own distinctions between power and influence. Most of the members of the Committee were content to talk in terms of power but these two members argued that Mills is best thought of in terms of influence, and it is safe to assume that the rest of the Committee would agree with their formulation of the distinction. The first member, reacting as though using the term power in connection with Mills was somehow unrealistic, said,

> Power—you mean influence? In the sense of influence? I agree with that. He's considerate. He's as considerate of the most junior member as he is of the most senior. That's why he's 'powerful.' In the Committee, too, it all starts in the Committee. I don't think Mills is the wizard he's made out to be, it takes him time, he doesn't always see the full meaning of what you're doing. But he does, finally, through hard work.

The second member, also a Democrat, compared Mills, the influential Chairman, to Carl Vinson, the powerful former chairman of Armed Services, as follows:

> I wouldn't use the term powerful, I'd say influential. There's a difference. I'll compare him with . . . Carl Vinson. Carl had power and he used it, he wasn't afraid to use it. Mills is different. He has influence. I don't mean influence in the sense you peddle influence. It isn't you do this for me or on a committee assignment. . . . It isn't this way with Mills. He can always bring you together. He has such great respect and influence.

In further elaboration of the differences be-

[22] This definition of power is, of course, taken from the definitions of Parsons, Dahl and Catlin. See Talcott Parsons, "On the Concept of Influence," *Public Opinion Quarterly*, 27 (Spring, 1963), 37–62; Robert A. Dahl, "The Concept of Power," *Behavioral Science*, 2 (July, 1957), 201–215; G. E. G. Catlin, *The Science and Method of Politics* (New York: Alfred A. Knopf, 1927), pp. 222–223.

[23] Parsons, *ibid.*, p. 48.

tween influence and power Parsons, in response to a critique by Coleman, insisted on the utility of the distinction, adding that influence is backed by: (1) arguments; and (2) giving or withholding of approval or disapproval. Power, on the other hand, is a way of activating obligations backed by the threat of negative sanctions.[24] This distinction between the bases of influence and power is useful, but it is still too nebulous for purposes of analyzing the Ways and Means Committee. The difference between a negative sanction and withholding social approval, for example, is not too clear, and in any event it is possible to think of a more detailed typology of the bases of influence than Parsons' two-part breakdown. Focusing on, in Coleman's words, "the investments that a person makes in another which permits the other to have influence on him,"[25] draws attention away from the influencer to the influenced, and with this focus we will discuss five bases of Mills's influence. The five bases of influence are adapted from French and Raven's useful classification of the bases of power, as follows:

1. *Expertise:* based on Mills's knowledge of the subject matter.
2. *Legitimacy:* based on Mills's rights as the formal leader of the Committee.
3. *Rewards:* based on what Mills can do for the members in a positive way.
4. *Reference:* based on the identification of others with Mills.
5. *Sanctions:* based on what Mills can do, in a negative way, to promote compliance with his objectives.[26]

[24] For Coleman's critique see James S. Coleman, "Comment on 'On the Concept of Influence,'" *ibid.*, pp. 63–82. For Parsons' rejoinder see *ibid.*, pp. 87–92.

[25] *Ibid.*, p. 67.

[26] John R. P. French, Jr., and Bertram Raven, "The Bases of Social Power," *Group Dynamics*, eds. Dorwin Cartwright and Alvin Zander (Evanston, Ill.: Row, Peterson & Co., 1960), pp. 612–621. These may not be the only bases of power which can be distinguished but they are more inclusive than other typologies, such as Weber's threefold classification of authority. For Weber's treatment of charismatic, rational-legal, and traditional authority see Max Weber, *The Theory of Social and Economic Organization*, ed. Talcott Parsons, trans. A. M. Henderson and Talcott Parsons (New York: The Macmillan Co., Free Press, 1964), pp. 328–363. I have substituted the term sanctions for Raven and French's "coercive" power because of the connotation of the latter, and their third base of power, legitimacy, is considerably different from the way it is used here. By legitimate power they mean internalized values which dictate that someone has a right to influence someone else. As used here legitimacy refers more to the decisions Mills can make because he is Chairman, and how these decisions affect his influence.

1. *Expertise.* As befits a leader whose style is that of persuasion—giving "facts" from which others can draw their own conclusions—Mills, as he is seen by the members, has great expertise in the abstruse areas considered by the Committee. When asked to explain Mills's influence in the Committee no reason was given as often as the Chairman's knowledge of the subject matter, and its only serious rival in importance was his fairness. The evidence gathered from the members illustrates perfectly Blau's comment that, "A person whose demands on others are fair and modest relative to the great contribution he makes to their welfare . . . earns their approval."[27]

Mills has earned the approval of the Committee members on his hard work and his mastery of the subjects coming before the Committee. Moreover, by becoming an expert Mills has reduced the cost of Committee membership to others. Those members who are unsure of the answers to complex questions can rely on his judgment, safe in the feeling that he knows what he is talking about, and those members who do not care to immerse themselves in technical complexities can let him do it, confident that he knows what he is doing. By acquiring some degree of expertise the Chairman has allowed other members "to perform rewarding activities with less effort, less anxiety, or in less time—in general, at lower cost.[28] And by lowering costs to others Mills has raised his own influence.

A Republican member of the Committee articulated the theme that the members feel secure with Mills because of his expertise. "His knowledge, the fact that he does his homework, is in the back of why they feel safe, why he gives them security." Referring to the complexity of the subject matter, he said, "They know he won't lead them into anything, and there's a lot of things to get into in these things." "He knows the bills thoroughly," a second Republican observed. Mills "knows what he's talking about," said another, and three of his colleagues agreed:

> Mills is preeminent. He's a real student. I don't know what the Democrats will do when he goes. He was the real leader before he became chairman

[27] Peter M. Blau, *Exchange and Power in Social Life* (New York: John Wiley & Sons, 1964), p. 201.

[28] Thibaut and Kelley, *op. cit.*, p. 109.

because he studies so much. Leadership depends on hard work and knowledge.

He knows the tax code inside and out and he knows what Ways and Means has done for the last twenty years. He can cite and does cite section after section of the code.

He's so single-minded, never goes out, no social life or cocktail parties. He's thoroughly absorbed, goes home and thinks about the legislation.

Democratic members of the Committee are as impressed—if not more so—with the Chairman's ability as the Republicans. They too see him as a man who has put years into the study of Ways and Means business ("work is his hobby"), knows more than they do ("take the experts, the specialists who have spent 40 years in the subject, apart"), has great natural ability ("best mind on the Committee"), understands the material ("isn't anything in taxes he fails to understand or fails to relate to what has gone before and tie it into today"), and who, with all of this, is fair and considerate in his relations with them. A Democrat summed up the general opinion, "He's an astounding man."[29]

The above odes to Mills's expertise and ability, qualified by only a few members who attributed Mills's influence more to hard work than to great native intelligence ("he's a plodder") help explain why he has great influence with the Committee members. Some of them see in his expertise great value to them. "He sort of quarterbacks it and lets me off. Takes the burden off me." But just as important as the general benefits they derive from his knowledge is the fact that he is inclined to use his knowledge not as a barrier to their policy objectives but as a guide to finding acceptable compromises. He is perceived as an expert who sometimes argues persuasively against proposals but also as a pragmatist who more often hits upon, as one member put it, the "honorable agreement." When he does this the members, more often than not, will settle for the compromise he offers as the best they are likely to get.

2. *Legitimacy*. Committee chairman are not ipso facto leaders in Congress, but they can become leaders if they have certain kinds of skills, and certain kinds of followers. The kinds of men on Ways and Means are men oriented toward bargaining, accommodation, peaceful settlement of disputes, and the subordination of specific policy objectives to the maintenance of the power and prestige of their Committee. They are men who actively strive for legislation but who operate on the principle that the legislative process makes one fundamental demand, patience. The question now before us is, what does it take to exert influence in a decision-making system composed of men who are, in theory, peers, and who must make important decisions while they are engaged in close, long-term relationships with one another?

Seniority is one answer the members of Congress have given to this question. Seniority is nothing more than a norm which organizes the decision-making process, giving to the senior members the right to make certain decisions which structure the legislative process. The list is familiar: control of the legislative agenda, appointment of junior members to subcommittees, organizational questions such as whether or not to have subcommittees, how many subcommittees there should be, management of the committee staff, the right of recognition of members who desire to speak, and general leadership in the shepherding of bills through the House. But none of these rights guarantees influence to the senior leaders. They are resources to be used in meeting the demands of the members or in reducing the costs of committee membership to them, and only if they are used in this way will there be a relationship between seniority and influence.

Homans, in discussing the successful leader, illuminates the basic qualification that must be placed on the formal prerogatives of senior leaders in the House: "If his chief external job is to be successful, his chief internal one is to be just. 'He's fair,' are the words in the mouths of his followers from which all other praises spring."[30] The successful committee chairman in the House is the just chairman in the committee, justice meaning in this case distributive justice, a favorable balance between rewards and costs (inducements and contributions) for the members resulting from the actions of the chairman. If the chairman so uses his prerogatives he enhances his influence with the members. If, however, the costs of the way he performs his leadership functions exceed the benefits to the members he may be able to implement his will for a time but his influence with the members will decrease, and if it decreases to a certain point the members, as they have done in some committees, may strip him of all but the gavel.

Put baldly, the argument applied to the Ways

[29] Similar comments were received from staff members and executive branch specialists. The following is typical: "He has a memory like an elephant, never forgets a thing, and he can tell you more about the welfare administration than most people in this building."

[30] George C. Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, Brace & World, 1961), p. 295.

and Means Committee is that the rights granted to Mills because he is chairman are sources of influence because he exercises them in such a way that the members approve of his leadership. By so doing Mills is seen by the members as a chairman who makes important contributions to their welfare and, in Blau's words, "their common approval of his fairness, reinforced by their consensus concerning the respect his abilities deserve, generate group pressures that enforce compliance with his directives."[31]

"He has the ability and he's very fair," was the judgment on Mills by one Republican, and no one interviewed for this study disagreed. Even the one member who was highly critical of Mills said, "He is considerate of the fellow members, no one is more considerate. Don't get me wrong—I have to say his good points. He knows his stuff." A Democrat declared, "He is fair. He doesn't ride roughshod over anyone, although it has happened—it's been known to happen. But it's rare that he's roughshod." According to a Republican, "If I have an idea he's always ready to hear it out, to listen, and if he doesn't agree he says so and we go from there." This man compared Mills to another chairman,

> If Wilbur said what the hell there's 15 votes why not vote it through, don't waste all this time, then of course it'd be different. Jere Cooper was this way. Hell, he'd get a social security bill one day and report it the next.

Another Republican noted that Mills does not have to call bills up if he does not want to, and there is little the Republicans could do to force him to act. "I think it's a tribute to him that he lets them be aired." This man also related how one of his early experiences on the Committee bound him to Mills,

> Inside [Ways and Means] complete fairness so I feel attached to him. I remember when I was lowest man on the totem pole and I had some interrogation I wanted to do and there were a couple of secretaries there but it was late, 5:00 or 5:30. He said we'll come back in the morning the next day to hear Mr. ....., and they had to come back. I appreciate that.

"The Chairman," said a junior Democrat, "always allows everyone to get his questions in." If anything, Mills runs the Committee meetings in such a loose way that his patience with some members sometimes aggravates—while it impresses—other members. A Democrat, for instance, said,

> He'll listen forever. We have one member . . . who can never get enough information. I once said we'd need a building as large as the Rayburn Building to store all the information he wants. He's always asking for more information and Mills gets him more, more, more, more, more and more. He's a fine chairman. I don't think you'll find anyone who wouldn't say that about him.

The man who said Mills never rides roughshod over anyone summed up: "The overriding thing is his patience. He just keeps going and going and going and he *outlasts* everyone else. This is the most important thing. He is fair."

Viewed from this perspective, the contribution of his legitimate prerogatives to Mills's influence is not that he has certain rights but that he uses them to win approval from the members. The question of the staff is a good example. As one of the more perceptive members put it, "Sure all the members have the staff but they are his men first—the Chairman's men. *And the reason they are is that he does the work.*" The staff gives Mills's needs precedence over other members' needs but they are available to the other members and most of the members are quite satisfied with the staff. A few think the Committee could use more staff but there is no widespread demand for more and, if there were, the Chairman would probably be receptive to hiring more people. The point is that an expert staff in the hands of an inexpert chairman can be a liability instead of an asset, and the reason the staff professionals are seen as the "Chairman's men" on Ways and Means is not that he is chairman but that he is the kind of chairman he is. The other members do not begrudge him the staff because they know that he puts more into the Committee than they do and consequently has more need of the staff.

Another good example is the Chairman's decision not to use subcommittees for legislative purposes. Committee members prefer to work without subcommittees but it is no doubt true that the absence of subcommittees makes for a highly centralized operation in the Committee, an operation centered around Mills. As a Democrat said, "This means that he is there with five or six members who would normally be a subcommittee and he is chairman of all the subcommittees." But the main significance of conducting the Committee's legislative business in plenary session is that although it makes for high centralization the members do not mind. In fact, with Mills as the center they prefer it to the normal subcommittee arrangement. No doubt the members realize that this style of operation increases the opportunities for the Chairman to influence their decisions but the Chairman's influence is precisely what they like about the Committee. "If we had a new chair-

[31] Blau, *op. cit.*, p. 202.

man we might have subcommittees," a Democrat commented, "someone who wanted to delegate. But as long as Mills wants all the responsibility we're willing to let him have it. We don't have white hats as I told you. A lot of the bills are tough." A second Democrat, when it was suggested that perhaps one reason Mills does not establish subcommittees is that it might lessen his control on the Committee, gave the revealing reply that, "No, Wilbur just isn't that kind of person. He's too big for that. I think the reason is just because it'd be hard to get the members interested enough to attend." And a senior Democrat said that if he were chairman he would *have* to have subcommittees because he could not be as central a figure in the Committee as Mills: "I'd have to have two or three members around me. I don't have the ability or the interest to do it like Mills. I might need two or three subcommittees." For this man, were he to become chairman, the establishment of subcommittees would be a sign of weakness and the need for decentralization, not a decision by which he could enhance his own influence. To save some influence he would have to use his authority to share the potential for influence with others. For Mills, the authority of the chairmanship is used to buttress the centralization of the Committee, to centralize the resources of influence, and no one seems to mind.

In considering legitimacy as a base of influence for the Chairman it is important to remember than many of the mundane functions of the Chairman which may increase his ability to persuade the members on certain questions fall within what Barnard calls the "zone of indifference."[32] For a member like Thomas B. Curtis (who left the Committee in 1968) the zone was very narrow and he constantly supervised Mills to make sure that the Committee was, by his standards, well run, an oversight function which on occasion irritated the Chairman. For other members, however, the zone of indifference is comparatively large and may include everything but how to cast votes in the final stages of the Committee process. On many questions the members are content to rely on Mills. Ranking the tedious Committee meetings low on their list of priorities, these members open up a wide area in which Mills can make decisions in a relatively unrestrained way. Mills, characteristically, has a good feel for the boundaries of his autonomous zone and when he receives requests from members which fall within it, such as having someone in particular testify before the Committee, he accedes and thus insures the continuation of his general control over such matters. It is conceivable, indeed likely, that decisions such as allowing executive department representatives to participate in the executive sessions of the Committee affect the decisions of the Committee, and may affect them in ways desired by Mills, but few members care deeply about such matters. For the most part, the members are willing to let Mills run the Committee because on many questions of procedure he consults them and on nearly everything he runs the Committee, with or without consultations, to their satisfaction. By so doing, Mills maximizes the influence potential of his formal powers while minimizing the complaints that are sometimes heard about other committee chairmen in the House. Thus he is considered by one member (and nearly all) as "the best damn chairman in the House, best I've seen in all my years in the legislature."

3. *Rewards.* Bargaining, regardless of what form it takes (logrolling, side payments, splitting the difference), is peculiarly suited to a decision-making system in which every man has some influence, no man and no group of men has undivided control over important resources of influence, and in which men who are in a formal sense equals must bring a common interest out of a diversity and conflict of interests. Such a system is built for bargaining, as those who built it—with their concern over factions—no doubt realized. And such a system, if it is to function smoothly, requires agreement on one fundamental norm: the duty to reciprocate assistance or rewards received from others.

Most of the exchanges that take place in Congress are subtle exchanges to which political scientists and others have given inelegant names: logrolling, *quid pro quo,* and backscratching. Normally there is little need for the members to spell out what is expected when one gives or receives assistance. "He keeps his word" is high praise in Congress but the word, if the relationship is good, need not be spoken. Most of the bargaining in Congress is, in short, implicit.

To become an influential leader in such a system requires much more than seniority. Influence in Congress is earned, not bestowed. Those men whose influence transcends their own vote have made use of whatever limited formal advantages they are given plus—and this is the vital factor—their skill at exchanging benefits with their colleagues. No one, neither the Speaker nor the most influential committee chairman, commands; he negotiates.

Compared with leaders of many organiza-

[32] Chester I. Barnard, *The Functions of the Executive* (Cambridge, Massachusetts: Harvard University Press, 1956), pp. 167–70.

tions, committee chairmen have few material incentives or rewards with which to negotiate.[33] A committee member's salary, for example, is not dependent upon how well he performs in the committee, his attendance record at committee meetings, or the number of hours he spends each week on committee business. Nor does the chairman have control over other common rewards, such as stock options, insurance plans, paid vacations, and the like. He may, of course, allocate committee funds so some members can travel to boring conferences in exciting places but in the case of the Ways and Means Committee the Chairman is notoriously frugal on such matters. How, then, does Mills, by using the limited rewards and favors at his disposal, induce the members to make contributions to the Committee's work and to his own objectives?[34]

Before discussing Mills's use of rewards it should be noted that by constantly guarding the Committee's reputation in the House he is in effect protecting one of the most significant rewards the members have. Like leaders of all groups the Chairman relates the organization to others and this function is particularly important in the House where the tendency is strong for committees to be equated with and judged in terms of the chairman.[35] If the Ways and Means Committee ranks high in the House it is partly because of Mills and his leadership.

To some people decision-making through bargaining and exchange of favors has pejorative connotations. It contrasts with making decisions according to a normative "rational" model: Step 1: problem; Step 2: weigh pros and cons of alternatives; Step 3: study merits; Step 4: make decision. Congressmen are no less sensitive to the pejorative aspects of exchange than other decision-makers; indeed, given the persistence of the rumor that legislatures are bastions of log-rolling, congressmen are probably more likely than others to deny or discount the importance of the exchanges that occur. This makes gathering of evidence on exchange in Congress difficult. Bargaining is an intensely personal relationship among the members, one they are reluctant to discuss with outsiders.

It is true, nonetheless, that bargains, implicit or explicit, are made in Congress, and that exchanges of rewards and favors are inevitably associated with the decision-making process.

But going beyond the assertion that men such as Mills use rewards and favors to build good relationships with other members, though necessary, is not easy. With the difficulties clearly in mind, it was decided to attack the problem by asking the members of the Committee, point-blank, how they stood with Mills on the exchange of rewards and favors. It was clear that Mills was the source of at least some rewards for the members, in addition to the rewarding aspects of his leadership discussed above, and it was felt that Mills probably did more for the members than he requested of them. If true, this is another reason why the members are inclined to respond favorably to his leads and, though not the only reason for his influence, is a reason.

Accordingly, 18 members of the Committee (ten Democrats and eight Republicans) were asked the following question:

> In the normal course of passing legislation Mills has the opportunity of doing a number of things for the members. He also calls on members to do things for him occasionally. In your relations with Mills how do you stand: would you say you have done more for him, he has done more for you, or are you about even?

Since no political scientist had ever reported the results in such a question, or probably even asked about exchanges in such a direct way, it was not known what to expect. The results, despite the good reasons for going after such information in a more subtle way, were better than expected. The question had the merit of posing the issue starkly and forcing the members to confront it explicitly, perhaps for the first time. The results, of course, have to be handled with care but I deem them of sufficient validity and interest to warrant reporting here. By the time I asked the question most of the members were aware that I had done a good deal of research on the Committee, rapport had been established with most of them, and though a few appeared to be taken aback no one seemed to take offense that such a question would be asked.

Table 2 presents the responses. As expected, most of the members replied that in their opinion Mills has done more for them than they

[33] See Barnard's discussion of incentives, *ibid.* pp. 139–149.

[34] Parsons uses the term sanction to include rewards and punishments. Talcott Parsons and Edward A. Shils (eds.), *Toward a General Theory of Action* (New York: Harper Torchbooks, 1962), p. 191. Rewards and sanctions are distinguished here because the former increase the atrtactiveness of the giver, while the latter decrease his attractiveness. On this point see French and Raven, *op. cit.*, p. 615.

[35] On the importance of this function in a very different setting see William Foote Whyte, "Corner Boys: A Study in Clique Behavior," *American Journal of Sociology,* 46 (March, 1941), p. 661.

TABLE 2. MEMBERS' ESTIMATES OF THEIR BARGAINING POSITION WITH THE CHAIRMAN

Responses	Democrats	Republicans
1. Mills has done more for me.	2	5
2. It doesn't work that way.	4	2
3. We are about even.	3	1
4. I've done more for him.	1	0

have for him, a close second was the response that it does not work that way in the Committee, four members felt they are about even with the Chairman, and only one man felt the balance favored Mills. As we shall see, this man's reply was one of the most interesting received.

As was not expected, but perhaps should have been, the Republican members of the Committee feel more in Mill's debt than the Democrats, a finding which supports the rather common Republican observation that Mills tends to take the Committee Democrats for granted while he pays relatively more attention to the minority members. More than half of the Republicans who were asked the question, and fully half of the ten Republicans on the Committee, said that Mills has done more for them than vice versa. Only two of the ten Democrats replied in kind.

Two Republicans who said Mills has done *much* more for them than they have reciprocated revealed how they felt about Mills in the process, and why it is that on some questions they are likely to be influenced by him. The first Republican stressed Mills's loyalty to the members of the Committee, regardless of party affiliation. "If I want a letter any time for my campaign, support for reelection, he'll give it. A picture with him—any time. He's loyal with his members and I appreciate this." He also discounted his ability to repay the Chairman: "He's a hundred thousand watts and I can't light that candle." When asked if he ever used a letter from Mills in his campaign he said no, but that he has used a picture of himself with the Chairman. Moreover, he thinks the picture has helped him,

> I think it helps you that the Chairman hasn't put you in the isolation box. I'd love to have him come out to my district and give a speech. Not an endorsement, just a speech. I'd love to show him to my businessmen. Why, he'd draw a crowd twice as big as I do. I think he's the top fiscal brain in the country.

The second Republican has had Mills out to his district for a speech. Some local people contacted him about inviting Mills and he took the matter up with the Chairman. His approach to Mills was impersonal: he told the Chairman that he knew how busy he was and would understand if he could not take time to make the trip, but that these people did want him to come, so, if possible, it would be a nice thing to do for them. Mills made the request very personal by asking the man, "Do *you* want me to do it?" The member, a bit reluctantly, said yes. Mills did him the favor.

But this is not the end of the exchange between Mills and the Republican, it is the beginning. The Republican member and Mills both know, undoubtedly, that repayment of the favor will not involve defecting from the Republican side on a major party issue. It may not involve anything specific at all for, as the member said, "Wilbur Mills is not the kind to ask you on specifics." It is important to understand that the second step in the exchange is implicit, vague, and if Mills ever directly asks the man for assistance on something neither one will have to recall the Chairman's trip. All the trip did was lay the foundation for future exchanges and, since Mills did the first favor, its value may never be completely exhausted.

Similar favors were mentioned by other Republicans but more revealing than a catalogue of the little rewards Mills gives the Republicans is that the two Republicans who said it does not work that way in Ways and Means meant that their relationship with Mills is based more firmly than an emphasis on reciprocation of benefits would allow. Both men admitted that they have received kindnesses from the Chairman but that this plays a minor part in their relationship. A senior Republican thought the question was a good one but had difficulty answering it,

> It's a good question but I don't know. . . . I never think in terms of due bills and maybe this is more applicable to the fellows farther down the table. When you get to the top you want to avoid due bills. I don't think anyone keeps a ledger. I certainly wouldn't want to get into a position of owing the Chairman something for what he's done because I'd never know what it would be I'd have to pay off on. We cooperate but it's not on that basis. . . . What most of the fellows want he can't give them, they're so big, so I don't know how much of this there is, actually.

His junior colleague also discounted the importance of favors in accounting for Mills's influence. He argued that if Mills can convince him, fine, but if he cannot he will not follow the Chairman.

Doesn't operate that way. I suppose it's an unwritten rule. . . . You see in the press about member's bills but it's not as easy as the Chairman saying Jim's a nice fellow let's give him this. It takes unanimous consent, one objection is usually enough, and for most of them the departments are agreed. They have to be good little bills. I suppose Mills by objecting has control over all of them. I can honestly say Wilbur Mills has never asked me for a vote on anything. It doesn't operate that way. If he can convince me, fine, but I haven't seen much of the other generally. I'm a representative of my people and he's a representative of his, and we look at the merit.

A final subtlety that came out of the interviews with the Republican members is that Mills is so cooperative and helpful with the members that they impose their own limits on what they ask of him. The Chairman's willingness to do favors for them is its own control on what they request. "In fact," said one Republican, "he's so cooperative and agreeable that you, I do at any rate, hold off on taking advantage, don't want to ask too much. If it's something big but not quite right yet I'll hold back; something small, I'll go to him and he'll say remind me to bring that up." Like others, this man has received praise from Mills which he has used in his campaign for reelection.

Democratic responses to the reward question, as indicated, were much more in the direction of discounting the importance of favors than the Republicans. Two of the Democrats, however, did say that Mills has given them more than he has received. One of them cited three bills which were passed with Mills's help and, he felt, because of Mills's help. The other said, "He's never refused me on anything—any reasonable request I've had—*and I've never asked for anything unreasonable.*" He feels that he has repaid Mills by, among other things, speaking for Committee bills on the floor where his reputation as a conservative Democrat might help get some southern votes, but in the light of the Chairman's own influence among the southerners he feels that his efforts may be a little redundant.

One important point needs to be made about the Democratic responses: although it is true that the Democrats are less apt to say that Mills has done more for them, they feel that, (a) he has done a lot for them; and/or (b) if they ever need his help he will very likely give it. Five of the seven Democrats whose answers fell in categories two and three made one or the other comment, or both. The other two claimed that in Ways and Means merit alone determined all the outcomes.

To digress for a moment, one member who denied the importance of the *quid pro quo* in Ways and Means stressed the importance of group-identity in the Committee. "You have to understand that we are 25 men in very close relationship. We have rapport with each other. It isn't *quid pro quo*. We do things for each other but it's not on a *quid pro quo* basis." What this member seems to be saying is that the predominant form of exchange in the Committee is implicit, as it no doubt is, but this does not mean that exchanges do not take place. Indeed, it might be argued that in a cohesive group exchange will probably always be implicit and, if the members ever reach the point where transactions are overt, the group is probably in the process of disintegrating. That Committee members help one another without a signed contract is evidence of a highly integrated group, not a group without exchanges occurring among the members.

Returning to Mills's exchange relationship with the members, it bears noting that he interacts with the members in such a way that they sometimes feel indebted to him without quite knowing why, and without being conscious of what is exchanged between them. A Democrat who answered "about even" reflected on his relationship to the Chairman in a way that merits close attention,

> I've probably done things for him but I'm not aware of any. I'm not conscious of it. It's not a transferral of mortgages with Mills. There are a lot of mortgages around here, all of a sudden the paper is presented and the mortgage falls due. But not with Mills. I suppose I've done things for him but I can't remember any. That's the kind of man he is. No reasonable request of mine has been turned down.

His relationship with Mills, in short, is much more nebulous than one based on an explicit series of exchanges, but, for this reason, he is probably that much closer to the Chairman.

Yet the vagueness of the exchanges should not be overdone. There was nothing unclear or imprecise in the statement of the Democrat who said, "It's pretty much a one-way street. Me to Mills rather than Mills to me." What is most interesting of all—and this serves as a warning against the proposition that an influential man is one who makes debtors out of others through the skillful use of rewards—is that this man, far from feeling that he had a claim on Mills, was, of all the members interviewed, probably the most likely to be influenced by Mills. The symbolism he used to describe his situation was extreme but revealing: "It's like being allowed to touch the hem of the Lord's gown. That's how it

was with me." This statement, which was the epitome of respect for Mills, was made after the man said,

> Mills is autocratic as hell, but he doesn't make you feel that he is. He'll give you just enough things, let you ask enough questions, so he'll have you. This is what he does: you have something you want, you bring it to him, you bring the people who are on your back to him, and after making you sweat he'll let it in. And then he has command of you from then on. I always thought of myself as one of his boys. He's aloof but he'll bring you in.

Asked by the member for support on a proposal, Mills, after an appropriate wait, gave his support, gained the man's backing, and, by not alienating the member, kept his backing until it would be useful to his own purposes. With this member Mills probably never had to ask for assistance; the member no doubt looked for ways of helping the Chairman.

At one end of the continuum Mills may do no more for a member than agree to spend a few minutes listening to someone who has asked a member to set up a meeting with him. This costs Mills time if not agreement with the supplicant's proposals but it allows the member who acts as the middleman to impress those who have access to him, and it makes him indebted to the Chairman. Mills listens patiently to the interest group representative and the representative, informed beforehand by the Committee member not to expect Mills to give any unequivocal reaction, leaves content with having had the opportunity to state his case where it counts. To the uninitiated lobbyist this can be a disconcerting experience. One former lobbyist found Mills "always cooperative, courteous, but you could never get Wilbur Mills to take a stand. If you didn't know him well you could go out of his office thinking he agreed with you and then realize later that he didn't really say that after all." But, at the very least, the interested party has had a hearing and a memo to a client which begins with, "Today I had a chat with Chairman Mills . . . ," is an accomplishment. At election time, when he is confronted by the congressman with, "Remember that chat you had with Wilbur . . . ," he is likely to respond. In between these events, the chances are good that Mills has put his arm around the member's shoulder and without reference to past favors asked if he couldn't possibly see his way clear to doing this or that, thus closing the circle of exchanges which makes up a large part of congressional life.

At the other end of the continuum, Mills may commit himself to the inclusion of some member's proposal in a Ways and Means bill and expend a great deal of energy in getting it enacted into law. The members know that because of his influence in the Committee his backing or at least his benevolent neutrality are important for them to implement their ideas. Mills rewards the members every time he actively supports and makes his own one of their suggestions.

The Chairman's reward base is not confined solely to assisting members meet the substantive demands of their constituents and friends. As the occupant of the formal position of leadership on the Committee he can make decisions which determine how the Committee functions, discussed above as his legitimate power, but he can also use his formal powers as rewards for the members. Too much should not be made of the little favors the Chairman does for members, but they should not be dismissed as irrelevant either. As a Democrat said, with some exaggeration, "Members have bills they want passed, members they want to get on committees, provisions they want enacted, hearings they want held—these are the little intangibles that make him powerful."

Consider, for example, his control over such procedural matters as hearings and witnesses. It is not accidental that members commonly have the opportunity to deliver a flowery introduction for a prominent constituent. And, should a member get into a tight spot with a witness, Mills will often help extricate him. In both of these ways Mills utilizes the public hearings to improve his relations with the members.

In addition to the hearings Mills controls the terms of the House floor debate on the Committee's bills. Committee members have first call on debate time although there is usually sufficient time for all members who really want to speak on the floor to do so. If he wants to he can shift the spotlight from himself to other members by letting them handle time on the floor while he does other things.[36] He may even let a member's name appear on the Committee report and permit him to floor manage a bill, as Eugene Keogh (D., N.Y.) did on a bill he pushed for years, H. R. 10.[37]

[36] This happened to Dan Rostenkowski during the 1965 debate on excise taxes when he happened to be close to Mills when the Chairman wanted to leave the microphone. See *Congressional Record,* July 2, 1965 (daily edition), p. 11891. Rostenkowski was pleased.

[37] House, Committee on Ways and Means, *Contributions by Self-Employed Individuals to Pension Plans, etc.,* 89th Cong., 2d Sess., 1966, H. Rept, 1557 to accompany H. R. 10. *Congressional Record,* June 6, 1966 (daily edition), pp. 11672–82.

Another tactic the Chairman uses on the floor is the public acknowledgement of the great contribution some members of the Committee have made to the legislation being considered. This inducement, which Mills dispenses on a bipartisan basis,[38] is so much a part of the floor ceremony by now that it is doubtful if anyone gives it a second thought, but it is precisely the commonness of the practice that tells much about how Mills operates. It is, simply, something one expects from him. The point is, however, that the same expectation is not universally associated with committee chairmen.

A related example of the Chairman's reward power is that he will help the members defend their record on the Committee if an opponent tries to use it against him. A staff man recounted how some disparaging remarks he made about a member's attendance and participation in the Committee were used by the member's opponent. Mills wrote a letter testifying to the attacked member's service on the Committee and thereby helped blunt the force of the charge. By thus protecting the members from criticism he gains their favor (and, it might be noted, removes one more stimulus for them to attend the meetings).

Taken singly, none of the above favors is of great consequence. The members are right: no one but those concerned have an interest in little things such as members' bills. But without exaggerating the importance of favors and rewards to Mills's influence in the Committee it can be said that many members benefit from their relationship with him and that the members, especially Republicans, believe that they get more from Mills than they return. Mills does not have a large number of valuable rewards to offer the members in return for their help, but by making use of what he has he establishes positive relationships with most of the members. When he suggests a compromise in the Committee, when he requests some assistance in passing a bill in the House, when he needs a minor adjustment in the social security law to help a lady in Arkansas, when he wants a freshman from Arkansas appointed to the Appropriations Committee—all of which have happened —the response of the Committee members is likely to be favorable because of all the favors he has done for them. For as one member said regarding the committee assignment, once Mills let it be known he wanted it for his man "you might as well not even have a vote, no one is going to vote against him on this kind of thing. He doesn't ask that much. . . ."

4. *Reference.* A fourth source of influence for the Chairman, one which probably stems from the above three, is that most of the members of the Committee, if not all, identify with him. The members feel a sense of oneness with him, they like being closely associated with him, they care about their relationship with him, and they are influenced by him. The Chairman, by treating the members with respect, by being fair, by being knowledgeable in the subject matter, and by being receptive to their demands and needs, has ingratiated himself with them. Consequently, when a member disagrees with Mills he does it in such a way that the disagreement does not strain their relationship and, if possible, he prefers to agree with the Chairman more often than not.

The reference factor appeared in the interviews in different ways. One member, a Republican recently appointed to the Committee, commented that Mills treats the members so well—unlike some chairmen he has heard about —that the members feel drawn to him even though they may oppose him on certain issues. In his own case, he mentioned that one of the reasons he attends the lengthy Committee meetings is because of Mills: "I feel compelled to go because he's there and if he does it. . . ." He also observed that Democratic attendance is not too good, "But Mills is there, he's conscientious." A Democrat, when asked if anyone had given him any advice as a newcomer to the Committee, said yes, but perhaps more important, "Unconsciously you sit there, watch Mills, and you start following him—that's all." A few members were asked if they would tell a newcomer to the Committee to emulate anyone, and the answers were predictable:

> Mills. He's quite a guy and a new member should watch the Chairman closely and get to know him well.
>
> I'd tell them to emulate Byrnes and Curtis. They are real students and they have done their homework. Mills too. Wilbur is a real student. He's not arbitrary but he's got respect and he's effective.
>
> Mills. No one else.

Another member talked disparagingly about those members who take pains to get close to Mills, who praise him all the time, who want, in his imagery, to "get close to the throne." This

[38] In 1965 Mills lauded Joel Broyhill's contribution to the medicare bill. Broyhill, who was strongly criticized for leaving the District and Post Office committees for Ways and Means, might not have found it necessary to use Mills's statement in his campaign, but he could have if he wanted to. *Congressional Record,* April 7, 1965 (daily edition), p. 6959.

man does not do this because he finds it "repulsive." But in 1967 when Mills received a lot of publicity in connection with the tax surcharge issue the member visited his district and received an unusual amount of attention because of his membership on Ways and Means. He returned and told the Chairman, "Wilbur, you've made a great man out of me." That this member cares about his relationship with Mills was manifest in a story he told about what happened after he put something in the *Congressional Record* critical of a bill the Committee had reported. Mills "called me to task on this and I learned a lesson early." He assured the Chairman the material was not meant as a personal criticism of him and that, on the contrary, he had always tried to be one of Mill's "biggest public supporters." A similar kind of thing happened to a second member who, informed by a staff man that Mills thought something he had said was critical of the way the Committee was run, informed the go-between that the statement in question was directed at the Administration, not at the Chairman. It is not known if the word ever got back to Mills but the incident does show that this member, like his chastised colleague, is sensitive to his relationship with the Chairman.

The above evidence indicates that Mills has great influence with the members of the Committee. The members themselves see him as a master at influencing them and others. But if this is true it should be recognized that Mills is not a master puppeteer. He deals with men who have all kinds of interests and are subjected to all sorts of competing demands and it sometimes happens that these demands pull them in ways contrary to the Chairman's direction. To a large extent, his influence lies in his skill at aligning his demands on the members with the other demands made upon them. In this process, his aforementioned proclivity to compromise, to bring opposing sides together, and to negotiate settlements of disputes, are of prime significance.

Yet it is equally true that although nonconformity with Mills's decisions may turn out to be a necessity, once one has been drawn close to Mills it is an unpleasant necessity; if at all possible, the member is apt to agree with Mills and to behave in ways approved by Mills. This intricate process reaches its height when the member relieves his inner tension by explaining to Mills why he cannot go along with him and the Chairman, who probably has enough votes anyway, casually dismisses the incident as of no importance to their future, and firmly-rooted, relations. By so doing the Chairman maintains—indeed adds to—his base of influence with the members.

5. *Sanctions.* "You talk about a chairman who uses his power ruthlessly, you're not talking about Wilbur Mills." This comment, by a Democrat, illustrates what is by now abundantly clear: the influence of Mills in the legislative process is not based on the sanctions that he, as the Committee Chairman, could use. Sanctions are defined as actions designed to bring about results through non-physical coercion or force and hence, in contrast to rewards, generate negative affect on the part of other members of a group.[39]

The mark of Mills's leadership is that in doing the job as he defines it he relies on rewards, favors, expertise, persuasion, negotiation, and bargaining, not on coercing the members by using the sanctions that are available to him.

Unlike some committee chairmen, such as Graham Barden of North Carolina who chaired the Education and Labor Committee for eight years, Mills has not used his authority as chairman in such a way as to provoke extreme hostility—mixed with grudging respect—from the members. Mills's objective has been to get legislation out of his Committee with enough support behind it to pass in the House. Barden's objective was to kill and, failing that, delay as long as possible federal aid to education, and his behavior as chairman reflected his perception of the Education and Labor Committee's job.[40]

With fundamentally different objectives Mills and Barden typify fundamentally different styles of leadership in the House. To marshall a majority Mills allows members and witnesses to speak at length; to prevent a majority from coalescing Barden permitted and encouraged the filibuster. To mark up legislation Mills holds the Committee to a stiff regimen; to thwart legislation Barden called meetings irregularly and established ad hoc subcommittees which he controlled. To proceed with speed Mills actively seeks a quorum; to procrastinate Barden banged the gavel and adjourned when a quorum was not immediately present. To ease the burdens of formulating legislation Mills surrounds the Committee with expert help; to heighten these burdens Barden kept the staff weak. To promote a workmanlike atmosphere in the Committee Mill seeks consensus; to promote confu-

[39] See French and Raven's treatment of coercive power, *op. cit.*, pp. 614–615.

[40] The comments on Barden are based on Fenno's analysis of him, Richard F. Fenno, Jr., "The House of Representatives and Federal Aid to Education," in *New Perspectives on the House of Representatives*, eds. Robert L. Peabody and Nelson W. Polsby (Chicago: Rand McNally & Co., 1963), pp. 209–212.

sion Barden sowed dissensus. And, finally, to pass the bill Mills carefully maps the House terrain; to defeat the bill Barden allocated disproportionate time to opponents and once, near the end of the proceedings, threw the House into a frenzy by resigning as floor manager.

At one time or another Mills has used many of the tactics employed by Barden, but with basically different objectives. Extended public hearings have delayed legislation but in the meanwhile Mills has quietly sought a reconciliation of demands with the objective of passing some form of the legislation. Confronted by resistance to a compromise on federal unemployment compensation standards in 1966 he, on at least one occasion and to the chagrin of no one, adjourned the Committee for lack of a quorum with 12 members present whereas, if time were of the essence, the 13th member probably could have been found. Like Barden, Mills has relied on Republicans to defeat certain measures over the protest of a majority of Democrats on the Committee (e.g., medicare) but he has not done this so often that the members criticize him for being a Republican in Democratic disguise, as they did with Barden. And, perhaps of greatest significance, Mills has never brought his opposition to the House floor and attempted to defeat the bill by parliamentary legerdemain; as a matter of fact, bills he opposes do not come to the House floor.

Stark is the contrast between Barden and Mills, therefore, even when, objectively, their tactics seem the same. For Barden, federal aid to education was an issue on which he was willing to sink, and go down with, the ship; for Mills, there has never been a no-holds-barred issue on which he has risked everything. The only bill before the Ways and Means Committee which bears a strong resemblance to federal aid to education is medicare and even here Mills, characteristically, had informed Speaker McCormack and through him the White House that given enough time he would be able to work something out.[41]

Thus Mills, in seeking a consensus, relies on four of the five bases of influence: expertise, legitimacy, rewards and reference. In the Committee's deliberations he will, if possible, stay in the background by letting the active members, with some coaxing, hit upon the compromise through their own efforts but, since the Quaker-like sense of the meeting may evade them, he will often articulate it and when he does the Committee has made a "decision," always tentative depending upon further developments. The low amount of negative affect expressed toward the Chairman indicates that his style of leadership is approved of by the members, and their agreement on the way he influences them through four of the five bases of influence helps explain why, when he intervenes in the discussion, his suggestions often constitute a Committee decision.

[41] Theodore C. Sorensen, *Kennedy* (New York: Harper & Row, 1965), p. 343.

III. MILLS: THE SWING VOTE

When Mills persuades the members of the Committee to make certain decisions it may be on the basis of his expertise, or his influence may be based on the way he has handled interpersonal relations in the Committee, or it may be because the members are indifferent on the outcomes. But then again it may be none of these. It may be a simple matter of arithmetical influence: he, for all the reasons discussed above, has the votes. The Chairman's influence in the Committee is on some issues closely related to his position as the crucial swing vote between the coalitions that appear most often on Ways and Means issues. If the parties split cleanly Mills and the 14 Democrats beat the Republicans 15–10. If an issue divides the Committee along liberal-conservative lines Mills and 12 Democrats can beat the Republicans and two conservative Democrats 13–12; or, in some cases, Mills and two conservative Democrats plus the Republicans can defeat the 12 Democrats 13–12. Mills, as a moderate on policy questions, straddles the coalitions in Ways and Means and, because he does, it is a rare coalition that sees him on the losing side. For many years two conservative Democrats, A. Sydney Herlong of Florida (who left the Committee in 1968) and John Watts of Kentucky, played leading roles in the coalition make-up of the Committee. Both men were close to Mills and if the other Democrats on the Committee banded together into a 12-vote bloc they still lost 13–12 without the Chairman. With the Chairman, the 12 Democrats won by the same margin. The pivot was the Chairman.

Evidence for the nature of coalitions in the Committee comes both from interviews and votes which have occurred in the Committee. Since most issues are resolved without votes (a "consensus" is reached), partly because the shape of the blocs is relatively easy to predict, there are not many recorded votes to consider. But some have been collected and they substantiate, with exceptions, the results of the interviews.

An HEW official sees Mills in this way: "If he took an all-out Democratic or Administration line he could steamroller it through, but he's more conservative than the Administration, he

doesn't do this." One of his colleagues in the Department sees the importance of Mill's coalition position to his influence in the Committee even more clearly,

It's not only that he's prestigeful, its not only that he's chairman. It's that Mills stands at the center of the Committee and when he accepts something it doesn't get enacted just because he says 'poof,' but because he's at the center. He's generally more moderate than the Republicans and he's more conservative than the Democrats, so the Republicans know they can get just about what he accepts and same with the Democrats. If you sell Mills you've got it is both true and a truism. It's not just his personality, it's the nature of things in that Committee.

Liberal Democrats on the Committee are potentially a sizable bloc of votes but, with one or two exceptions, they are liberals who are unlikely to split off into a discordant minority and attempt to reverse Committee decisions on the House floor. They are aware of the political realities in the Committee but, given the style of Mills's leadership, they are not closed off from influence in the Committee, which is one more reason they do not usually attempt to overturn the Chairman on the floor. One liberal Democrat, a typical non-crusader with a 100 percent rating by the Americans for Democratic Action, understands the Chairman's "problem" in these terms: "Two or three of us on the Committee don't always get everything to our liking—liberals—but we understand his problem. I think the worst thing in the world for him would be to get defeated on a proposition on the floor. We understand his situation so it doesn't cause any problems—we can't win, we know it." He explained that if the Committee reported a bill by one vote the bill would have trouble passing the House so, although the Committee liberals may not be fully satisfied with the Mills consensus, they support it anyway. This Democrat's approach to liberal questions was well illustrated by a story he told about Aime Forand's unsuccessful efforts to get medicare reported by the Committee. According to his man, Forand got so upset with Mills that he planned to force the issue to a vote in the Committee even though everyone knew the Forand bill did not have a chance. This man, no less ardent in his desire for medicare than Forand, felt that Forand was unwise to press so hard for a lost cause.

Another liberal Democrat confirmed the futility of having votes in the Committee because of Mills's strategic position, but a Democrat who also classified himself as a liberal, and who feels that Ways and Means is "two-thirds conservative, probably always will be," feels that despite their minority status they can have an effect on the legislation under a chairman like Mills:

We liberals, though a small band, have an impact. I remember when aid to dependent children came up and they were going to cut it off at five children. I told them they had just declared war, it was an act of war in my district, and if they wanted a revolution they'd go ahead with it. I got so mad I walked out—but Mills said, 'Oh come on back, let's talk about it'—that's what he says. I killed that proposal. . . . I was ready to take it to the floor, and they knew I would too, the bastards. They changed it.

With the presence of some conservative Democrats on the Committee, and with the possibility of a stray Republican vote or two, poaching by Mills and Byrnes is a familiar part of the coalition-building process in Ways and Means. Both party groups have defectors except on the most basic party issues (e.g., debt bills, reporting medicare, major revenue bills). A Democrat noted, "The Republicans can reach right across and get Herlong and Watts anytime they want them. Mills is a swinger." It is not that easy for the Republicans (or for Mills) and the Democrats most likely to defect often stay with Mills, but the minority is aware of their potential, if success is less than complete. A Republican told how it works,

It's not the sort of thing you do formally. You don't sit down with them and say if we do this . . . it's not any kind of logrolling operation. But yes we have done this. You anticipate, sometimes you get it from how the hearings are going, and we may leave something out in hope of getting some of them. I thought we had Sydney [Herlong] on that provision you remember that tied the tax cut to some sort of expenditure control—he had introduced it. I talked with Sydney about it but Mills got to him and talked him out of it—even made a speech on the floor. Mills can do this sort of thing.

Forty record votes in the Committee have been collected since Mills became chairman in 1958,[42] and the results confirm the dominant position of the Chairman in the Committee. These votes are not comprehensive because more votes were taken in the Committee than were reported. It must be noted, too, that many important issues never come to a vote in the Commit-

[42] The forty votes include votes reported in the *Congressional Record, Congressional Quarterly,* an unpublished doctoral dissertation on the 1964 Revenue Act written by Everett Cataldo, the public press, and, in some cases, by private sources.

tee. For example, in 1964 Cecil King did not force medicare to a vote because he knew it would lose.

Of the 40 votes, the Chairman has been on the losing side only seven times: a 1959 bill dealing with interest rates, a 1961 bill regarding tax treatment of the self-employed, a 1962 vote on unemployment insurance, two votes during the consideration of the tax reduction bill in 1963, a social security vote in 1964, and a 1966 vote on providing federal standards for unemployment compensation programs. Based on available data, therefore, Mills's reputation as a winner in the Committee is deserved.

It is impossible to know whether Mills, had he made an all-out effort on the seven votes he lost in the Committee, could have won, but regardless of the circumstances surrounding these votes the fact remains that he was on the losing side. He can be and has been beaten in the Committee, more often than in the House, and, in the light of his reputation as an invincible chairman, a look at his losses is in order.

Votes in 1963, 1964, and 1966 are particularly revealing. In 1963 the Administration, as part of its tax cut and tax reform proposal, suggested that Congress repeal the four percent stock dividend credit and $100 exclusion. Under existing law, long subject to criticism by tax reformers, a person with dividend income from domestic corporations could exclude from his income the first $50 of dividends received and take four percent credit against dividend income over $50. This provision, worth $460 million a year in tax revenue, was put to vote in the Committee, and the Administration lost. By an 11–14 vote, with four Democrats joining the ten Republicans—and with Mills on the minority side both times—the Committee rejected outright repeal and a compromise plan to repeal the credit but keep the exclusion. Then, by a 13–12 vote, with only Herlong and Watts voting with the Republicans, Ways and Means adopted a compromise which repealed the four percent credit but doubled the $50 exclusion. Evidently Mills could not persuade enough Democrats to move very far toward the Administration proposal, but he could get 12 of them part of the way.

The next year Mills was on the losing side of a vote which some people believed would have killed medicare under social security. Mills, Herlong and Watts joined the Republicans on a motion to increase social security benefits by six percent, thereby raising social security taxes so high that it would have been difficult to fund medicare without an unacceptable increase in the tax. The Republican plan lost on a 12–12 vote when, with one Democrat absent, Bruce Alger voted with the pro-Administration majority because he could not vote for such an increase in benefits. Were it not for this anomaly, Mills's side would have won.

The most recent defeat for Mills came in 1966 on an extremely controversial issue, the establishment of Federal standards for minimum state payments for unemployment compensation. Backed by organized labor, a plan acceptable to the Administration was defeated in the Committee when five Democrats, four of them conservatives, voted with the Committee's eight Republicans (the ratio had been changed in 1965) to kill the plan 13–11. One Democrat was absent but Mills could hold only ten of his party colleagues on this issue. Thus even with a 17–8 majority in 1966 it was possible for the Chairman to lose on a key vote. After the vote the Committee proceeded to write a bill which could pass the House but, as Mills accurately predicted, "won't please either the White House or organized labor."[43]

Seven losses are not much to work with but it may be noted in passing that, as expected, if Mills is on the losing side it is most often because enough Democrats have defected to the Republicans to decide the issue. Five of the seven losses fall in this category. On the two remaining losses Mills did not take enough Democrats with him to win on one vote (1964 social security) and on the other he was hopelessly in the minority with four Republicans. On the basis of available data, therefore, it can be said that for the Ways and Means Committee the primary job of the Chairman on votes is to contain Democratic defections, not take Democrats with him to defeat the Democratic majority through a bipartisan coalition (this has happened, but only twice). It bears repeating, however, that the ability of Mills to defeat the Democrats by voting with the Republicans has acted as a deterrent to pressing issues to votes, and on the vital question of medicare it was such a coalition that kept the bill in the Committee for several years.

This analysis of voting in the Committee treats Mills as a swing vote, but vote patterns alone do not show a causal relationship between the way he votes and the votes of the other members. We are justified in treating his votes as of special significance to the Committee only because of the findings reported in the first two parts of this study, plus the perceptions of many observers that he does in fact stand at the center of coalitions in the Committee. From the

[43] Quoted in "Jobless Aid Plan Facing Reversal." *New York Times,* May 13, 1966, p. 21.

mere fact that two Democrats voted "present" on the Republican amendment in 1963 to cut taxes only if the budget was no more than $98 billion in FY 1965, and the debt no more than $303 billion, for example, we cannot conclude that Mills influenced them. It is obvious that such a conclusion cannot be inferred from the vote despite the additional fact that this key amendment lost by only one vote (11–12). But everything that is known about the inner workings of the Committee suggests this conclusion and, in the absence of contrary evidence, the leap from the votes to Mills's influence does not appear to be unwarranted. Many other influences are at work in these votes but the Chairman is most certainly one of the factors, and from the interviews conducted, he is one of the most significant.

IV. CONCLUSION

An intensive look has been taken at the Chairman of Ways and Means because, in the eyes of the members and in the eyes of those associated with the Committee, he is the central figure around whom much of the Committee's life revolves. Contrary to the impression one sometimes receives from newspaper stories about the "all-powerful" Chairman Mills, he is perhaps as responsive to the Committee as the Committee is to him. Great influence with the members he no doubt has, but it is influence earned by the way he approaches his job and develops its potential. Ways and Means is highly centralized under Mills but the Committee's policy decisions emerge from an exhaustive —and collegial—process. The decisions of the Committee are shaped and articulated by Mills, but if his word comes close to being law in the Committee it is because he has listened well to the words of others, particularly to the ranking Republican, John Byrnes. The fact that Mills legitimates Committee decisions in no way lessens his leadership of the Committee, but it is a particular kind of leadership, a kind very likely to be effective given the nature of the group he leads.

Mills's influence in the Committee is also attributable to his pivotal position in the blocs that form on policy questions in the Committee. The Chairman, known as a leader who keeps his position, if he has one, to himself, has good reason to be cautious on the controversies surrounding Ways and Means. By letting others commit themselves before he does he follows a winning strategy which has paid rich dividends: he never loses, except once in a while. And because he leads the members as he does he can sometimes persuade enough of them to help him win by voting present, by voting with him, or by not voting at all.

It remains to be seen whether the analytical framework used in studying the leadership of Mills, the variables which are important in understanding him, and the five bases of influence discussed in connection with his influence will be useful in future research on leadership in Congress. Yet it appears clear that an understanding of the legislative process—including the outputs of that process—requires attention to individual leaders, their followers, and how the two interact. This study is intended as a step in that direction.

CITIZEN DEMANDS AND THE SOVIET POLITICAL SYSTEM*

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Political scientists interested in non-Communist systems have paid considerable attention to demands (expressions of opinion that an authoritative allocation with regard to particular subject matters should or should not be made by those responsible for doing so)[1] coming from the intra-societal (domestic) environments of these political systems. The importance of intra-societal demands, including citizen demands, for non-Communist systems is well established. Researchers interested in the Soviet political system have paid relatively little attention to intra-societal demands, especially demands coming from those whom David Easton would call citizen gatekeepers, i.e., citizens who convert their wants into demands by articulating them.[2]

The reasons for the neglect of research in this area are obvious enough. Quite apart from the problem of gathering useful data, there exists the question of whether demands from the intra-societal environment, and in particular citizen demands, are really important for a "totalitarian" system. Nothing like the politically autonomous interest groups of the Western democracies exist in the Soviet Union. Whatever demands come from the intra-societal environment are therefore largely grassroots demands from the populace, and there is reason to doubt that Soviet authorities feel compelled to heed such demands when formulating policy. Lenin's assertion that the Party is the vanguard of the proletariat was clearly a rejection of the idea that the masses should direct the Party. His successors have continued to assert that the Party leads the masses, and not the masses the Party. Stalin argued:

> The Party cannot be a real party if it limits itself to registering what the masses of the working class feel and think. . . . The Party must stand at the head of the working class; it must see farther than the working class; it must lead the proletariat, and not follow in the tail of the spontaneous movement.[3]

The 1961 Party Program clearly reasserted the doctrine of the primacy of the Party as the guiding force in society.

> The period of full-scale communist construction is characterized by a *further enhancement of the role and importance of the Communist Party* as the leading and guiding force of Soviet society.[4]

The directing element of the Party is much narrower than its total membership. Although Western scholars' estimates vary from under 1,000 to several thousand, they agree that the number of persons who really count in the formulation of policy in the Soviet Union is small.[5] Furthermore, Soviet leaders have promoted not merely programs that were clearly contrary to the wishes of important segments of the general populace—collectivization—but also programs contrary to the wishes of segments of the elite —Khrushchev's bifurcation of the Party.

I. SOME HYPOTHESES CONCERNING DEMANDS OF SOVIET CITIZENS

The existence of an apparently limited political community, the lack of independent associational interest groups, the leadership's apparent

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[1] I will use the term "demand" only in this restricted sense. For a full discussion of the concept see David Easton, *A Systems Analysis of Political Life* (New York: John Wiley & Sons, Inc., 1965), Part II.

[2] For a discussion of citizen gatekeepers see Easton, *op. cit.*, pp. 93–94.

[3] J. V. Stalin, "Foundations of Leninism," *Problems of Leninism* (New York: International Publishers 1928), p. 73, as quoted in Merle Fainsod, *How Russia is Ruled* (Cambridge: Harvard University Press, 1963), p. 137.

[4] Program of the Communist Party of the Soviet Union, reproduced from a supplement of *New Times* (No. 48), 29 November, 1961, in Leonard Schapiro (ed.), *The U.S.S.R. and the Future* (New York: Frederick A Praeger, 1963), p. 310. Emphasis in the original.

[5] Compare Merle Fainsod, *op. cit.*, p. 205; Derek J. R. Scott, *Russian Political Institutions* (New York: Frederick A. Praeger, 1961), p. 54; and Wolfgang Leonhard, *The Kremlin Since Stalin*, trans. Elizabeth Wiskemann and Marion Jackson (New York: Frederick A. Praeger, 1962), pp. 11–15. See also John A. Armstrong, *The Soviet Bureaucratic Elite* (New York: Frederick A. Praeger, 1959), Chapters I and II.

contempt for mass opinion, the marked centralization of decision-making all suggest that demands from what Easton calls the intra-societal environment are far less important in the Soviet system than in the Western democracies, and even in many of the non-democratic but non-Communist states.

Certainly much of the work of Soviet-area specialists supports Easton's contention that although "there is still room for articulation of political demands" within totalitarian systems despite "the severe restrictions upon popular participation,"[6] the number and variety of gatekeepers—those who convert wants into demands by articulating them—is so restricted that input overload is not "likely to occur or even threaten."[7] He contrasts "modern totalitarian or dictatorial systems" with democracies, which, "within the limits imposed by other aspects of the political culture," encourage every citizen to participate in the system by tending his own gate, that is, by converting his own wants into demands by directing statements concerning the allocation of values to authorities.[8]

The hypotheses that in the Soviet Union (1) citizens are not encouraged to tend their own gates; (2) the number of gatekeepers is so restricted that input overload is unlikely to occur or even threaten; (3) demands from the intra-societal environment, and in particular citizen demands, are of little importance, seem tenable. I will try to show that these hypotheses, if not false, are true only in a very limited sense. In the course of my argument I also will attempt to clarify the way in which these demands are processed, and indicate some of the problems that arise in conjunction with their processing.

II. SOVIET CITIZEN GATEKEEPERS AND THEIR DEMANDS

Evidence that Soviet citizens tend their own gates and that higher officials require local officials to pay attention to the resulting citizen demands has existed ever since Merle Fainsod published his study of the Smolensk Province Party Archives in 1958.[9] In his chapter on the right of petition he noted that this right existed prior to World War II and that the citizens of Smolensk Province vigorously exercised it.[10] Fainsod also noted that citizen petitions had at least two functional consequences. First, the petitions (demands in Easton's terminology) served to expose and, therefore, inhibit misconduct at the lower administrative levels. Second, they tended to diffuse popular discontent and direct it from the center to local officials.[11]

The inaccessibility of Party archives, other than the captured Smolensk Archives, makes a replication of Fainsod's study impossible. Nevertheless, data available for Moscow and Leningrad clearly show that Soviet urban citizens in the 1950's and 1960's continued to make their demands known to local authorities, just as their rural counterparts had done during the pre-war period in Smolensk Province. By the 1960's thousands of citizen demands were pouring into the city raion (borough)[12] agencies and organs, either directly from the citizens or via higher officials who simply sent any demands within the jurisdiction of a raion to that raion in the individual raw form in which the citizen had submitted it.

Whatever the route of the demands, the

[6] Easton, *op. cit.*, p. 110.

[7] *Ibid.*, p. 93

[8] *Ibid,* pp. 93–95.

[9] These archives were captured first by the German army during World War II and subsequently by U.S. forces. They are the only party archives open to non-Communist scholars.

[10] Merle Fainsod, *Smolensk Under Soviet Rule* (New York: Vintage Books, 1963), Chapter XX.

[11] *Ibid.*, p. 408.

[12] Moscow and Leningrad are divided into a number of raions (city boroughs). Each raion has its "popularly" elected legislative assembly (soviet); executive committee (borough council), which is "elected" by the soviet; and numerous governmental administrative agencies. According to the legal theory of dual subordination, the raion soviet is subordinate to its electors and the "popularly" elected city soviet. The raion city executive committee is subordinate to its soviet and the city executive committee, and the raion administrative agencies are subordinate to their executive committee and the corresponding city administrative agency. Similar dual subordination exists for city organs and agencies, the next higher level for Moscow and Leningrad being the RSFSR.

In each raion there is a raion party organization consisting of the large raion party committee (raikom), which is "elected" by the larger raion party conference; the bureau of the raikom, which is "elected" by the raikom; and the party administrative agencies. The raion party organizations are subordinate to the city party organization, which also have a conference, city party committee (gorkom), bureau, and administrative agencies.

At each level the governmental organs and agencies are subordinate to their appropriate party organs and agencies. Candidates to governmental offices are selected, or at least approved, by the appropriate party organizations and by higher state organs and agencies. Party officials must be approved by higher party organizations.

number was astounding. In the first four months of 1962 some 11,803 citizen demands poured into the offices of the Kirov raion of Moscow.[13] In the first half of 1963 officials of a single raion in Leningrad received over 15,000 letters and visits from the populace involving demands of various sorts.[14]

The demands themselves cover nearly the entire range of activities under the jurisdiction of the city governmental apparatus. The citizens complain about the quality of new construction; request additional housing space; and complain about the slowness of housing repairs and their quality. They make demands concerning the quality, assortment and availability of goods in stores and shops and the location of retail outlets and restaurants. They complain about the quality of service in retail outlets, restaurants, and consumer service facilities. They complain about taxi service; they make demands concerning mass transportation facilities. They express their concern about health facilities, about poor street lighting, about unkept parks, about cultural facilities, and about the condition of streets and sidewalks. They complain about the behavior of officials, and they complain about the activities of their neighbors. The regime that has undertaken, with great pride and deliberate purpose, more activities than any in modern history is confronted with demands covering a wider range of subjects than any regime in history.

The vast majority of citizen demands, whatever their topic, have certain common characteristics. They are specific demands from individual citizens involving limited and usually individual or neighborhood needs and wants. They are, in other words, raw demands. Because large autonomous interest groups, which exist in other modern societies are not permitted in the Soviet Union, these raw demands must be processed within the political system without the benefit of any prior efforts to sort, combine and consolidate them into general proposals for political action, but more about this later.

Most of the demands examined in this study are also apparently spontaneous and processed within the governmental apparatus. This obviously is not true of all demands. Fainsod's Smolensk study revealed that during the 1920's and 1930's some processing of citizen demands also went on within the party apparatus. No doubt it still does, although the inaccessibility of party records makes impossible a detailed discussion of party processing for any later period. I can, however, state that the Party does play a considerable role in the processing of demands included within the electoral mandates, which are lists of demands that are binding, at least formally, on the local governmental officials to whom they are addressed. Furthermore, these demands often are not spontaneous.

To be included within an electoral mandate, a demand must be presented in a particular manner and meet certain criteria. It must be presented in a meeting called either to nominate deputies to the soviets or to present candidates for deputy seats to the electorate; it must be discussed by those present at the meeting with respect to its expediency and practicality; it must be adopted by a majority vote; it should involve matters of common concern to the electors; and it should be substantial enough to require inclusion in the plan for fulfillment.[15]

At the proper meeting a citizen may offer a demand for inclusion in the mandate, but frequently demands offered have been discussed first in the organs of public and voluntary associations.[16] These organs, of course, are dominated by party members. Furthermore, subjecting the proposed demands to public discussion may eliminate some of them. Discussions at public meetings are often rather open, vigorous and frank; but whether any resolution opposed by the party group present at such meetings stands any chance of being adopted is doubtful. The influence of the Party on the mandate has been clearly indicated by two Soviet jurists who state that the Central Committee considers the mandate an important means by which the

[13] Decision of the Moscow Executive Committee of 29 June 1962 in *Biulleten' ispolnitel'nogo komiteta Moskovskogo gorodskogo Soveta deputatov trudiashchikhsia (Bulletin of the Executive Committee of the Moscow City Soviet of Working People's Deputies)*, No. 15 (August, 1962), p. 9. Hereafter this publication will be referred to as *M.B.*

[14] Decision of the Leningrad Executive Committee of 7 August 1963 in *Biulleten' isopolnitel'nogo komiteta Leningradskogo gorodskogo Soveta deputatov trudiaschikhsia (Bulletin of the Executive Committee of the Leningrad City Soviet of Working People's Deputies)*, No. 18 (September, 1963), p. 1. Hereafter this publication will be referred to as *L.B.*

[15] G. V. Barabashev and K. F. Sheremet, *Sovetskoe Stroitel'stvo* (Moscow: Iuridicheskaia literatura, 1965), pp. 374–375. See also I. A. Azovkin, R. A. Saforov and Iu A. Tikhomirov, "Deiatel'nost' deputatov mestnykh Sovetov v izribatel'nom okruge i na proizvodstve,' *Mestnye Sovety na sovremennom etape* (Moscow: Nauka, 1965), pp. 285–287.

[16] Barabashev and Sheremet, *op. cit.*, pp. 375–376.

Party sets tasks for the organs of state authority.[17]

III. FAILURES IN HANDLING CITIZEN DEMANDS

The thousands of raw demands pouring into the system create serious problems for local officials, especially the raion officials. They are in close contact with the populace and receive the major portion of citizen demands. The failure of hard-pressed raion officials to respond to many of the demands encourages citizens to route them through higher authorities in the hope that demands so routed have a better chance of attracting the attention of lower officials. Even so, a response is by no means certain. Between 1958 and 1960, 40 per cent of the demands received by officials of the Sokol'nicheskyi raion in Moscow had been sent via higher authorities, many of them, according to the city executive committee, for the second time. Of these 50 per cent had not been disposed of within the required 20-day period.[18]

The failure of officials to fulfill citizen demands extends even to those legally binding demands included in the electoral mandate. Although their own record is not flawless, city officials often criticize raion officials for failures in this area. Many things go wrong. Sometimes officials simply ignore certain items in mandate, failing even to discuss them. Sometimes officials sacrifice items in a mandate in order to fulfill more important items in the plan. The plan has its priorities; and whatever standing mandate items may have in law, many of them do not enjoy much priority in the plan. Sometimes local executive committees fail to set a specific date for the fulfillment of some mandate demands, thereby making control over fulfillment difficult. Other times the executive committees find themselves without the resources needed to fulfill a mandate.[19]

Although the decrees of the raion soviets and city executive committees clearly show that local officials, especially raion officials, have trouble coping with the flow of citizen demands, little or no effort is made to stem the flow. On the contrary, evidence indicates that higher officials may view large number of demands flowing into the raions with enthusiasm. In 1962 the Moscow city executive stated it considered a large number of citizen demands on raion officials as desirable insofar as the demands indicated increased citizen activism. In the same decree the executive committee also stated it viewed large numbers of such demands as evidence of the raion officials' neglect of citizen needs.[20] Apparently, the city officials view a high volume of demands as a negative indicator for the performance of raion officials and a positive indicator for the existence of citizen support. This makes some sense. Certainly, an unusually high level of demands in one raion might indicate poor performance by raion officials, but at the same time indicate that citizens have confidence in the system's ability to satisfy their needs and wants.

Within the past decade the difficulties of the raion officials may have been aggravated by the demand of higher authorities that the raion officials adopt practices likely to increase both their work load and the flow of citizen demands. For example, city officials urged raion executive committees to hold "circuit" meetings at places where citizens work or live in order to facilitate citizen participation in the discussion of raion problems. They also urged raion officials to send out advance publicity on sessions of the raion soviets in order to give the citizens an opportunity to send in complaints and suggestions concerning questions on the agenda. The raion officials were supposed to consider these for inclusion in the draft resolutions submitted for adoption by the raion soviet. Authorities also ordered raion executive committee officers and raion agency heads to make periodic reports at public meetings and to use the meetings as opportunities to gather additional citizen demands.

The city officials and higher authorities claimed these practices brought officials into closer contact with the masses and improved their understanding of the masses' needs. The reluctance of some raion officials to adopt such practices suggests that they were not unaware that adoption would increase their work loads at a time when they were already having problems handling citizen demands.

IV. THE PERSISTENCE OF THE THREAT OF INPUT OVERLOAD

The city and raion officials have only a limited ability to reduce the threat of input overload by correcting the conditions that give rise to the deluge of citizen demands. If the demands

[17] *Ibid.*, p. 374.

[18] Decision of the Moscow Executive Committee of 5 February 1960 in *M.B.*, No. 6 (March, 1960), p. 10.

[19] For a discussion of some of the difficulties that arise concerning the fulfillment of the electoral mandate see E. Sovershaeva (Senior instructor of the Leningrad Organization-Instructor Department) "O rabote po vypolneniiu nakazov izbiratelei," in *L.B.*, No. 12 (June, 1963), pp. 13–15.

[20] Decision of the Moscow Executive Committee of 29 June 1962 in *M.B.*, No. 15 (August, 1962), p. 9.

concern such limited problems as a specific street in poor repair, a dirty park, poor street lighting in a certain neighborhood, etc., the local officials can often effectively reduce the input load by correcting the conditions that give rise to the demands. Such corrective action ordinarily involves only the proper allocation and distribution of available resources, and this is within the capabilities of local officials.

When demands arise from conditions created by centrally determined investment and allocation priorities or certain basic characteristics of the economy, local officials can do little. For example, city officials can do something about demands concerning the poor quality of food in restaurants insofar as its quality depends on preparation and handling. They can satisfy the demands of citizens in a particular neighborhood for a restaurant in that neighborhood by using some of the limited resources available for such purposes to build a restaurant in that part of the city. However, they can do nothing to reduce effectively the flow of demands resulting from the general lack of restaurants or other service facilities because this lack results from investment decisions falling outside their jurisdiction.

In such cases city officials can fight for their share of a small pie; they can lend what support they can to whatever reforms are being discussed at higher levels; they can reassure the citizen that they are aware of the problems and are trying to do something to ameliorate the general conditions; and in individual cases they can provide some material relief. But the officials cannot change the basic conditions; consequently, they can do little that might reduce the input of demands resulting from those conditions.

As a result, many of the demands voiced by citizens in 1968 were the same as the demands voiced years earlier. This is occasionally revealed in local decisions. Thus, in 1963 the Leningrad soviet noted improvement in the work of service enterprises between 1958 and 1963, and pointed with considerable pride to expanded facilities. In the same decision the soviet noted that complaints from citizens had also increased. The reason is to be found in the growth figures reported in the decision. The per capita volume of services had increased by only 1.3 per cent over the reported period.[21]

V. DISAPPROVED RESPONSES

Confronted with a difficult situation, the local officials resort to various disapproved tactics in an effort to conceal their failures in handling the deluge of citizen demands or to reduce it. Some simply falsify their reports by untruthfully claiming to handle properly a substantial proportion of received demands. Some try to reduce the flow by curtailing reception hours for citizens wanting to make oral demands. Other officials bury the demands by sending complaints for verification to those very persons against whom the citizens lodged them. Still others simply ignore citizen demands. These tactics provide some temporary relief, but at the risk of censure from higher authorities.

The citizens' perception of the inability of local officials to meet many citizen demands causes citizens to turn to disapproved devices for the satisfaction of their wants and needs. This fosters the rise of various small "businessmen," whose activities are both illegal and ideologically objectionable. Whereas demands addressed to authorities provide the regime with information on popular attitudes and official performance and with an opportunity to build support, the disapproved devices provide neither. They do, however, provide satisfaction for the citizen.

The disapproved devices may also serve some of the needs of local officials. No way exists to discover the motives behind the local officials' toleration of disapproved devices, but that they tolerate them is beyond doubt. The toleration may be indicative of actual corruption or simply a desire not to harass unnecessarily local citizens. Local officials also may tolerate formally disapproved devices because they see them as reducing their input load by providing an alternative way of satisfying citizen needs and wants.

Disapproved devices take various forms, some of which are serious from the regime's standpoint. There is the hardly concealed black market in currency and clothing illegally but rather openly purchased from foreign tourists. There is even a black market operation in public housing. A "businessman" may turn a profit by registering, quite properly, for new housing on the grounds that his current housing space is inadequate. When he receives new housing, he

[21] Decision of the Leningrad City Soviet of 24 June 1963 in *L.B.*, No. 14 (July, 1963), pp. 1–2. As a result of an increase of 4.3 times in dry cleaning capacity between 1959 and 1961, the people of Leningrad could have their dry cleaning orders filled within twenty days in 1961. This was a considerable improvement over the sixty day period of 1959. However, according to the decision of the city soviet, demands in this area were still not being met. See the decision of the Leningrad City Soviet of 11 December 1961 in *L.B.*, No. 1 (January, 1962), p. 4.

then exchanges it with another citizen for less desirable housing plus an additional material consideration. Again lacking adequate housing space, he places his name back on the list for new housing. Other devices are less ambitious, but no less illegal. Both private automobile owners and drivers of state owned trucks provide taxi service. Other citizens illegally make and sell consumer items or offer services for a fee.

The local official tolerating any of these activities runs a risk of being reported. Rather than use these devices, some citizens report to higher authorities both the person engaging in an illegal activity and the local officials tolerating it. Some of the reports may result from personal animosities; but the regime's considerable efforts to build socialist consciousness must have some effect, and some of the reports are surely motivated by a genuine sense of civic duty. Nevertheless, local officials sometimes ignore citizen complaints concerning illegal activities until investigations by higher authorities force them to act.[22]

Although the use of disapproved devices by citizens and the collusion of local officials pose serious problems for those seeking to impose the official norms, these problems should not be permitted to obscure the possible, if unintentional, beneficial consequences for the regime. If the regime successfully suppressed these practices, which so clearly violate officially prescribed norms, and if, as a result, citizens and local officials could no longer resort to them to satisfy their felt needs and wants, the level of popular dissatisfaction with the political leadership, its policies and the institutional structure it has established might sharply increase.

Propaganda and coercion may contribute to mass conformity to officially prescribed norms. However, the regime's willingness to tolerate (within limits) citizen gatekeepers and its inability to close off all officially disapproved means by which citizens can obtain satisfaction of needs and wants the regime is unable or unwilling to satisfy, may, by making life more tolerable, be no less important for the maintenance of popular acceptance or acquiescence in the system. Ironically, the use of disapproved devices by citizens, may, by reducing dissatisfaction, make less likely any challenge of what the ruling oligarchy regards as a legitimate right: namely, the right to impose on the people policies that the oligarchy allegedly believes to be in the best interest of the people, even when the people disagree.

[22] See decision of the Moscow Executive Committee of 18 December 1962 in *M.B.*, No. 2 (January, 1963), pp. 19–21.

VI. APPROVED RESPONSES

There are approved means for reducing the demand input load. City officials may properly refer many of the demands they receive to raion officials for final action; but the raion officials are not so fortunate. They can refer some to public or voluntary associations such as the trade unions, the parents' committees attached to schools, apartment house committees, shoppers' councils, etc. These organizations may also directly absorb a certain number of demands. The trade unions devote some attention to working conditions and safety standards at places of employment. Parents' committees provide some material aid to children of needy families.

However, the ability of these organizations to handle or absorb demands is not very great. In fact, the small local voluntary associations—the parents' committees, apartment house committees, shoppers' councils, etc.—are likely to add to the flow of demands on raion officials. These groups are valuable primarily because they involve citizens in civic work, which helps build civic pride and support, and because they provide free labor for small civic projects such as clean-up and shrubbery planting campaigns. To a considerable degree, the effectiveness of the volunteer organizations depends on the amount of organizational aid the local officials are willing to give them. Therefore, the organizations are as likely to increase the work load of local officials as they are to reduce it. Some volunteer groups have become moribund for the lack of such aid, which suggests that some local officials may find the groups require more time and work than they are worth.

Although higher officials look with disfavor on the enlargement of paid raion staffs and forbid, without perfect success,[23] any unauthorized increases, they have encouraged local agencies to organize volunteer staffs. In 1961 the chairman of the executive committee of Moscow's Leningrad raion reported positively on the use of volunteers. His report also indicated the severe shortage of paid staff members at the raion level.

He reported that in 1948 the raion Cultural Department's staff consisted of the head of the department and one inspector for libraries. At the time it had under its jurisdiction one movie theater, two clubs and two libraries. By 1961 it had under its jurisdiction fifteen libraries, twelve clubs, two houses of culture, sixty-five red corners, ten movie theaters, four music schools

[23] Decision of Moscow Executive Committee of 7 January 1964 in *M.B.*, No. 2 (January, 1964), pp. 13–14.

and six book stores. Its staff still consisted of the head of the department and one inspector for libraries. According to the chairman the work of the department improved greatly with the help of a twenty-nine member voluntary group composed of pensioners, housewives and workers from the various raion cultural institutions.[24] The executive committee chairman reported similar benefits from the use of volunteers in other departments. In the raion Department for the Allocation of Housing Space, an area of some sensitivity, he reported that the number of citizen demands answered in a period longer than twenty days declined from 750 in 1958, when there were no volunteers, to around a dozen in 1960. The volume of demands pouring into this one department was enormous, numbering some 14,000 in 1959.[25]

Even though this was probably an exceptional success story, it does suggest the magnitude of the problem that confronts the raion officials. Volunteers would seem to promise some relief, and participation may provide the additional bonus of increased support. How much relief volunteers provide is a question that cannot be answered, but the following points should be noted in addition to the above report.

First, a number of raions were slow to adopt the practice of using volunteers, despite criticism from above concerning the quality of their work in handling citizen demands, and despite prodding from above to seek a remedy through the use of volunteers. This refusal to use volunteers simply may indicate organizational inertia. It also may indicate that the creation of effective volunteer staffs required a considerable amount of work by already busy paid staff members, and that all such experiments were not as successful as the one in the Leningrad raion. Furthermore, the Leningrad city executive committee warned that the use of volunteer staffs to check on the fulfillment of decisions did not relieve raion officials of responsibility for the quality of work in their raions,[26] thereby suggesting that the quality of volunteer work was not always high and that raion officials tried to hide behind volunteers in an effort to evade responsibility for shortcomings. Finally, I was told that by 1965 there was some problem in maintaining public enthusiasm for volunteer work, some of which was not voluntary in the strictest sense of the word. Pseudo-volunteer participation is unlikely either to provide efficient help or build support.

In addition to the use of volunteers, city officials have urged the raion officials to reduce the red tape and paper shuffling that so often accompanies the handling of citizen demands. The available data indicate that many raion officials quite needlessly add to their own burdens. In February of 1963 the Moscow city executive committee noted that despite a 1960 decision ordering raion officials to cease demanding numerous unnecessary forms, certificates, and notarized documents from citizens before undertaking such routine administrative matters as changes in residence, school registration and the issuance of passports, the practice continued, especially in Kuybyshev, Sverdlov and Timiriazev raions. The demand for notarized documents was so great that in a 21-month period one notary office in one of the raions notarized over 625,000 documents. The housing exploitation office of the Sverdlov raion had demanded and received in a single year over 75,000 certificates of various sorts from the citizenry. In the same raion a citizen wishing to acquire better housing had to present not only various forms, certificates and documents, including a personal financial report, but also fill out a seven-page questionnaire containing nearly 200 questions.[27] In view of the small size of the raion staffs such figures are astounding. The executive committee decision gives no indication of why the raion officials persisted in the use of procedures that increased their own work load despite contrary orders from higher authorities. Such procedures apparently had been standard at an earlier date. Perhaps some raion officials believed a large amount of documentation provided them with a ready defense against charges from above that they were giving housing or other material goods or services to unqualified applicants.

VII. THE USE OF DEPUTIES

Higher officials have encouraged raion officials to work with the deputies "popularly elected" to the soviets and to weld them into an effective work force. This also may increase the work load of raion officials. The effective use of deputies requires considerable organizational work and other aid from the raion executive

[24] A. Ia. Goncharov, "O praktike vovlecheniia obshchestvennosti v rabotu Leningradskogo raionnogo Soveta g. Moskvy," *Ot sotsialisticheskoi gosudarstvennosti k kommunisticheskomu samoupravleniiu*, pp. 298–299.

[25] *Ibid.*, p. 295.

[26] See unsigned article, "O rabote obshchestvennykh otdelov po konotroliu ispolneniia reshenii," in *L.B.*, No. 10 (May, 1962), p. 13.

[27] Decision of the Moscow Executive Committee of 21 February 1963 in *M.B.*, No. 4 (February 1963), pp. 23–24. See also decision of Leningrad Executive Committee of 25 March 1960 in *L.B.*, No. 9 (May, 1960), pp. 3–6.

committees and their administrative agencies.[28] Furthermore, one of the major jobs of the local deputy is to transmit his constituents' demands to the local officials. Therefore, an effective deputy can add significantly to the total input of citizen demands.

On the other hand, the deputy can also relieve the local officials of some of their burdens by undertaking the initial investigation of demands, and the verification of the fulfillment of official decisions concerning them. Deputies also can organize the citizenry to work for the satisfaction of their own demands on a self-help basis. This activity is very helpful to local officials when they find themselves in the awkward position of not having the resources to meet citizen demands, especially the legally binding ones included in the electoral mandate. For example, deputies in one raion, faced with the prospect of having to tell their constituents that the raion had no resources to build a shop that had been included in the mandate, organized the citizens, issued appeals to enterprises for material aid, and led the citizens in the construction of the shop.[29]

The deputy does not confine his activities to the electoral mandate. He is expected to hold office hours in public places for the collection of demands from his constituents. He must periodically report to them on his work and hear their demands at public meetings. He also receives numerous letters from constituents. The citizen demands may involve matters of concern to the entire district, such as those pertaining to the quality of food in local restaurants, the need for new shops, or the lack of adequate cultural and recreational facilities. Other demands may be highly personal and relate to such matters as the enrollment of children in special schools, or disputes with neighbors, or even trouble within the family. No problem is supposed to be too personal or too small to take to the deputy.[30] He is supposed to take the same interest in the personal affairs of his constituents as the old ward politician did in American cities. The reason is probably the same in both cases. Personal attention builds popularity and support for the administration. Once the deputy receives a demand, he should investigate it, present it to local authorities if it has merit, and make certain that the citizen gets some sort of response from the officials, even if only a verbal one.

VIII. DESIRABLE AND UNDESIRABLE CITIZEN DEMANDS

The demands discussed up to this point have been ones that the authorities, at least the higher authorities, regard as desirable and think should be given some measure of gratification, either material or psychic. The authorities undoubtedly regard many of them as legitimate and routine claims for goods or services—housing, housing repairs, pensions, etc.—to which the claimants are officially entitled. Authorities may find other demands desirable because they provide feedback on regime policies, reveal popular attitudes, or provide information on the performance of lower level functionaries. The authorities may value some merely as acceptable means for people to vent their frustrations.

Certainly, local officials who try to suppress or conceal demands of this sort run the risk of censure from above. The higher authorities' obvious desire that desirable demands receive some type of satisfaction is understandable. By satisfying the demands the authorities build support and cultivate in the minds of the citizens the belief that their demands are efficacious. This sense of efficacy itself may lead to greater support, and it almost certainly encourages citizens to submit additional desirable demands. This provides additional opportunities to build support and assures the continued flow of needed information to the authorities. The very fact that the citizens use official channels, rather than the disapproved but available means, to satisfy their needs and wants is itself an indication of support.

Undoubtedly, the authorities do not regard as desirable all citizen demands flowing into the system. The sources used in this study contain no information on whatever directives may exist concerning the handling of undesirable citizen demands. Consequently, any discussion of undesirable demands must be speculative.

Soviet citizens are more or less aware of the extent to which the regime is willing to act at a given time to meet certain kinds of demands. Whatever shortcomings the Soviet mass media may have as general news sources, they quite carefully provide information concerning official policy lines on the various subjects for which established policy lines exist. Research on Western political systems has shown that political participation is related to a sense of efficacy. No way

[28] See decision of the Leningrad Executive Committee of 4 June 1956 in *Sbornik reshenii i rasporiazhenii ispolnitelnogo komiteta Leningradskogo gorodskogo Soveta* (Leningrad, 1958), pp. 5–10. See also decision of Moscow Executive Committee of 12 October in *M.B.*, No. 20 (October, 1965), pp. 1–5.

[29] A. Zhuravlev (Chairman of the Petroslaviansk Settlement Executive Committee), "Metodom narodnoi stroiki" in *L.B.*, No. 16 (August, 1963), pp. 21–22.

[30] *Vechernii Leningrad,* 26 November, 1964, p. 1.

exists to verify this hypothesis in the Soviet Union; but it seems reasonable to assume that there, too, active participation such as stating demands will vary according to the citizen's perception of the efficacy of his actions.

The authorities may try to discourage some demands by simply ignoring them, but the Soviet press provides clear evidence that the authorities also actively try to discourage certain demands. A mild form of negative response is the article reporting that certain nameless individuals hold an unacceptable view. Other articles will single out particular individuals. Sometimes the press will publish a letter (selected from among the thousands the Soviet press receives daily) containing a disapproved demand and follow it with critical commentary. Mass media content of this sort must inhibit persons considering making similar demands.

In his study of the Smolensk Archives, Fainsod noted that citizen petitions expressing deep hostility to the regime were simply referred to the police for action. As a result, by the mid-thirties, such petitions nearly disappeared from the archives.[31] The sources used in this study do not enable me to determine whether such petitions existed in the 1950's and 1960's, but I can state that the Soviet citizen knows that the careless expression of certain opinions may arouse police interest. This is not to say that the citizen never expresses such opinions; but he does so cautiously, being selective about to whom and where he expresses them.

The norms governing acceptable demand expression, although not nearly as restrictive and inhibiting as some in the West seem to think, effectively restrict the citizen's expression of demands. His awareness of these limitations has the effect of reducing the number of demand inputs flowing into the system and, even more important, of restricting their content. The regime is thus protected from the possible consequences of a divisive public debate on basic policy questions.

IX. THE LIMITED IMPACT OF CITIZEN DEMANDS ON OFFICIAL DISCUSSION

The impact of citizen demands, or more precisely the impact of the data that results from the aggregation of citizen demands by local officials, on the deliberations of local decision-makers is difficult to determine because of the lack of access to the minutes of the most important local decision-making bodies: the city executive committee and the bureau of the city party committee. Nevertheless, a few observations, based on the local press reports of meetings of the full city party committees of Moscow and Leningrad from 1953 to 1967, are possible.

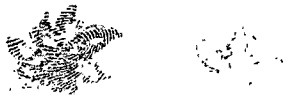
These reports have certain shortcomings. They are not verbatim accounts, but extended summaries. No way exists to determine the effects of censorship on the reports. Nevertheless, the reports are extensive summaries, attributing specific remarks to specific speakers. The speakers themselves constitute a fair cross section of that part of the local elite with an interest in the matter under discussion. Finally, the press reports provide the only accessible source of information on the deliberations of the local elite.[32]

The city party committees discussed various topics, among which were the state of housing construction, consumer goods production, and the state of retailing and various consumer and communal services. These were areas of governmental activity that gave rise to numerous citizen demands. During the course of these meetings, speakers sometimes stated that citizen needs, wants, complaints, or requests in some area were not being met. However, the speakers usually attributed the failure to meet citizen demands to the poor performance of some particular agency, institution, enterprise, or branch of the economy and never to centrally established policies and priorities. In other words, speakers occasionally used aggregated data on citizen demands to raise more general demands, but the more general demands themselves were limited. Speakers used the citizen demands as an indicator of how well some agency, institution, or branch of the economy satisfied the needs or demands of the populace within the limits set by centrally established policies and priorities.

Even this limited use of citizen demands has not been common. The members of the local elite are subject to various pressures; but of these pressures, the pressure of public opinion is not among the strongest. As a result the local elite is not very responsive to it. The Party leads the masses, not the masses the Party. The aggregated data on citizen demands are treated like any other aggregated data on performance. The information provides an opportunity to act and is taken into account, but it provides no compulsion to act. Indeed, aggregated data on unmarketable stocks of consumer goods may seem more compelling than aggregated data on consumer complaints about goods purchased.

[31] Fainsod, *Smolensk Under Soviet Rule,* pp. 378–379.

[32] For examples of such discussions see: *Vecherniaia Moskva,* May 30, 1956, p. 2; December 9, 1960, p. 1; and *Leningradskaia Pravda,* February 28, 1961, p. 1; April 25, 1961, p. 1.



The speakers from the various agencies, institutes, and organizations have their own particular demands to promote. They are spokesmen not for the citizenry, but for some segment of the governmental, party, or economic apparatuses; and they usually use aggregated data on citizen demands only when the data supports their own particular perceived interests.

To assert that the party and governmental officials have no regard for public opinion or the welfare of the citizenry would be incorrect; but even the highest ranking city officials—the secretaries of the city party committee—have only a limited ability to cope with the more general problems implicit in the aggregated citizen demands. They can provide some satisfaction for the demands by calling on the government agencies fulfilling the housing construction program to build better housing. They can call on enterprises to produce more and better consumer goods. They can support the requests of the city construction agency, the city trade agency, or the city consumer services agency for more resources. The city party officials' demands directed to the center are, no doubt, often influential because of their political standing in the system. However, even they can call only for the most efficient use of available resources to attain centrally determined goals; goals that might or might not be consistent with the satisfaction of citizen demands.

X. THE IMPORTANCE OF DEMAND PROCESSING

The lack of any autonomous groups in the intra-societal environment capable of aggregating and processing the specific raw citizen demands into a program for political action that can serve as an alternative to the one advanced by the ruling oligarchy weakens the impact of citizen demands on official deliberations and policy making. In the Soviet Union the raw citizen demands are processed entirely within the political system. This imposes a burden on officials, but it also assures the leadership a greater amount of decision-making autonomy by sharply reducing the pressures of public opinion. At the same time it assures the leaders opportunities for building support and a flow of needed information.

This deserves further elaboration.[33] One may argue that in any society the political system's institutional structure, the composition of the political leadership and public policies generate frustration, anxiety and dissatisfaction among the people. The people respond in various ways, among which is the articulation of demands by individual citizens. In many societies this is followed by a collective redefinition of demands by various organizations involved in the political process. Many of these organizations are independent of the regime and some stand outside the political system, although linked to it. Thus political parties, both those participating in the government and those in opposition, pressure groups, and other organizations take raw citizen demands along with other materials and aggregate and process them into alternative programs and policies. This leads to activities, protests and movements seeking to implement new policies, to capture and even to modify or overthrow the existing institutional structure for political decision-making. The result is civil debate and conflict. Citizen demands do not merely offer rulers useful information and opportunities to build support, but generate additional strain within the society and pressure for changes in public policy, leadership composition, and institutional structure.

In the Soviet Union the process is short-circuited at a critical point. Strains are generated; frustration, anxiety and dissatisfaction within the general populace exist; and citizens do articulate demands. However, these demands feed directly into the political system to be processed by members of the governmental and party apparatuses serving the ruling oligarchy. The lower level officials process and make use of citizen demands in ways that are useful to themselves, but they cannot, according to the norms they observe in self-interest and which they probably regard as legitimate, process the citizen demands into more general proposals that challenge the existing institutional structure, the policies and priorities of the leaders—unless given a signal that discussion of such matters is desired by the leaders—or the composition of leadership. In fact, established norms prohibit even the leading members of the politburo from freely appealing to the public or groups for support on controversial matters.[34] Consequently, the potential of citizen demands for generating additional strains and pressures for change is sharply reduced. On the other hand, the toler-

[33] The following discussion is based on an adaption of a "Simplified Systematic View of the 'Collective Behavior' Approach to a Theory of Institutionalism" presented by Walter Buckley, *Sociology and Modern Systems Theory* (Englewood Cliffs, N.J.: Prentice Hall, 1967), p. 138.

[34] For a discussion of this point in the context of high level disputes over agricultural policy see Sidney I. Ploss, *Conflict and Decision Making in Soviet Russia, A Case Study of Agricultural Policy, 1953–1963* (Princeton, N.J.: Princeton University Press, 1965), p. 84.

ation and encouragement of citizen gatekeepers provide the officials and ruling oligarchs with opportunities to build support and a flow of information useful in adapting the system's structure and policies in ways the *leadership* believes appropriate, plus a means of reducing the erosion of support that may result from policies adopted and pursued with little regard for public opinion.

XI. CONCLUSION

Soviet citizens clearly tend their own gates and the quantity of citizen demands is sufficient to pose a serious threat of input overload at the lower administrative levels. The attempt to distinguish the Soviet political system from those of Western democracies on the basis of the absence of numerous citizen gatekeepers in the former and their existence in the latter is erroneous. Distinctions must be based on the kinds of demands that may be raised by these gatekeepers; the absence in the Soviet Union of institutions independent of the regime that can aggregate these demands; and the impossibility in the Soviet Union of using demands to create an alternative program to that advanced by the ruling oligarchy.

Citizen gatekeepers may be either an inevitable consequence of democracy or a necessary condition for it; but they are not a sufficient condition. In the absence of other democratic institutions, they may serve the interests of a dictatorship as well as they have ever served the interests of a democratic regime. They provide both with needed information and opportunities to build support.

This support may be built very cheaply. Not all demands need be given actual material satisfaction. The mere acceptance of a demand by local officials, some expression or indication of interest and concern (even if this amounts to nothing more than helpless thrashing about), can create support. The citizen is able to vent his grievances and make his wishes known. He may derive some release of hostility merely from seeing the discomfort of local officials as they struggle to meet his demands and suffer the criticism of higher authorities. Even this useless effort may make the regime seem more human and interested in the welfare of the ordinary citizen. The more limited, the more private the demand and the more personal its handling by local officials, perhaps the greater the support generated for the regime, even though the regime persists in the policies that created the conditions that gave rise to the demand.

THE ELECTORAL POLITICS OF GAULLISTS IN THE FOURTH FRENCH REPUBLIC: IDEOLOGY OR CONSTITUENCY INTEREST?

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The complex relationship between a deputy's Parliamentary voting record and his behavior in electoral politics in his constituency has rarely been traced in French political studies in a precise quantitative manner.[1] One would like to contrast the extent to which both the deputy's Parliamentary voting record and his choices regarding coalitions in local politics depend upon his personal ideology with the extent to which the same behavior depends upon the political characteristics of his constituency. Even though ideological cleavages may exist across constituencies themselves, finding behavioral outcomes related to constituency characteristics could suggest that the deputy is motivated by his desire to be reelected.

Studies that attempt a systematic analysis of deputies' behavior are few, despite Lerner's early exploration of the relationship between constituency characteristics and voting on the European Defense Community (EDC) issue.[2] Recently, MacRae, in his volume on Fourth Republic Parliaments, has clearly emphasized the primacy of ideology, particularly with respect to the voting patterns that resulted in unstable governments. More specifically, MacRae gives an ideological interpretation to his finding that intra-party voting divisions can be accounted for by issue-related scales.[3]

MacRae also tries to relate Parliamentary behavior to analyses of voting behavior in the constituencies. In general, however, his analysis, relegated to one brief chapter dealing with "Assembly Votes and the Constituencies," appears to have been incomplete.

For example, MacRae treats the Gaullist R.P.F. in the constituencies in less than a page. He states "we should expect the constituency relations of the Gaullists, as inferred from their scale positions after they began dividing on roll calls, to have been loose."[4] On the contrary, one of MacRae's Gaullist scales is in fact strongly related to an important extra-Parliamentary coalition behavior variable. Insofar as MacRae argues that "Among the many factors that contributed to these divisions [read different positions on the scales], ideology seems to have predominated,"[5] one might conclude that this finding supports the hypothesis that deputies' behavior in the Fourth Republic had a heavy ideological base and that the perceptions of other writers that politics is largely a "game" are exaggerated.[6] Alternatively, one might argue that both Parliamentary voting behavior and coalition behavior have a joint common determinant in the deputies' interest in reelection. We will in fact go on to show that coalition variables can be explicated just as well by models of pure political opportunism with regard to (1) the deputies' interest in possible winning coalitions in their constituencies and (2) the availability of appropriate coalition partners. Thus, for the specific case considered here, a "game" explanation of Fourth Republic politics seems just as valid as the ideological explanation at the present state of research. (With regard to "explanations," in a statistical sense we can only test whether behavior is independent of constituency characteristics. Factors other than personal ideology might well explain variations not explained by the constituency characteristics.)

Before proceeding with the empirical results, it will be useful to make explicit three different models of deputy behavior. MacRae's concept of Gaullist behavior is that "the Gaullist movement was ideological, and the most enthusiastic Gaullists took their stands [which might apply to coalitions as well as roll-call voting] as a

[1] The relationship between U.S. Congressional voting behavior and constituency variables (attitudinal or socioeconomic) has been explored with positive findings by Warren Miller and Donald Stokes, "Constituency Influence in Congress," this REVIEW, (March 1963), 45–56, and Lewis A. Froman, Jr., "Inter-Party Constituency Differences and Congressional Voting Behavior," *Ibid.*, 57–61. These studies cite the earlier literature.

[2] Daniel Lerner and Raymond Aron (eds.), *France Defeats E.D.C.* (New York: Praeger, 1958). The work was originally published in French as *La Querelle de la C.D.E.* (Cahiers de la Fondation Nationale des Sciences Politiques, Colin, Paris, 1956).

[3] Duncan MacRae, Jr., *Parliament, Parties, and Society in France 1946–58* (New York: St. Martin's Press, 1967), pp. 8–9, 67, 176–77, 214, 329.

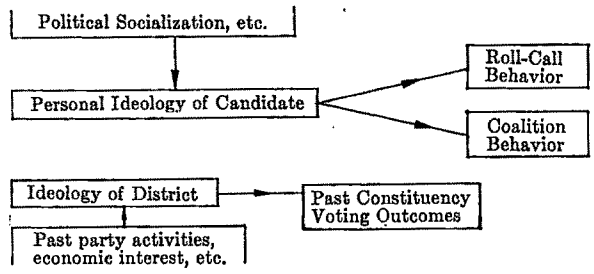
[4] *Ibid.*, p. 299.

[5] *Ibid.*, p. 214.

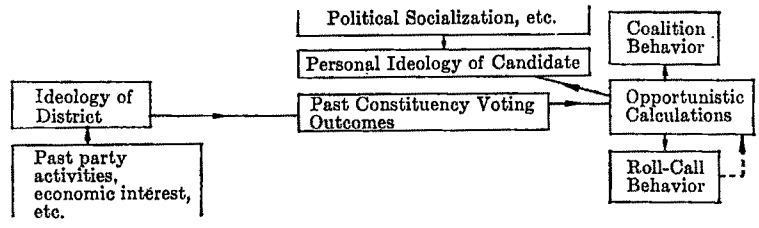
[6] See, for example, Nathan Leites, *On the Game of Politics in France* (Stanford: Stanford University Press, 1959).

THREE MODELS OF GAULLIST DEPUTY BEHAVIOR

A. Pure (MacRae) Ideological



B. Opportunistic



C. Congruent Ideological

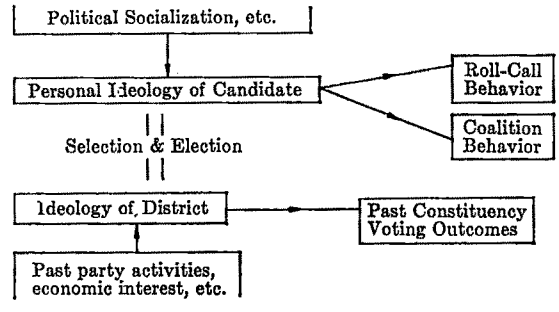


FIG. 1

matter of personal conviction rather than because they came from "Gaullist districts"—or conformed to them.[7] Hence, as shown in Figure 1A, past constituency voting behavior ought not to be related to either roll-call voting or coalition choices.

In Figure 1B, an alternative model is presented. This model, which may be termed the opportunistic model, posits the deputy as a calculating individual who simultaneously adjusts his roll-call behavior, his coalition behavior, and his personal ideology so that they might all be congruent with his constituency. (Of course, the opportunistic calculations may be influenced by goals such as cabinet status as well as the goal of being reelected). That is, opportunism is a behavior pattern with emphasis upon direct concerns with reelection. In this case, past constituency voting behavior ought to be related to both roll-call voting and coalition choices.

However, the same prediction could be made for the model of Figure 10, which can be termed the "congruent ideological" model. In this case, as in the MacRae model, the deputy would develop ideological convictions independently of constituency voting behavior. These convictions constitute a belief system, a program for future behavior. Those individuals with like convictions ought to have similar roll-call voting patterns regardless of any constituency interest. (The Communist Party accepts this premise and the converse that identical voting implies identical

[7] MacRae, *op. cit.*, p. 299. See also pp. 142–143.

individuals convictions.) They ought to have a common frame of reference for coalition building that extends beyond opportunism. Nonetheless, a deputy could choose to stand for election in a district that was ideologically congruent with his personal ideology. Thus his roll-call choices and coalition choices would, while proceeding out of personal conviction, be related to constituency voting behavior. Although the empirical analysis for 1956 does lead to the rejection of the MacRae model, it does not lead to a clear choice between the other two models since it was not possible to operationalize the intervening variables.

Of course, one is not bound to a choice between opportunistic models and congruent ideological models. Elements of both may be at work. Moreover, one must recognize the possibility that roll-call votes can exercise an influence on the calculating process, as shown by the dashed arrow in Figure 1B.

In 1951, the deputies had to vote upon the electoral law immediately prior to the election. Deputies voting against the coalition system might wish to remain consistent with their roll-call choice and not engage in coalitions when circumstances were otherwise favorable. This possibility will be considered for the 1951 elections. Our analysis for these elections will develop only weak support for the congruent ideological model while opportunistic considerations will provide a necessary if not sufficient condition for Gaullist coalition formation. Since the data show stronger connections for 1956, it is perhaps best to begin with the later year.

I. THE 1956 ELECTIONS

For the 1956 election, we focus upon the choice of *apparentement* coalitions by the Gaullist *Républicains-Sociaux*. The electoral law permitted parties to preserve their identity while making coalitions. Any coalition, or *apparentement* as they were termed in France, operated as a simulated single list in the proportional representation system. Any *apparentement* that won 50% of the votes in the constituency won all the seats of that constituency.[8]

In the 1956 elections, one can distinguish four basic types of coalition choices. First there was the possibility of engaging in no coalition. Secondly, the Gaullists could affiliate themselves with other parties on the political Right (i.e. parties other than the Communists, Poujadists, and Republican Front parties). When they actually engaged in *apparentements*, this was their modal choice. The third possibility was to participate in coalitions whose other members were part of the Mendesist Republic Front. This means lists sponsored by the Socialists, the Radical-Socialists (but not R.G.R.), the U.D.S.R. (a small left of center post-war group headed by a Fourth Republic premier, René Pleven, and De Gaulle's opponent in the 1965 Presidential elections, François Mitterand), the *Jeune République* (largely intellectuals, a forerunner of the present political clubs), and *Redressement Economique et Sociale* (a group of Left Catholics). Finally, the Gaullists could participate in the rare coalitions that straddled both Republican Front and Rightist parties.

The *Républicains-Sociaux* are an interesting group to study with respect to coalition choices not only from the historical viewpoint that many of these candidates later went on to become leading figures in the Fifth Republic, but also for the particular situation that confronted the *Républicains-Sociaux* in 1956. Because the Gaullists have always protested against being assigned a position in the traditional Left-Right continuum of French politics, one could expect to find them somewhat unwilling to accept the simple Left-Right split between the Republican Front and the other parties that was sparked by conflict over the *loi Barangé*, a measure to aid parochial schools passed in 1951, and crystallized by conflict between two Radical prime ministers, Pierre Mendès-France and Edgar Faure. As Williams observes, "The campaign offered the voter the clearest choice since the war. *Apparentements* were made between Socialist and 'Republican Front' Radicals and between M.R.P., Conservatives, and Radical dissidents; the R.S. (Gaullists) were divided."[9] The divisions among the Gaullists concerning their participation in the Republican Front have generally been exaggerated.[10] Only two incumbent Gaullists, Krieger in Moselle and Bricout in Aisne, participated in coalitions in which the only other partners were

[8] A complete exposition of the electoral system is available in Peter Campbell, *French Electoral Systems and Elections* (New York: Praeger, 1968), p. xvi.

[9] Philip Williams, *Politics in Post-War France: Parties and the Constitution in the Fourth Republic* (London: Longmans, Green, Second Edition, 1958), p. xvi.

[10] For example, Williams, *op. cit.*, p. xxii, cites that M. Chaban-Delmas joined the Republican Front. But there was no Republican Front *apparentement* in M. Chaban-Delmas' district. Other "Mendesist" Gaullists had similar behavior. Williams' analysis is also found in his revised and extended work, *Crisis and Compromise: Politics in the Fourth Republic* (London: Longmans, 1964), pp. 47–48, 136.

Republican Front parties; and Bricout's coalition only went as far as the Radicals. Gaullist lists without incumbents joined the Republican Front in only seven of the 95 *apparentement* constituencies. All of these (Bouches-du-Rhône —1er, Haute-Garonne, Indre, Lot-et-Garonne, Haute-Marne, Pyrénées-Orientales, and Yonne) were districts in which the Right could not have expected to win under the criterion advanced below. Consequently, basically the Gaullists had a choice between participating in Right coalitions or not joining any coalition.

1. *Parliamentary voting and coalition behavior.* One might easily hypothesize that the decision of a Gaullist to participate or not in a coalition would be related to his political opinion or ideology. After all, the Gaullists had originally been heavily opposed to the *apparentement* system when it was voted in 1951.[11] Insofar as the *apparentement* system remained an important symbol of the hated Fourth Republic, one could expect that the purest of the Gaullists would resist the temptation to enter such a system and the less pure would be more prone to enter into coalitions.

Our analysis is confined to a group of relatively pure or "hard-core" Gaullists, those who not only remained in the U.R.A.S. after the more "opportunistic" members had formed the A.R.S. but also adhered to the *Républicains-Sociaux* at the end of the legislature.

The purity of a deputy's Gaullism can perhaps be measured by his vote on measures of relevance to the Gaullist movement. MacRae gives the position of the Gaullist (U.R.A.S.) deputies in the Second Legislature on two scales.[12] The first of these scales concerns economic matters. It does not give a good differentiation of coalition behavior. The second scale concerns the deputies' attitudes towards German rearmament and voting on this issue does have a strong relationship with coalition behavior. This might be expected, for, while the Gaullists were not strongly differentiated from Fourth Republic governments on economic matters, they tended to deviate strongly with respect to foreign policy. Not conforming to the General's position on foreign policy was critical. The Gaullists had, in 1954, played a key role in defeating the European Defense Community in the French Parliament. The Western nations replaced the European Defense Treaty with the Paris Accords which carried out German rearmament through German membership in NATO. Votes on the Paris Agreements comprise most of the items on MacRae's Germany scale.

Although MacRae's scale has four levels, a simple dichotomization of the scale is sufficient to discriminate coalition behavior. Gaullist deputies were thus categorized into two groups. The anti-German rearmament group produced negative votes on five motions concerning the Paris Agreements, including final passage.[13] The pro-Germany group voted these motions. General De Gaulle's strong opposition to German arms would indicate that the "anti-Germans" are to be considered the pure Gaullists who would not be willing to participate in coalitions.[14] A preliminary indication that the "anti-Germans" are in fact the pure Gaullists comes from the fact that both the General's brother, Pierre De Gaulle, and brother-in-law, Jacques Vendroux, belonged to the "anti-Germans." (However, the General's "Mendesist" nephew Diomede Catroux was "pro-German.") Jacques Soustelle, the only original founder of De Gaulle's post-war party, the R.P.F., standing for reelection, was also "anti-German."[15]

The test of the relationship between position on the Germany issue and coalition behavior was made in the following manner. First, Gaullist *apparentement* participation in every district where an incumbent Gaullist deputy stood for reelection was classified into the four coalition categories described above. Then, each district was categorized as being "pro-German" or "anti-German" according to the position of its highest listed Gaullist incumbent standing for reelection. (There was one incumbent, M. Bourgeois, who could not be scaled by MacRae. The position of his *co-listier*, M. Ritzenthaler, was used.) The results are shown in Table 1. Of the 19 "anti-German" districts only six show coalitions and only four had coalitions with the Right. In contrast, the "pro-German" districts show almost precisely the reverse behavior. Ten show coalition with the Right and only one has no coalition at all. (Comparing only Right coalition districts to no coalition districts shows a Q coefficient of .94).[16] After excluding the four districts where other than Right *appar-*

[11] However, some R.P.F. members individually favored joining coalitions. See Christian Purtschet, *Le Reassemblement du Peuple Français* (Editions Cujas, Paris, 1965), pp. 310–311.

[12] MacRae, *op. cit.*, p. 194.

[13] *Ibid.*, p. 140.

[14] The positions of various Gaullists are discussed in Stanley Hoffmann, "Les Oraisons Funèbres," in Lerner and Aron, *op. cit.*, pp. 59–87 (French edition).

[15] Purtschet, *op. cit.*, p. 67, lists the founders.

[16] The Q coefficient is used here largely for reasons of comparability with the MacRae study. Other measures of association might be appropri-

entements took place (as is done throughout the paper), there are only five deviant cases. It would indeed seem that MacRae's "anti-Germans" are those who refused coalitions in 1956.

2. *"Opportunistic" Coalition Behavior.* Nonetheless, there is an alternative explanation of the coalition behavior. The present writer has worked for some time with models of coalitions in which coalition behavior was seen to be a function of party membership and the distribution of political forces in the constituency. No attempt is made to take into account ideological differentiation within the parties.[17]

From the "opportunistic" viewpoint of these models, the Gaullists ought to become part of right-based coalitions where the Right could in fact win a majority of the vote in the election. Of course, they might not become a part of the coalition if the other parties could form a majority without them, but this condition would hold in only a small number of districts.

The most obvious calculation of districts with a potential Right majority would simply be to total up the votes from the *previous* election, held in 1951. Where the Right totalled over 50%, one would postdict that the Gaullists would join a coalition. A uniform calculation of this type was made. For all districts, based on the 1956 split between the Mendesist Republican Front Radical-Socialists and Edgar Faure's R.G.R., the Right was defined as follows: R.P.F., *Modérés,* M.R.P., joint lists of Radicals and *Modérés,* R.G.R. in districts where a Radical-Socialist list also existed, R.G.R.I.F., and other Right parties.[18] Tabulating this variable against the coalition outcomes yields a positive relationship (Q = .78). In only one of the nine districts where the Right would lose did the Gaullists make Right-based coalitions, while they did so in 13 of the 22 districts where they could win. Nevertheless, this relationship is weaker than the one found from the scale variable. Comparison of Tables 1 and 2 shows there are four more deviant cases in the latter case.

ate were the data to be used for non-descriptive purposes.

[17] See Howard Rosenthal, "Voting and Coalition Models in Election Simulation," in William Copline (ed.), *Simulation and the Study of Politics* (Chicago: Markham Press, 1968) and "Size and Winning Coalitions in the Fourth French Republic" in Sven Groennings et al. (eds.), *The Study of Political Coalitions* (New York: Holt, Rinehart and Winston, 1969).

[18] Election statistics from *Les Élections Legislatives du 17 Juin, 1951,* (La Documentation Française, Paris, 1955).

TABLE 1. 1956 ELECTION DEPUTY'S SCALE POSITION AND COALITION BEHAVIOR-CONSTITUENCIES WITH INCUMBENT GAULLIST CANDIDATES

Coalition Entered by Gaullist	Head of Gaullist List		
	"Pro-German"	"Anti-German"	
Republican Front	1	1	2
Republican Front and Right	1	1	2
None	1	12	13
Right	10	4	14
Total	13	18	31

However, clearly a more sophisticated "opportunistic" calculation could be made. The R.P.F. in 1951 was a "flash" movement attracting substantial support from uncommitted voters and habitual "left" voters. By 1956, the *Républicains-Sociaux* were much weaker than the R.P.F. and a reasonable expectation would have been that this uncommitted and left support had dissipated. Thus, the politicians might have discounted the R.P.F. vote in their 1956 calculations.

The discounting can be simulated by adding only a fraction of the R.P.F. vote to the remaining right vote in calculating a majority. By running a number of simulations in which this fraction varies, one can find that fraction(s) which minimizes the number of deviant cases; one can also test the sensitivity of the postdiction to the discount factor.

The best fit occurs with 74% of the R.P.F.'s 1951 total added to the vote of the other Right

TABLE 2. 1956 ELECTION POSSIBLE WINNING COALITIONS AND COALITION BEHAVIOR-CONSTITUENCIES WITH INCUMBENT GAULLIST CANDIDATE R.P.F. 1951 VOTE WEIGHTED AT 1.0

Coalition Entered by Gaullist	Right Coalition Would		
	Win	Lose	Total
Republican Front	1	1	2
Republican Front and Right	0	2	2
None	8	5	13
Right	13	1	14
Total	22	9	31

parties. The tabulation produced from this simulation (Table 3) shows as strong a relationship with coalition formation as does the scale variable ($Q = .91$). There are five deviant cases in both situations. Moreover, any value (see Figure 2) of the fraction between 60% and 90% would have given no more than seven deviant cases as against the minimum of five. Hence, we conclude that while the postdiction is definitely improved by introducing some discount of the R.P.F. vote, any reasonable value would have worked well!

Confidence in the necessity of introducing a discount on the R.P.F. vote in the calculation of constituency structure is increased by the demonstration that the discount model is superior to an alternative model. The alternative model is that the politicians simply biased the earlier calculation of the Rightist vote upwards or downwards. This can be simulated by multiplying the Rightist vote by a number greater than one for upward bias, a number less than one for downward bias. Figure 2 shows that this model never yields fewer than eight deviant cases and is much less stable than the discount model. Hence, the importance of discounting the R.P.F. vote.

In addition to these two models, a posterior model was also examined in which the 1956 vote for the Right plus a fraction of the Poujadist vote was used as indicating a potential Right majority. This also shows a positive relationship, but, as Figure 2 indicates, not so strong a relationship as the prior model.

Finally, it may be asked why no results have been presented for simple dichotomizations of the percentage of the Right vote in 1951. The reason is that such dichotomizations are equivalent to the simulation involving a biased Right vote. For example, predicting a coalition in districts where the Right vote multiplied by .8 is greater than 50% is equivalent to predicting a coalition in districts where the Right won over 40% of the vote. However, we have preferred to state relationships in terms of the majority figure, since the majority concept is clearly related to coalition theory.

TABLE 3. 1956 ELECTION
POSSIBLE WINNING COALITIONS AND COALITION BEHAVIOR-CONSTITUENCIES WITH INCUMBENT GAULLIST CANDIDATE. R.P.F. 1951 VOTE WEIGHTED AT .74

Coalition Entered by Gaullist	Right Coalition Would		
	Win	Lose	Total
Republican Front	1*	1	2
Republican Front and Right	0	2	2
None	2	11	13
Right	11	3	14
Total	14	17	31

* Majority Right Coalition Without Gaullist.

The utilization made here of the majority concept is undoubtedly overly simple as it has been assumed that the discount factor was constant across districts. But information as to the "leftism" of R.P.F. voter support across districts should only serve to improve the model.

The finding that both the voting record variable and the constituency interest variable discriminate coalition behavior could only result if in fact districts where the Right could win usually were districts that produced pro-Germans (in 10 out of 14 cases) and districts where the Right could not win were almost always districts that produced anti-Germans (in 14 out of 17 cases). (The Q coefficient for the Right win-No Right win dichotomy vs. the Pro-German–Anti-German dichotomy is .84). Thus, for the Gaullists, we have a very close relationship between constituency structure and Parliamentary voting behavior on the Germany issue. This relationship is much weaker if the R.P.F. vote is not discounted.

Further indication that the R.P.F. discount measure of Rightist strength could have been used to predict the Parliamentary behavior of Gaullist deputies is that there is an expected, if weak, positive association between voting on the Right in MacRae's "economic" scale and representing a district where the Right could win ($Q = .59$).[19] Since this scale refers largely to taxation policy rather than general left-right views, the low level of association is perhaps not surprising. Unfortunately, the association of the R.P.F. discount measure with other scales could not be checked as MacRae did not indicate the deputies' scale positions on these scales.[20]

One could also expect constituency structure to be most influential for those deputies who in fact stood an objective chance of reelection. Now the U.R.A.S. deputies scaled by MacRae but not heretofore included in this analysis

[19] Scale categories "0" and "1" were counted as "Right"; "2," "3," and "4" as "Left." See MacRae, *op. cit.*, pp. 140, 194.

[20] Although Professor MacRae has kindly made his data tapes available through the Inter-University Consortium for Political Research, at this writing the author has not reprocessed these tapes to compute the additional scores.

INCORRECT POSTDICTIONS OF GAULLIST COALITION
BEHAVIOR FROM VARIOUS SIMULATIONS

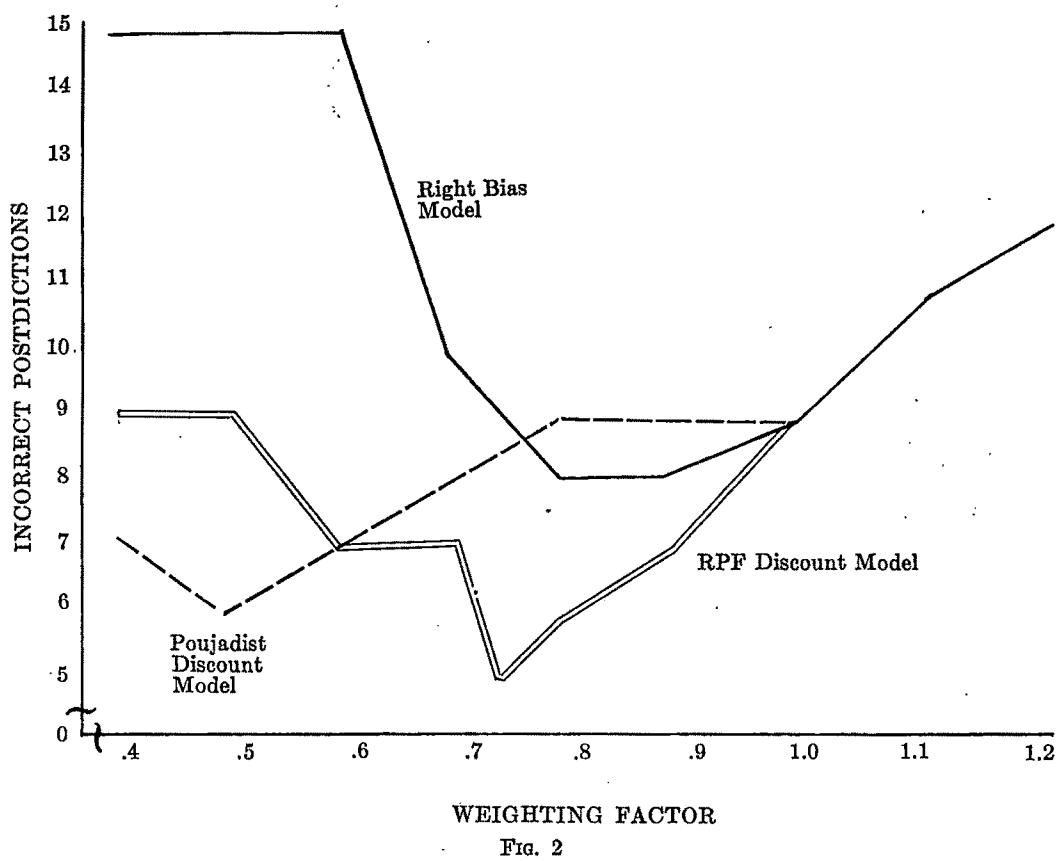


FIG. 2

Note: At a Weighting Factor of 1.0, the Right Bias and RPF Discount models are equivalent.

are deputies who either did not stand for reelection or ran in a list position behind that of another *Républicains-Sociaux* deputy. In the latter case, the decline in Gaullist electoral fortunes meant that such deputies stood virtually no chance of being reelected. The corresponding Q coefficient for the remaining U.R.A.S. deputies on the Germany issue is .75, against .84 for the highest-placed deputies. This difference is small but in the expected direction.

3. *Constituency structure in districts without incumbents.* While it is not possible to extend the analysis based on roll-calls concerning German rearmament to districts which had no incumbent Gaullist deputy, we can extend the analysis of constituency structure to the remaining districts. This analysis will further suggest that constituency structure is most influential in situations where there is a real possibility of winning a seat. First, it is necessary to dispose of 28 districts in which the Gaullists presented no list (or had lists that obtained no votes) and of course could not participate in coalitions. Even if lists had existed in these districts Gaullists would rarely have been important coalition partners since most of these districts were in areas of Gaullist weakness. Thus, 16 of the 28 constituencies were constituencies in which the Republican parties had formed majority *apparentements* in 1951. Three more, Bouches-du-Rhône (2), Corrèze and Isère were heavily Leftist with high Communist polls; the Communists obtained 34% of the vote or better in all three districts in the previous election of 1951. In nine districts, including six not previously accounted for, when the Gaullists split up during the Second Legislature, all those elected on the R.P.F lists in 1951 deserted the movement and placed their lot with the *Modéré* A.R.S. group. (The Gaullists never entered a coalition in districts in which all the R.P.F. incumbents shifted to A.R.S.) Thus only three of these districts that

did not have a Gaullist list might have been somewhat favorable to Gaullism.

Eliminating these 28 districts in addition to the 31 districts that had incumbent Gaullists leaves a residual 36 districts with Gaullist lists without incumbent Gaullist deputies. The relevant cross tabulation (for the R.P.F. discount model at .74) is shown in Table 4. First of all, this table is consistent with the hypothesis that the Gaullists only entered Republican Front coalitions or coalitions that straddled the Left-Right divide when a Right winning coalition was not possible. It also confirms that there was a greater tendency to produce coalitions with the Right in districts where the Right could in fact win, although this tendency was weaker than in districts with incumbent Gaullist deputies. (The comparable Q coefficient is only .47. However, the Q coefficient for the case where the Right coalitions are compared to the totality of the other three categories is .62).

The weaker relationship found in districts without incumbent Gaullist deputies could be related to the fact that, in areas without incumbent Gaullists, the Gaullists were less visible and less likely to be sought as coalition partners by the other Right parties. In two of the four districts where the Gaullists did not enter a coalition where the Right "could" win, the other parties could evidently dispose of the Gaullists since they formed a winning *apparentement* in those districts. (These districts were Orne and Vendée.) This exclusion is thoroughly compatible with current coalition theory following Riker's size principle.[21] (Removing these two cases then leads to a Q coefficient of .69.) Similarly, one of the deviant cases in districts with incumbent deputies can also be reconciled by the size principle. This is the Moselle where Krieger's Gaullist list joined a Leftist *apparantement* in a district where a Rightist *apparantement* had the majority.

Perhaps even more so than size considerations, direct cases of political conflict appear to have had an influence upon coalition entry of non-incumbent *Républicains-Sociaux* lists. Eight of the thirty-six non-incumbent districts are districts where all the former R.P.F. deputies running for reelection had deserted to A.R.S. and other groups. Removing these eight districts from the computations yields a Q of .81 when Right coalitions are contrasted with no coalitions and .88 when Right coalitions are contrasted with the totality of the other three categories.

TABLE 4. 1956 ELECTION
POSSIBLE WINNING COALITION AND COALITION BEHAVIOR-CONSTITUENCIES WITHOUT INCUMBENT GAULLIST CANDIDATE. R.P.F. 1951 VOTE WEIGHTED AT .74

Coalition Entered by Gaullist	Right Coalition Would		
	Win	Lose	Total
Republican Front	0	7	7
Republican Front and Right	0	2	2
None	4*	11	15
Right	6	6	12
Total	10	26	36

* 2 of 4 contain majority Right coalitions without Gaullists.

[21] William Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962).

II. THE 1951 ELECTION

In contrast to the 1956 situation, neither roll-call information nor constituency information can be used to discriminate the coalition behavior of *incumbent* Gaullist deputies in 1951. For despite the fact that the Gaullists concluded 13 *apparentements*, no *apparentements* were recorded in the 19 districts where an incumbent Gaullist stood for reelection as the head of a list (although one incumbent, M. Montillot in Haute-Saône, was the second man on a list in a coalition district).[22] Yet those *apparentements* concluded were profitable; the Gaullists gained five seats over what they would have won without the *apparentements*.[23] Some party members believed that had the Gaullists actively pursued a coalition policy in 1951 they would have been able to come to power in 1951 rather than in 1958.[24] Why the reluctance of the incumbents?

1. *Roll-call vote and coalition choice.* The avoidance of *apparentements* by the incumbents does not appear to have its roots in a common ideological orientation. In the First Legislature, while eight of these men had been classified by MacRae as belonging to the Right, five others were drawn from the Radicals, with the remaining six coming from splinter groups not scaled by MacRae, five of these coming from the *Ré-*

[22] Candidates running on homogeneous R.P.F. lists have always been treated as Gaullists. In the case of joint lists, a classification was made on the basis of whether the deputy joined the R.P.F. Assembly group after the election. (All heads of joint lists were elected.)

[23] On the assumption that the votes by party would not have been changed.

[24] Purtschet, *op. cit.*, p. 280.

publicains Popularies Indépendants and one from the *Action Démocratique et Sociale.*

The heterogeneity found in the First Legislature is repeated in the Second. Six of the incumbents had been defeated for reelection. Of the remaining thirteen, four joined the right wing of the R.P.F. in splintering into the A.R.S. Of the nine remaining U.R.A.S. members, four were anti-German, five pro-German. They also failed to cluster on the economic scale. Thus the incumbent Gaullist heads, all of whom failed to join *apparentements,* did not vote similarly on roll-calls that divided the Gaullists.

Should similar roll-call voting be expected if the coalition behavior was ideologically motivated? The *apparentement* system was opposed by the Gaullist leadership not only because of its intent of reducing Gaullist representation but also because it was an institution and symbol of the Fourth Republic. In turn, the behavior pattern for Gaullists called for opposing the Republic at every opportunity.[25] Thus one would expect, on an ideological basis, the coalition joiners to be the weak-hearted Gaullists, those who later on wound up in the A.R.S. or were classified as pro-Germany on the Germany scale or "right" on the economic scale. Those who did not join coalitions should tend to be "anti-Germans" and "left" on economic matters.

It has thus been shown that the incumbents, in contrast to the ideological expectation, did not form a group of hard-core Gaullists in the Second Legislature. One may argue that they failed to join coalitions since they did not want to incur voter displeasure for having participated in coalitions they voted against. Or, alternatively, one may, as is done below, find an alternative explanation in the characteristics of their constituencies. But it will first be necessary to explore the Parliamentary behavior of non-incumbents.

Did those heads of lists elected after having joined an *apparentement* fulfill the ideological expectation that they would be among the less loyal Gaullists in the Second Legislature? Of the 10 elected heads of lists, only three defected to the A.R.S., yielding a defection rate equivalent to that observed for the non-coalition building incumbents. Of the seven U.R.A.S. deputies, three were anti-German, four pro-German. There was also no clustering on the economic scale.

The *apparentement* makers can be compared with another group, the 27 non-incumbent heads who were elected without *apparentements* (from districts where *apparentements* were permitted) in 1951. Of those deputies that could be scaled, 11 were anti-German as against only five pro-Germans (again no clustering was found on the economic scale). This suggests that among non-incumbents, those who did not make coalitions tended to be loyal Gaullists. But in the relevant computational comparison between non-incumbent joiners and non-joiners, the measure of association is fairly weak ($Q = .42$). Furthermore, the non-joiners had a rate of defection similar to that of the joiners. Seven of the 27 joined the A.R.S., one joined P.C.D., and one was not a member of any group at the end of the Second Legislature. On the basis of the above results, it does not seem possible to make a case for ideological coherence in coalition decision-making.

2. *Modéré incumbents: a necessary condition.* Since neither roll-call voting nor Parliamentary group membership give a clear discrimination of coalition behavior, it remains to work through an opportunistic calculation. Inspection of the R.P.F. *apparentements* indicates that in no case did the R.P.F. conclude an *apparentement* that did not include a Modéré partner. This is true even when Radicals and/or M.R.P. were part of the *apparentement.* Inclusion of the Modérés in any R.P.F. alliance may have been necessary for the R.P.F. to retain the support of conservative voters. The Modérés may also have been necessary brokers between the R.P.F. and other partners, particularly M.R.P. where there had been strong opposition over the electoral law, foreign policy, and other matters. Consequently, it is appropriate to ask what circumstances may have facilitated a Modéré-R.P.F. alliance. The presence of a Modéré incumbent running for reelection, either as a Gaullist or a Modéré, ought to have been important, for the incumbency would indicate (1) the availability of a prominent politician to conduct negotiations (interview materials indicate the primordial role of incumbents in coalition negotiations[26]); (2) the availability of a large enough package of votes to generate payoffs to the coalition.

In fact, the indicator of Modéré incumbency has a reasonably strong relationship with the coalition outcomes. (Members of the U.D.I., R.I., P.R.L., and *Action Paysanne* Parliamentary groups were treated as Modérés.) Only 2 of the 13 R.P.F. coalitions occurred in districts without Modéré incumbents, suggesting that the presence of such an incumbent can be regarded as a necessary condition for a R.P.F. alliance. Significantly, the two deviant cases (Nièvre and Pyrénées-Orientales) represent, in terms of total

[25] *Ibid., passim.*

[26] Interviews conducted by the French Institute of Public Opinion (IFOP) for research directed by the author and Guy Michelat.

vote, the two smallest R.P.F. coalitions. Both coalitions were with the miniscule Taxpayers (*Contribuables*) party; in both cases the partner added only 7% of the vote to the R.P.F. total. The overall relationship, represented in Table 5, shows a Q-coefficient of .76.

The foregoing relationship, which once again links a constituency characteristic to coalition behavior, holds at the .79 level if districts with R.P.F. incumbents as heads of list are removed. And, to draw the relevant comparison with the earlier exploration of ideological relationships, a .76 Q-coefficient results if the comparison is further limited, as in Table 6, to districts in which the Gaullists won at least one seat.

(The posterior dichotomy R.P.F. seat vs. no R.P.F. seat has a Q of .75 with the coalition variable. The dichotomy of blocked (districts where in 1946 Communists plus Socialists totalled over 50% of the vote) vs. non-blocked yields a Q of .55.)

The role of Modéré incumbency may also hold a key to the behavior of the non-joining R.P.F. incumbents. Of these nineteen heads of list, nine came from districts without a Modéré incumbent. Thus, their behavior could be explained without any reference to preserving consistency with the roll-call vote on the electoral law. (In passing, it can be noted that these nine men did not appear to select their districts on the basis of ideological congruence—two were "anti-Germans," three "pro-Germans," two A.R.S., and two were not reelected.) What, however, of the ten men who did run in Modéré districts? Again there is an alternative to the roll-call consistency explanation, at least for seven of the ten districts. In these districts, the Modéré incumbents were all members of R.P.F. sponsored lists. In such a circumstance, it would be reasonable to assume that there would have been greater difficulty in negotiating a coalition. (Nevertheless, it should be pointed out that a similar condition occurred in two of the coalition constituencies.) Thus there may be some

TABLE 5. 1951 ELECTION
MODERE INCUMBENCIES AND R.P.F.
COALITION BEHAVIOR

Gaullists In *Apparentement*	Modéré Incumbent		
	Yes	No	Total
Yes	11	2	13
No	35	47	82
Total	46	49	95

TABLE 6. 1951 ELECTION
MODERE INCUMBENCIES AND R.P.F. COALITION
BEHAVIOR-DISTRICTS WHERE R.P.F. WON
AT LEAST ONE SEAT

Gaullists In *Apparentement*	Modéré Incumbent		
	Yes	No	Total
Yes	9	1	10
No	15	12	27
Total	24	13	37

basis for the suggestion that coalition behavior on the part of Gaullist incumbents was linked to constituency factors as well as to the need to be consistent with the roll-call vote on the electoral law.

Now it would be hard to conclude that Gaullists preferred coalitions in Modéré districts on the basis of personal ideology. It can be assumed that Modérés, incumbent or not, represent similar ideological alternatives. Nonetheless, a relevant question is whether the incumbent Modérés in the coalition districts can be differentiated ideologically from the other incumbent Modérés. However, no clustering was found, with respect to both Parliamentary group membership and the two scales MacRae presents for the Right in the First Legislature. Thus, for 1951, MacRae's treatment of the roll-call data yields no important linkages to R.P.F. coalition behavior. But since Modéré incumbency yields only a necessary condition for R.P.F. coalitions, one can ask, but not answer, if the final determinants of R.P.F. coalitions reside in undiscovered ideological or calculating factors or simply in personal quarrels and other local idiosyncracies.

III. COMPARISON OF THE OPPORTUNISM MODELS

For 1951, opportunities for coalition formation were viewed as coming from incumbency locations while in 1956 a size calculation was made. Could one not have applied a version of the 1951 model to 1956 and vice-versa?

For 1956, the *Républicains-Sociaux* never formed coalitions without either Radicals or Modérés. In districts with *Républicains-Sociaux* incumbents, there were too few Radical incumbents to permit an evaluation of Radical incumbency influence. Modéré incumbency showed no relationship ($Q = .11$) to coalition formulation.

In non-incumbent districts, with the eight defector districts again excluded, there is a positive association ($Q = .50$ for Right vs. no coalitions, and $Q = .67$ for Right vs. all other

categories), but a much weaker one than that found for the size calculation.

A size calculation could also be made for 1951. One calculation would be to estimate that the R.P.F. would tend to form coalitions where the Communists (regular and Trotskyist) plus the Socialists totaled less than 50% in 1946. Such a computation leads to a moderate Q of .55. Another suggestion would be that the R.P.F. would join coalitions where the "center" (S.F.I.O., Radicals, and M.R.P.) parties did not carry greater than half the votes in 1946. This leads to a Q of .70. However, this model is very unstable. Any discounting, either of the total or of the M.R.P. vote alone, lowers Q quite rapidly. On the other hand, it can be argued that the 1946 election was not as good a source of information for the 1951 election as 1951 was for 1956. For example, in 1946, the Right had no representation at all in many constituencies. Therefore, better information gathered, say, from intervening local elections, might lead to a valid size calculation.

One must, nevertheless, conclude at present that the two models are not interchangeable. The facility of a size calculation in 1956 is suggested by Williams' comment, cited earlier, that 1956 offered a clear choice, a polarization between the Republican Front and the Right. In 1951, such an easy computation was not available and, furthermore, information about the distribution of voting strength may have been relatively poor. In such a case, incumbencies rather than votes would become the key behavioral indicators.

IV. CONCLUSION

To compare the usefulness of the constituency interest model to the roll-call voting record model for predictive purposes, for 1951, the available roll-call data had virtually no pay-off. For 1956, it can be seen that any reasonable discount figure in the opportunism model would have worked well. In contrast, to use a scale for predictive purposes, one would have had to have been able to specify which votes were significant and what the relevant dichotomizations should be. Furthermore, the fact that the Gaullists participated in three non-Right coalitions in districts where the Right would lose is quite consistent with opportunism. Krieger's behavior in Moselle can also be termed opportunistic since he had been refused admission to the Right coalition.[27] In terms of opportunism, it seems reasonable to move to the Left when you cannot win with the Right. On the other hand, it is more difficult to see why those with a "Right" position on the Germany issue would take a "Left" coalition posture, and these cases of participation in non-Right coalitions are left unexplained by the Germany scale model.

It would consequently appear that the explanation of coalition behavior on the simple basis of political opportunity ought to be taken seriously. Nonetheless, it is clear that we cannot entirely reject the hypothesis of the "congruent ideology" model.[28] The paucity of deviant cases excludes an examination of partial relationships. Is it the case that the "anti-Germans" simply select to run in districts in which the Right cannot win? On the surface, this proposition may seem ridiculous, but one would have to admit that on a matter so important to the General as the Paris Agreements, the votes of Jacques Vendroux and Pierre De Gaulle were not likely to have been dictated by constituency interests. And, in districts where the Gaullists entered Right coalitions even though the Right could not win, there are some indications that ideology played a role. For example, in Tarn-et-Garonne, it appears that the Gaullists were originally prepared to enter a Left coalition but finally entered a Right coalition after the key political figure in the department, the Radical deputy and editor of the *Dépeche du Midi*, Jean Baylet, wanted the Gaullists to sign a paper endorsing the lay position on the famous *question*

We have done so since we always consider homogeneous Radical-Socialist lists as Republican Front lists in order to have consistent classifications.

[27] Williams, *Politics, op. cit.*, p. xxii, n. 19, implies that Krieger had in fact tried to join with the Right and was rebuffed. Also, Williams would not count Bricout's alliance as Leftist.

[28] It has been shown here that constituency characteristics are at least as strongly related to electoral coalition decisions as are intra-party voting decisions. It has also been shown that intra-party voting is related to constituency characteristics. But insofar as the "economic" scale is less strongly tied to the constituencies than the "Germany" scale (.52 vs. .84) and since MacRae suggests that the "economic" scale is more closely linked to cabinet stability, we must entertain the hypothesis that ideology is more strongly related to cabinet coalition decisions than to electoral coalition decisions especially since cabinet decisions are more removed from the voter than electoral coalition decisions. In this case there would be room for both MacRae's "ideological" hypothesis and the present "opportunistic" hypothesis. See MacRae, *op. cit.*, pp. 140–142.

scolaire.[29] In general, however, the proposition that constituency interests dominated voting in the Assembly seems more tenable. Those districts where the Right cannot win are, by the other side of the coin, largely Leftist. They were thus less likely to be dominated by a rigid anti-Communism that could be used to rationalize German rearmament in a country that had been invaded by German arms three times in a century. (The R.P.F.'s virulent anti-Communism attracted many of the less stalwart Gaullists.)[30] In the case where constituency interests do in fact dominate voting in the Assembly, politics in France may well be a game. Even if French politics are not, in the interior of the Assembly, the narrow psychological game portrayed by Leites, at the exterior, there may be a pure political game of adapting one's position to winning electoral contests.[31] Consideration of what is necessary to win in a constituency may then dictate the deputies' decisions in the Legislature. From direct interviewing, it is known that at least the deputies' coalition decisions were influenced by what was necessary to win. Deputies claim not having made certain coalitions that they would have themselves preferred ideologically but which would not have been *"compris"* by their electors. Likewise, they frequently may have voted in the *Assemblée* in ways that would have been *"compris"* at home.[32]

The foregoing considerations may permit some reconciliation between the "ideological" and "gaming" interpretations of French politics Let us assume that ideology was important to voters and militants. But also assume that the politicians were interested in getting reelected and in other ego-related goals such as achieving positions of power. In this case, politicians and politics could, as indicated on Figure 1B, become ideological while remaining self-oriented. As such part of the "game" of unseating cabinets could center upon "ideological issues." The ideologies expounded are then not the mirror of the soul's conviction but of its ambition. The closeness of the associations between the constituency variables and both the voting record variable and the coalition variables are strong enough to present an alternative explanation to the emphasis placed by MacRae upon the personal ideologies of the Gaullist deputies.

While MacRae found that Parliamentary voting patterns clustered about issues, he only briefly considered the extent to which these issues represented constituency interests. One difficulty in his analysis concerned a choice of variables. While this paper has used variables directly related to the chance of being reelected, MacRae largely relied upon socioeconomic measures and measures similar to the measure of Rightist vote with no bias. These measures (and the one used here) fail to capture the situation where a deputy can base his reelection upon supporting minority positions on issues critical to the minority. They also fail to capture the fact that a deputy needed only a coalition of a subset of the voters and not even a majority of the voters to gain reelection.[33] Hence, opportunistic voting may be even more prevalent than shown in this essay.

Gaullists as a group nevertheless had a distinct set of political preferences. They differed sharply from, say, the M.R.P.; yet they frequently competed with the M.R.P. for the same clientele. There is no intention here to obscure such ideological differences as existed between the Gaullists and the M.R.P. These differences are important. But within these differences, there is room for maneuver, which is the basis for the intra-party cleavages that interested MacRae. This room for maneuver is undoubtedly also the origin of the gaming conclusions of Leites' succinct psychological analysis. What has been indicated here is the possibility that these cleavages in fact reflect a "gaming" or "self-oriented" situation insofar as positions are dictated by constituency characteristics. The pure Gaullists then would be those whose constituencies permitted them to be so. *Politique d'abord.*

[29] See n. 26.

[30] Purtschet, *op. cit.*, pp. 176, 180, 280, 298, 314, 328.

[31] Initially a number of deputies who *personally* favored the Paris Agreements voted against them, undoubtedly for *electoral* reasons, because they felt that the Agreements had enough votes to pass without their votes. Consequently, the Agreements were initially defeated and had to be reconsidered. See Leites, *op. cit.*, pp. 53–54.

[32] See n. 26.

[33] A manifestation of subset tactics is witnessed by the finding that on roll-call votes deputies from different parties in a given district "tended to take positions inversely related to one another." MacRae, *op. cit.*, p. 289.

MAJORITY DECISION-MAKING WITH PARTIAL UNIDIMENSIONALITY*

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A major dilemma for majority decision-making occurs when the summation of transitive individual preference orderings results in an intransitive social ordering. The problems posed by this phenomenon, which is known as the paradox of voting, can be seen in the following standard example.

Suppose there are three individuals, one with each of the following preference orders of three alternatives: *ABC, BCA, CAB.* Under majority rule, *A* would defeat *B, B* would defeat *C,* and *C* would defeat *A,* so there is no majority winner.

Most voting procedures, of course, yield a unique result whether or not the paradox occurs. But from this example it is apparent that when the paradox does occur, a majority of the voters prefer an alternative other than the one which is selected. Moreover, if a typical voting procedure is used, which of the alternatives is selected depends on the order in which the alternatives are voted on.[1] Clearly these results have important implications, whether one is concerned with normative questions about majority rule or with the practical politics of legislative decision-making.

In the burgeoning literature on the voting paradox, surely one of the most impressive and well-known findings is Black's and Arrow's demonstration that the paradox cannot occur if the set of individual preference orderings is single-peaked.[2] Since single-peakedness implies that the individuals and alternatives can be arrayed on a single dimension, their finding has a meaningful substantive interpretation. Namely, complete agreement on a dimension for judging the alternatives ensures that majority voting will yield a transitive social ordering of the alternatives.[3]

Note, however, that the preference ordering of *every* individual must be single-peaked for Black's and Arrow's result to hold. Empirically this is most unlikely to happen. Even excluding multidimensional judgments, other factors such as lack of information about, misperception of, and similarity of alternatives as well as simply error make it almost certain that one or more preference orderings will not meet the necessary conditions. Hence, for all its elegance, Black's and Arrow's finding is of limited value in explaining the frequency of occurrence of the paradox.[4]

Intuitively it is reasonable to suppose that the theorems of Black and Arrow can be refined by thinking of the paradox in probabilistic rather than categorical terms. For example, while we know that the paradox *can* occur if one out of 999 preference orderings is not single-peaked, it seems very unlikely that it will in fact

* I would like to thank Herbert Weisberg, whose continued collaboration helped stimulate this research. I also wish to thank James Callan for technical help. The University of Rochester Computing Center provided computer time.

[1] Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1958), pp. 39–40.

[2] Black, *op. cit.*, Chap. IV; Kenneth J. Arrow, *Social Choice and Individual Values,* 2nd ed. (New York: Wiley, 1963), pp. 74–80. A set of preference orderings is single-peaked if there is an ordering of the alternatives on the abscissa such that when utility or degree of preference is indicated by the ordinate, each preference ordering can be represented by a curve which changes its direction at most once, from up to down (i.e. has at most one peak).

[3] For most political scientists, the major work relying on the assumption of unidimensionality is Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957). In Downs' spatial analysis voters and parties unanimously view the electoral struggle in terms of a single ideological dimension. They do not, of course, agree on which party is best, but they do share a common dimension or continuum on which to judge the alternatives. Downs' work has often been criticized because unidimensionality is assumed. In a sense, I am making the same criticism of one aspect of Black's and Arrow's work, but I am also proceeding (a little way) with the analysis after dropping the criticized assumption.

[4] More general criteria than single-peakedness will also prevent the occurrence of the paradox. However, as with single-peakedness, every individual's preference ordering must satisfy the criteria. In addition, the more general conditions lack a meaningful substantive interpretation. See Amartya K. Sen, "A Possibility Theorem on Majority Decisions," *Econometrica,* 34 (1966), 491–499, and the references cited therein.

occur. At least it seems less likely than if 50 or 150 or 250 of the rank orders are not single-peaked. Indeed, the likelihood of the paradox would seem to increase as the proportion of single-peaked preference orderings decreases.

We are led to ask, then, what the probability of the paradox is, given that a certain proportion of the rank orders satisfy single-peaked criteria. The answer to this question for the three-alternative case provides the basis for this paper. The findings will suggest, first of all, why the paradox does not occur in many social situations. Secondly, they will suggest the type of situation in which the paradox is most likely to occur in reality. The discussion is limited to the three-alternative case because the calculations for n-alternatives have thus far proved intractable. Extension of the general ideas to the n-alternative case can probably be made.

The methods used here were developed by the application of Coombs' unfolding technique.[5] Although they could be translated into the more familiar language of single-peaked preference orderings, it seems profitable to introduce Coombs' ideas and techniques to researchers interested in the voting paradox and in collective choice more generally. Not only do they provide more convenient procedures for analyzing rank order data; they also ground these procedures in a psychological theory of preferential choice. A brief summary of the most relevant aspects of Coombs' work is provided before our own findings are displayed.

Unfolding Technique. The unfolding technique is a procedure for determining from a set of individual preference orderings the dimension(s) on which the choices were based. The basic assumptions of the theory of preferential choice underlying the technique are as follows. Each individual and each alternative (called stimuli in the original exposition) may be represented by a point in n-dimensional space. The "individual points," called ideal points, represent the best possible alternative from the point of view of each individual. Each individual's preference ordering of the alternatives (stimuli) from most to least preferred is given by the rank order of the absolute distances of the stimulus points from his ideal point, the nearest being the most preferred. For convenience, the preference orderings are called *I scales.*

A special situation, the unidimensional case, exists when all individuals and alternatives can be represented on a single dimension, called a *J scale.*[6] This is obviously a highly restrictive case, but it is of crucial importance in considering problems of majority decision. This is because there is a precise correspondence between the criteria of unidimensionality and single-peakedness. That is, single-peaked curves "correspond in unfolding theory to the case in which the preference orderings of the individuals all unfold into a common qualitative *J* scale." Thus Black's and Arrow's theorems can be stated in terms of unfolding theory. In Coombs' words, "If a set of individual preference orderings satisfies a common qualitative *J* scale, . . . majority decision necessarily yields a transitive ordering of alternatives."[7]

Using the unfolding technique, one can readily determine whether a common *J* scale exists, and if so, what the order of the alternatives is on this dimension. For a variety of reasons noted above, such a solution seldom exists. In this situation one can find instead the *dominant J* scale, that is, the ordering of the alternatives along a single dimension which satisfies the largest proportion of the preference orderings. The probability of the voting paradox as this proportion varies is what we now wish to find.

THE CONDITIONAL PROBABILITY OF THE PARADOX OF VOTING

Let us begin by making explicit the assumptions on which our analysis is based. These conditions are taken, with slight changes in notation, from an earlier publication.[8] The assumptions are similar to those found in most work on the paradox, and they do simplify the analysis. However, they are not inherent limitations in the analysis below, and an effort to relax them might be profitable.

Consider three alternatives, A, B, C. Let m individuals rank the alternatives. We assume the following:

I. For all pairs of alternatives such as A and B, $A \succ B$ (read: A is ranked above B) or $B \succ A$; that is indifference between alternatives is not permitted.

[5] Clyde H. Coombs, *A Theory of Data* (New York: Wiley, 1964), Chaps. 5–7.

[6] Coombs distinguishes a *quantitative J* scale, on which the spacing as well as the order of alternatives is determined, from a *qualitative J* scale, on which the order of alternatives but not their spacing has been determined. We will only be concerned with the latter type.

[7] Coombs, *op. cit.*, p. 395.

[8] Richard G. Niemi and Herbert F. Weisberg, "A Mathematical Solution for the Probability of the Paradox of Voting," *Behavioral Science*, 13 (1968), 317–323.

II. For each individual the rank order is transitive; e.g., if $A \succ B$, and $B \succ C$, then $A \succ C$.

III. In voting on a pair of alternatives, each individual votes for the alternative which stands higher in his rank order.

A social ordering is determined by simple majority voting between all pairs of alternatives. In order to avoid ties in the social ordering, we also assume that:

IV. The number of individuals is odd.

Individual preference orderings are indicated by a series of letters. For example, if $A \succ B$, $B \succ C$ (and $A \succ C$), this is indicated simply as ABC.

In order to determine the probability of the paradox given that a certain proportion of the individuals have preference orderings (I scales) satisfying a common J scale, it is relevant to ask first whether that proportion can vary over the entire range of 0% to 100%. Are there instances in which the preference orderings of no more than 10%, 25% or 50% of the individuals can be derived from a common dimension? It turns out that with three alternatives, the preference orderings of at least $[2m/3]$ of the individuals necessarily satisfy a common qualitative J scale, where $[2m/3]$ is the smallest integer $\geq 2m/3$. For example, if 99 individuals each rank three alternatives, at least 66 of their rank orders are satisfied by a common J scale—no matter how the orderings were arrived at! This can be demonstrated as follows.[9]

With three alternatives there are $3! = 6$ rank orders: ABC, ACB, BAC, BCA, CAB, CBA. Any of these six orderings constitutes a J scale. However, three of the orderings are mirror images of the others and will therefore satisfy the same rank orders. Hence there are only three distinct J scales: (ABC), (BCA), and (CAB), where the parentheses indicate that either end of the ordering may be considered the "left" end. Each J scale satisfies four of the six I scales, as shown below:

J scale	(ABC)	(BCA)	(CAB)
I scales satisfying the J scale	ABC	BCA	CAB
	BAC	CBA	ACB
	BCA	CAB	ABC
	CBA	ACB	BAC

Note that pairs of I scales—ABC and BAC, BCA and CBA, CAB and ACB—always occur together. If one of the pair is satisfied by a J scale, the other is too. Here we need not distinguish individual members of the pair. Essentially, then, we have only three sets of I scales, as follows:

J scale	(ABC)	(BCA)	(CAB)
Sets of I scales satisfying the J scale	I	II	III
	II	III	I

where Set I $= (ABC, BAC)$, Set II $= (BCA, CBA)$, and Set III $= (CAB, ACB)$.

It is apparent that any two of the three sets of I scales satisfy a common J scale. Thus to find the dimension satisfying the largest number of individual preference orderings, we simply identify the J scale which satisfies the sets containing the largest and second largest numbers of individuals. (The proportion of the m individuals having I scales in either of the two largest sets will be denoted by x/m. In other words, x/m is the *maximum* proportion of individuals whose preference orderings satisfy a common J scale.) These two sets will always contain the preference orderings of at least 2/3 of the individuals. This follows immediately from the partition of m individuals into three disjoint sets. The smallest set(s) can never have more than 1/3 of the individuals, so that the two remaining sets have at least 2/3 of them. In fact, exactly 2/3 of the preference orderings satisfy a common J scale only if m is a multiple of three, and each set contains exactly $m/3$ individuals.

Our task now is to find the probability of the voting paradox given that x/m preference orderings satisfy a common J scale, where $x/m \geq 2/3$. In order to do this, some assumption must be made about the initial probabilities of each rank order. Unfortunately, the final result—the probability of the paradox—varies from 0 to 1 as the underlying assumptions are varied. However, almost all previous work on the paradox has been based on the "equally-likely" assumption, and as a didactic device it is quite useful. In addition, it allows a direct comparison of our present results with total probabilities calculated earlier.[10]

[9] In general, given m alternatives, the minimum proportion of I scales satisfying a common qualitative J scale is $[2^{n-1}/n!]$. To show this requires a more general proof, which is not useful for the present analysis.

[10] Probabilities of the paradox for various numbers of individuals and alternatives are given in Colin D. Campbell and Gordon Tullock, "A Measure of the Importance of Cyclical Majorities," *The Economic Journal*, 75 (1965), 853–856;

The equally-likely assumption normally specifies that the probability of each rank order is equal, or one-sixth in the three-alternative case. Here, however, we are dealing with two disjoint groups of individuals—x with preference orderings satisfied by a common J scale and m-x with preference orderings not satisfied by that J scale. Hence, for calculating conditional probabilities, the equally-likely assumption must be modified to mean that $p=1/4$ for each of the four I scales which satisfy the common J scale and $p=1/2$ for each of the remaining two I scales.[11]

A computer program was written by Herbert Weisberg and the author to calculate the desired values. For a given m, the program generated all possible distributions of individuals among the six possible rank orders. For each distribution the program determined: 1) the probability of the distribution based on the equally-likely assumption (as normally interpreted), 2) whether the paradox occurred, 3) how many individuals' preference orderings satisfied a common J scale (x/m). With this information it was possible to determine directly the probability of the paradox for the given number of individuals, the probability that x/m preference orderings satisfy a common J scale, and (assuming the modified equally-likely assumption) the probability of the paradox given that x/m preference orderings satisfy a common J scale.

By generating every distribution of m individuals among the six rank orders, this program used the most direct approach possible. However, since the number of distributions of m individuals among six rank orders increases extremely rapidly as m increases, the program is limited practically by the amount of computational time required; it is used here for

TABLE 1. CONDITIONAL PROBABILITIES OF THE PARADOX OF VOTING, WITH THREE ALTERNATIVES AND EQUALLY-LIKELY RANK ORDERS

Number of Individuals	Number of Individuals whose Preference Orderings Satisfy a Common Qualitative J Scale																				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1	0*																				
3	—**	.250	0																		
5	—	—	—	.112	0																
7	—	—	—	—	.188	.039	0														
9	—	—	—	—	—	.250	.102	.017	0												
11	—	—	—	—	—	—	—	.168	.045	.008	0										
13	—	—	—	—	—	—	—	—	.214	.089	.022	.003	0								
15	—	—	—	—	—	—	—	—	—	.250	.142	.045	.012	.002	0						
17	—	—	—	—	—	—	—	—	—	—	—	.192	.077	.024	.006	.001	0				
19	—	—	—	—	—	—	—	—	—	—	—	—	.226	.119	.042	.013	.003	.000+	0		
21	—	—	—	—	—	—	—	—	—	—	—	—	—	.250	.165	.068	.024	.007	.001	.000+	0

* The entries are the probabilities of the paradox of voting under the given conditions.
** These conditions cannot hold, since it is shown in the text that at least $[2m/3]$ of the I scales always satisfy a common qualitative J scale.

Frank Demeyer, and Charles R. Plott, "The Probability of a Cyclical Majority," *Econometrica*, forthcoming; Mark B. Garman, and Morton I. Kamien, "The Paradox of Voting: Probability Calculations," *Behavioral Science* 13 (1968), 306–316; David Klahr, "A Computer Simulation of the Paradox of Voting," this REVIEW, 60 (1966), 384–390; Niemi and Weisberg, *op. cit.*; John E. Pomeranz and Roman L. Weil, Jr., "Calculation of Cyclical Majority Probabilities," unpublished paper, University of Chicago.

[11] In our earlier paper, questions were raised about the interpretation of the initial probability assumptions. These questions are fully applicable here. See Niemi and Weisberg, *op. cit.*, fn. 6. It should be added that instances in which the paradox is contrived are not adequately accounted for by a probabilistic model.

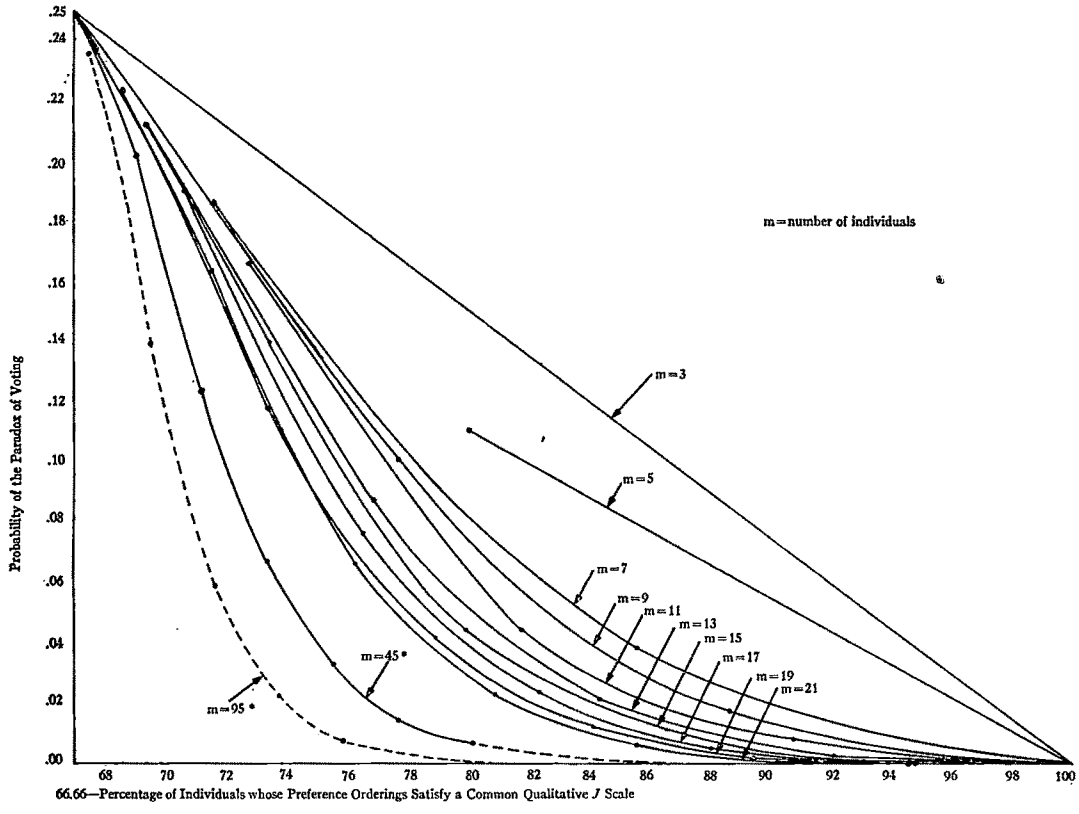


FIG. 1. Probabilities of the Paradox of Voting by Percentage of Individuals whose preference orderings satisfy a common J scale

*For m=45, actual probabilities were calculated only until 36 (80%) preference orderings satisfied a common J scale, after this point a dotted line is used. For m=95, probabilities were calculated for 64, 66, 68, 70 and 72 preference orderings satisfying a common J scale as explained in the Appendix.

only up to 21 individuals. To overcome this limitation, another program was written to provide the supplementary calculations described below.

The conditional probabilities of the paradox of voting, based on the assumption of equally-likely rank orders, are given in Table 1 for $m=1$ to $m=21$. (The conditional probabilities for larger m will be considered presently.) It is apparent, first of all, that with three alternatives and the equally-likely assumption, the conditional probability of the voting paradox is never more than .25. And that probability obtains only when the lowest possible proportion of individuals have preference orderings satisfying a common J scale. It is also obvious that for a fixed number of individuals, the probability of the paradox decreases monotonically as the proportion of preference orderings satisfying a common J scale increases. The rate of decrease is high at first, and then is very low as perfect unidimensionality is approached. The 0's along the diagonal indicate, of course, that the paradox cannot occur if all of the preference orderings satisfy a common dimension.

Figure 1 shows the conditional probabilities of the paradox graphed against the proportion of preference orderings satisfying a common J scale. The smooth curves are drawn to identify the points associated with each value of m. With small m, of course, only a few percentages have corresponding integer fractions and only those points can be "translated" into numbers of individuals. As m increases the points for a given value of m approximate a smooth curve. Thus the curves which are drawn in suggest the general form which the probability values take for larger m.

The probability values for $m=45$ and $m=95$

were calculated by a computer program written for this purpose. The procedure on which it is based is described in detail in the Appendix. We also show in the Appendix that as m approaches infinity, the probability of the paradox is .25 when exactly 2/3 of the preference orderings satisfy a common J scale; for any percentage larger than 2/3, the probability of the paradox approaches as close to 0 as desired (except for 100%, where the probability is 0). Thus, as m approaches infinity, the curve of conditional probabilities approaches the lines $x=2/3$, $y=0$.

From the shape of the curves in Figure 1, it is clear that the probability of the paradox depends both on the proportion of individuals with preference orderings which satisfy a common J scale (x/m) and on the number of individuals involved (m). Thus we are led to an entirely unexpected conclusion: Holding constant the proportion of individuals whose preference orderings satisfy a common J scale, the probability of the paradox of voting decreases as the number of individuals increases. This means, for example, that if about 70% of the preference orderings satisfy unidimensional criteria, the occurrence of the paradox (under the given assumptions) is relatively likely if the population is small, but is very unlikely if the population is large. For all values of m, however, the probability of the paradox decreases rapidly at first, so that unless m is very small, the paradox is unlikely if even 75–80% of the preference orderings satisfy a common J scale. For very large groups of individuals, as much as 70% agreement on an underlying dimension is sufficient to make the paradox a rare occurrence.

Before discussing the implications of these findings for majority decision-making, I should indicate the relationship between these conditional probabilities and the total probabilities given in an earlier paper.[12] As noted above, we were able to calculate the probability, for $m=1$ to $m=21$, that exactly x/m of the preference orderings satisfy a common J scale, for each value of x such that $x/m \geqq 2/3$. These values are presented in Table 2. They show that by chance a relatively low proportion of the individuals are likely to have preference orderings satisfying a common J scale. Thus, while the conditional probability of the paradox is near zero for moderate to large m and 70–75% of the preference orderings satisfying a common J scale, the likelihood of getting less than this degree of unidimensionality by chance is sufficiently large so that the total probability of

[12] Niemi and Weisberg, *op. cit.*

TABLE 2. PROBABILITY THAT A GIVEN NUMBER OF INDIVIDUALS HAVE PREFERENCE ORDERINGS SATISFYING A COMMON QUALITATIVE J SCALE, WITH THREE ALTERNATIVES AND EQUALLY-LIKELY RANK ORDERS

Number of Individuals	Number of Individuals whose Preference Orderings Satisfy a Common Qualitative J Scale																				
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1	1																				
3	0	.222	.778																		
5	0	0	0	.617	.383																
7	0	0	0	0	.288	.538	.174														
9	0	0	0	0	0	.085	.499	.337	.078												
11	0	0	0	0	0	0	0	.352	.425	.188	.035										
13	0	0	0	0	0	0	0	0	.170	.426	.289	.100	.015								
15	0	0	0	0	0	0	0	0	0	.053	.358	.366	.178	.039	.007						
17	0	0	0	0	0	0	0	0	0	0	0	.247	.372	.250	.103	.026	.003				
19	0	0	0	0	0	0	0	0	0	0	0	0	.120	.343	.303	.162	.058	.013	.001		
21	0	0	0	0	0	0	0	0	0	0	0	0	0	.038	.278	.328	.218	.100	.032	.006	.001

the paradox does not vanish but actually increases slightly as m increases.[13]

IMPLICATIONS FOR MAJORITY DECISION-MAKING

We noted at the outset that simply majority voting necessarily yields a transitive social ordering if *all* of the individual preference orderings are satisfied by a common dimension. However, it was also observed that complete unidimensionality is most unlikely empirically, so that the theorem of Black and Arrow provides little help in explaining the actual frequency of occurrence of the paradox. In this paper we have shown—for three alternatives and equally-likely rank orders—that even a moderate degree of unidimensionality makes it unlikely that the paradox will prevent a transitive majority decision. Depending on the size of the group, the paradox will infrequently occur if only 75% or 70% or even fewer of the individuals adopt a common standard (dimension) of judgment.

The present findings, then, highlight the role of all those factors which contribute to the widespread adoption of a common standard of judgment. Systems of political parties are perhaps the most commonly-cited factor. The two-party system is especially useful for this purpose, not only because the number of alternatives is sometimes limited to two (making the paradox impossible), but because judgments among several alternatives, such as presidential candidates within one party, are often structured along a dimension on which the parties are arrayed. Shared standards of judgment may also be fostered by socialization processes which tend to enforce a similarity of outlook. Elite socialization in particular may contribute to similar frames of reference among special subgroups of the population, such as legislators or business associates. Recruitment of individuals from homogeneous sources also encourages a uniform outlook by taking advantage of prior socialization. Discussion and other forms of interaction often structure the situation by formulating or clarifying the principal issue involved in a dispute. Similarly, interpreting particular issues in a broader context, such as the perennial conflict between the political "left" and "right," can provide a common dimension for judging otherwise isolated topics.

The important point, however, is that none of these factors, either alone or in combination, needs to accomplish the nearly impossible task of creating unanimous agreement on standards of judgment. The paradox can be very satisfactorily avoided if common frames of reference are widespread but far less than unanimous. Thus the extension in probability terms of Black's and Arrow's original theorem moves us a step closer to understanding the determinants of transitive social orderings based on majority decisions.

Our findings also give us a clue about where to look in the real world for intransitive social orderings. On the one hand, it is relatively fruitless to search for intransitivities where most individual preference orderings can be derived from a single dimension. An empirical example given by David illustrates this situation.[14] David used a simple classroom experiment to derive preference orderings of students for three alternatives dealing with federal aid to schools. Of the total of 69 students, 67 (97.1%) had preference orderings satisfying a common J scale, making the probability of an intransitivity virtually zero under the model used in this paper.

On the positive side, our findings suggest that intransitivities are most likely to occur in unstructured situations, where there are no common guidelines for judging the alternatives, or in situations involving multiple dimensions. For example, intransitivities might be found relatively often when new issues must be resolved. Until discussion creates some consensus or places new issues in the context of earlier disputes, preference orderings may reflect little agreement on the criteria for judging the alternatives. Similarly, the paradox probably occurs more often in *ad hoc* groups, which are less likely to be influenced by the socialization, selection, and discussion factors which contribute to greater uniformity of judgmental criteria. Such groups might be newly formed ones, groups which meet infrequently and irregularly, or groups which purposely bring together many diverse elements. In elections, the paradox may occur most frequently when parties are absent or when the parties vary along two or more major dimensions. Only in situations such as these is the proportion of preference orderings satisfying a common J scale likely to be small enough to allow frequent intransitivities.

Note, finally, that the paradox is most likely if unstructured situations or multi-dimensional structures can be found among small groups of

[13] One can find the total probability of the paradox (within rounding error) by multiplying the corresponding terms in Tables 1 and 2. For example, for $m=3$, $.250\ (.222)+0(.778)=.0555$.

[14] Paul T. David, "Experimental Approaches to Vote-Counting Theory in Nominating Choice," this REVIEW, 56 (1962) 673–676.

people. This does not mean, of course, that the paradox occurs most often among small groups, since agreement on standards of judgment might be reached most easily in such groups. But small groups which are plagued by lack of agreement about how to judge issues are also likely to be visited more frequently by intransitive majority decisions.

APPENDIX

As indicated in the text, the computer time required to generate all possible distributions of m individuals among six rank orders increases rapidly as m increases. Here we demonstrate and justify another routine for calculating conditional probabilities of the paradox. This method was used to calculate the probabilities given in Figure 1 for $m=45$ and $m=95$. We will also use the procedure to show the shape of the curve in Figure 1 as $m\to\infty$.

We are given that x out of m individuals have preference orderings satisfying a common J scale, where $2/3\leqq x/m\leqq 1$. The x individuals are distributed between the two sets of rank orders which satisfy the J scale. The smaller set contains no more than $x/2$ and no less than m-x individuals.[15] We can suppose, without loss of generality (see below), that the common J scale is ABC, and that Set I (defined in the text) contains at least as many preference orderings as Set II. This situation can be illustrated as follows:

All distributions of m individuals satisfying the above conditions

		Number of Individuals			
I scales satisfying the J scale	$\{ABC\}$ $\{BAC\}$	$2x-m$	$2x-m-1$	$2x-m-2$	$[x/2]$*
	$\{BCA\}$ $\{CBA\}$	$m-x$	$m-x+1$	$m-x+2\cdots x-[x/2]$	
I scales not satisfying the J scale	$\{ACB\}$ $\{CAB\}$	$m-x$	$m-x$	$m-x$	$m-x$
		(1)	(2)	(3) $\cdots$	(T)

*This means the smallest integer $\geqq x/2$.

For the individuals whose preference orderings satisfy the common J scale, we assume that $\Pr\{ABC\}=\Pr\{BAC\}=\Pr\{BCA\}=\Pr\{CBA\}=1/4$. This means, of course, that $\Pr\{\text{Set I}\}=\Pr\{\text{Set II}\}=1/2$. It also means that $\Pr\{ABC\}=\Pr\{BAC\}=1/2$ given that the individual is in Set I, and $\Pr\{BCA\}=\Pr\{CBA\}=1/2$ given that the individual is in Set II. For the individuals whose preference orderings do not satisfy the common J scale, we assume that $\Pr\{ACB\}=\Pr\{CAB\}=1/2$.

On the basis of these assumptions, we are now able to calculate, for each distribution $(1),\ldots,(T),\Pr\{A\succ B\},\Pr\{B\succ C\},\Pr\{A\succ C\}$ and their complements. For example, take distribution (1). If $A\succ B$ in the social ordering, at least $[(m+1)/2]-(m\text{-}x)$ of the individuals in Set I must have the rank order ABC. This follows immediately from observing that $(m+1)/2$ is a simple majority, that $A\succ B$ for the m-x individuals in Set III, and that ABC is the only other rank order in which $A\succ B$. Similarly, if $B\succ C$, at least $[(m+1)/2]-(2x\text{-}m)$ of the individuals in Set II must have the rank order BCA; if $A\succ C$, at least $[(m+1)/2]-(2x\text{-}m)$ of the individuals in Set III must have the rank order ACB.

Continuing the example of distribution (1), we now have $\Pr\{A\succ B\}=\Pr\{ABC\geqq[(m+1)/2]-(m\text{-}x)\}$, where the right hand side of the expression indicates the probability that at least $[(m+1)/2]-(m\text{-}x)$ individuals have rank order ABC. Similar expressions can be written for $\Pr\{B\succ C\}$ and $\Pr\{A\succ C\}$. These probabilities can be calculated using the binomial probability distribution. When available, they can be obtained directly from tables of the cumulative binomial distribution. Since $\Pr\{A\succ B\}$, $\Pr\{B\succ C\}$, and $\Pr\{A\succ C\}$ are independent of one another, we can write:

$$\begin{aligned}&\Pr\{A\succ B,B\succ C,C\succ A\}\\&\quad=\Pr\{A\succ B\}\cdot\Pr\{B\succ C\}\cdot\Pr\{C\succ A\}\\&\Pr\{A\succ C,C\succ B,B\succ A\}\\&\quad=\Pr\{A\succ C\}\cdot\Pr\{C\succ B\}\cdot\Pr\{B\succ A\}\end{aligned}\qquad(1)$$

Since these are the only possible intransitivities with three alternatives, we have now indicated how to calculate the probability of the paradox for distribution (1). Similar reasoning applies to distributions $(2),\ldots,(T)$.

[15] The smaller set obviously cannot contain more than half of the preference orderings which satisfy the common J scale. On the other hand, suppose it contained only $m-x-y$ I scales. Then the other two sets satisfy a common J scale, and they contain $m-(m-x-y)=x+y$ individuals. But this is contrary to the assumption that only x preference orderings satisfy a common J scale.

Now let us consider all possible distributions of m individuals among Sets I, II, and III (given that there are $m-x$ in one set). Every possible partition of m individuals (e.g., $m-x$ in one set, $m-x+y$ in another set, and $2x-m-y$ in the third set) is represented by one of the (T) distributions. Moreover, it can be shown that the probability of the paradox is identical when the partition of individuals into sets is the same, regardless of what the J scale is or which set is the largest. Thus we have shown how to calculate the probability of the paradox for each of the (T) partitions of m individuals into the three sets.

However, the number of distributions represented by each partition is not the same. Specifically, one distribution is represented if each set has the same number of individuals. If Set I and Set II, or Set I and Set III or Set II and Set III have equal numbers of individuals, three distributions are represented (i.e., any of the three sets can be the odd size set). If all three sets are of unequal size, six distributions are represented. Hence in order to use distributions (1), . . . , (T) to represent all possible distributions of m individuals among three sets, we need to assign a coefficient of 1, 3, or 6 to each distribution (1), . . . , (T) to denote the number of all possible distributions which it represents.

		Number of individuals	
I scales satisfying the J scale	$\begin{Bmatrix} ABC \\ BAC \end{Bmatrix}$	x	$\Pr\{A \succcurlyeq B\} = \Pr\left\{ABC \geqq \frac{3x+1}{2} - x\right\} = \Pr\left\{ABC \geqq \frac{x+1}{2}\right\}$
	$\begin{Bmatrix} BCA \\ CBA \end{Bmatrix}$	x	$\Pr\{B \succcurlyeq C\} = \Pr\left\{BCA \geqq \frac{3x+1}{2} - x\right\} = \Pr\left\{BCA \geqq \frac{x+1}{2}\right\}$
I scales not satisfying the J scale	$\begin{Bmatrix} ACB \\ CAB \end{Bmatrix}$	x	$\Pr\{A \succcurlyeq C\} = \Pr\left\{ACB \geqq \frac{3x+1}{2} - x\right\} = \Pr\left\{ACB \geqq \frac{x+1}{2}\right\}$

Now we need to calculate the probability of the (T) partitions of m individuals into Sets I, II, and III (represented by distributions (1), . . . , (T) and their coefficients). The number of individuals in one set is fixed at $m-x$. The binomial probability distribution gives us the probability of every possible distribution of the remaining x individuals into two disjoint sets. Here, however, only those distributions represented by the (T) distributions of individuals into Sets I and II are permissible. For each of these distributions, we multiply the binomial probability by the corresponding coefficient. The resulting figure is divided by the sum of the figures for all (T) distributions. This yields the probability of each of the (T) partitions of m individuals into Sets I, II, and III.

Finally, the probability of each partition must be multiplied by the probability of the paradox given that partition. Summing the result across all (T) partitions yields the probability of the paradox given that x of m individuals satisfy a common J scale.

The probabilities displayed in Figure 1 for $m=45$ and $m=95$ were calculated using the above procedure. A short computer program was written to perform the arithmetic calculations. The necessary binomial probabilities for $m=45$ were seven-digit values taken from tables compiled by the National Bureau of Standards.[16] For $m=95$, $\Pr\{A \succcurlyeq B\}$, $\Pr\{B \succcurlyeq C\}$, and $\Pr\{A \succcurlyeq C\}$ were taken from the above source. To determine the probabilities of the (T) partitions, individual terms were calculated from the five-digit values given by the Harvard University Computation Laboratory or were taken from the six-digit values given by Romig.[17] For convenience only the values indicated in the footnote to Figure 1 were used.

Conditional Probabilities as $m \to \infty$. The procedure just described can be used to show the shape of the curve of conditional probabilities as $m \to \infty$. Consider first the probability of the paradox when exactly 2/3 of the individuals have preference orderings satisfying a common dimension. Let $m=3x$, and $2x$ of the preference orderings satisfy a common J scale.[18] The situation can be illustrated as follows:

Since $\Pr\{ABC\} = 1/2$ for the individuals in Set I and x is an odd number (so that there cannot be exactly $x/2$ individuals with preference order ABC), $\Pr\{A > B\} = 1/2$; also $\Pr\{B > A\} = 1/2$. Similarly, $\Pr\{B > C\} = \Pr\{C > B\} = 1/2$ and $\Pr\{A > C\} = \Pr\{C > A\} = 1/2$. Then, $\Pr\{A$

[16] *Tables of the Binomial Probability Distribution*, Appl. Math. Series 6 (Washington: U. S. Government Printing Office, 1949).

[17] *Tables of the Cumulative Binomial Probability Distribution* (Cambridge: Harvard University Press, 1955). Harry G. Romig, *50–100 Bimonial Tables* (New York: Wiley, 1953).

[18] The proof in this paragraph—i.e. when exactly 2/3 of the preference orderings satisfy a common J scale—holds for every value of m which is a multiple of three (and an odd number).

$\succ B,\ B \succ C,\ C \succ A\} = \Pr\{A \succ C,\ C \succ B,\ B \succ A\}$ $= (1/2)^3 = 1/8$. Hence the probability of the paradox is $1/8 + 1/8 = 1/4$.

Now let us show that for a fixed proportion, x/m, of the preference orderings satisfying a common J scale, where $2/3 < x/m < 1$, the probability of the paradox approaches 0 as $m \to \infty$. We are given that x of the individuals have preference orderings satisfying a common J scale. The probability that any one of these x individuals is in Set I corresponds to a Bernoulli trial with $p = 1/2$. From the law of large numbers we know that as x (the number of trials) approaches infinity, the probability approaches 1 that the proportion of individuals in Set I differs from the probability of Set I by no more than an arbitrary number ϵ_1.[19] That is, if $\Pr\{\text{Set I}\} = 1/2$, the number of individuals in Set I will be as close to $x/2$ as we desire, for sufficiently large x. Thus, as x (and m)$\to\infty$, we can assume that the distribution of individuals is as close to the following as we desire:

		Number of individuals
I scales satisfying the J scale	ABC	$x/2$
	BAC	
	BCA	$x/2$
	CBA	
I scales not satisfying the J scale	ACB	$m-x$
	CAB	

Note that:

$$\Pr\{A \succ B\} = \Pr\left\{ABC \geqq \frac{m+1}{2} - (m-x)\right\} = \Pr\left\{ABC \geqq x - \frac{m}{2} + \frac{1}{2}\right\}. \tag{2}$$

Since the individuals who have preference ordering ABC must come from the $x/2$ individuals in Set I, we can rewrite (2) as:

$$\Pr\{A \succ B\} = \Pr\left\{\frac{ABC}{x/2} \geqq \frac{x - \frac{m}{2} + \frac{1}{2}}{x/2}\right\} = \Pr\left\{\frac{ABC}{x/2} \geqq 2 - \frac{m}{x} + \frac{1}{x}\right\},$$

and

$$\lim_{x\to\infty} \Pr\{A \succ B\} = \lim_{x\to\infty} \Pr\left\{\frac{ABC}{x/2} \geqq 2 - \frac{m}{x}\right\}.$$

Now we know that $1 < m/x < 3/2$. Let m/x take on any value in this range. Then $.5 < 2 - m/x < 1$, or $2 - m/x = .5 + \epsilon_2$, where $0 < \epsilon_2 < .5$. Applying the law of large numbers as before, we know that if $\Pr\{ABC\} = 1/2$ for the individuals in Set I and $x \to \infty$, the proportion of individuals with rank order ABC differs from $1/2$ by less than an arbitrarily small number ϵ_3. Now for any given ϵ_2, x can be made large enough that ϵ_3 is smaller than ϵ_2. Thus

$$\lim_{x\to\infty} \Pr\{A \succ B\} = \lim_{x\to\infty} \Pr\left\{\frac{ABC}{x/2} \geqq .5 + \epsilon_2\right\} = 0.$$

Using the same line of reasoning we have:

$$\Pr\{B \succ C\} = \Pr\left\{BCA \geqq \frac{m+1}{2} - \frac{x}{2}\right\} = \Pr\left\{\frac{BCA}{x/2} \geqq \frac{\frac{m-x+1}{2}}{x/2}\right\} = \Pr\left\{\frac{BCA}{x/2} \geqq \frac{m}{x} - 1 + \frac{1}{x}\right\}.$$

Also,

$$\lim_{x\to\infty} \Pr\{B \succ C\} = \lim_{x\to\infty} \Pr\left\{\frac{BCA}{x/2} \geqq \frac{m}{x} - 1\right\}.$$

The range of m/x is the same as before. For any value of m/x in this range, $0 < m/x - 1 < .5$, or $m/x - 1 = .5 - \epsilon_4$, where $0 < \epsilon_4 < .5$. By the same reasoning as above we have:

$$\lim_{x\to\infty} \Pr\{B \succ C\} = \lim_{x\to\infty} \Pr\left\{\frac{BCA}{x/2} \geqq .5 - \epsilon_4\right\} = 1$$

It follows that

$$\lim_{x\to\infty} \Pr\{C \succ B\} = 0.$$

We have now shown that in the limit, one factor of both equations of (1) approaches zero. Thus, for all values of x/m such that $2/3 < x/m < 1$, the probability of the paradox approaches zero as x (and m)$\to\infty$.

[19] William Feller, *An Introduction to Probability Theory and Its Applications*, 2nd ed., Vol. 1 (New York: Wiley, 1957), p. 141.

PRESIDENTIAL ELECTIONS: AN EXPLANATION OF VOTING DEFECTION*

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This paper proposes and tests a modest theory of voting defection, the act of voting contrary to party identification. The relevance of voting defection to popular control of government is clear. Except for the infrequent elections that Key calls "critical,"[1] the identification of people with their parties is very stable over time. The percentage of Democrats and Republicans in the electorate changed only slightly in the four presidential elections from 1952 to 1964.[2] Short term shifts in public attitudes, then, are reflected not in shifts in the distribution of party identification, but in the degree that people vote in accordance with their identification. When they are disenchanted with the President, defection will be high among members of the opposition party and low among members of the party in office. In 1952 people were weary of the Korean War; this weariness was apparent in the massive numbers of Democrats who thought Eisenhower a man capable of ending the conflict and who backed up their convictions with Republican votes.[3] In short, in the rate of defection we have a mirror that reflects popular discontent with the politics of the President. My present concern is to explore some personal attributes of the voters who make up this critical electorate, to augment the propositions surrounding party identification with one explanation of why it is that people vote contrary to their party allegiance.

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[1] V. O. Key, Jr., "A Theory of Critical Elections," *Journal of Politics,* 17 (Feb., 1955), 3–18. The notion of critical elections has been broadened to include a whole typology of elections. See Angus Campbell, "A Classification of the Presidential Elections," in Angus Campbell *et al., Elections and the Political Order* (New York: John Wiley and Sons, Inc., 1966), pp. 63–77.

[2] Philip Converse, "The Concept of a Normal Vote," in Campbell, *ibid.,* p. 13.

[3] The images of the parties and candidates in the 1952 election are discussed in Angus Campbell, Gerald Gurin, and Warren E. Miller, *The Voter Decides* (Evanston: Row, Peterson and Co., 1954), pp. 41–68.

I. POLITICAL ORIENTATIONS AND CROSS-PRESSURES

In a theory of electoral behavior, we would expect to give a large place to political attitudes. In terms of the immediacy of their impact upon behavior, political attitudes lie close to the voting act. Variables farther back in the causal network relate to voting primarily through the intervening variables of political attitudes. Thus, these attitudes have a high predictive quality.[4]

In *The Voter Decides,* the first major Michigan study, political attitudes are organized under three main heads: attitudes toward parties, candidates, and policies. These factors are explored as they affect both participation at the polls and choice of candidates.[5] The later Michigan studies have not preserved the simplicity of *The Voter Decides'* trichotomy. Rather, a variety of political attitudes are grouped together. One cluster of six attitudes is consistently associated with electoral choice.[6]

But the factors which affect an individual's vote do not always impel him in the same direction. Frequently, these forces are in conflict. From this observation comes the concept of cross-pressures, formulated originally by Lazarsfeld, Berelson, and Gaudet in their study of the 1940 Presidential campaign.[7] The Lazarsfeld

[4] This argument, of course, is the one made in Angus Campbell *et al., The American Voter* (New York: John Wiley and Sons, Inc., 1960), chapter 2.

[5] Campbell, *The Voter Decides.*

[6] Since the 1952 election the multiple correlations of these predictors have varied between .72 and .75. See Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review,* 60 (March, 1966), 19–28.

[7] Paul Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (2nd ed.; New York: Columbia University Press, 1948), p. 56. For a discussion of cross-pressures that places the concept into balance theory, see Ithiel de

study uses cross-pressures primarily as an explanation of the time of voting decision during the campaign and of the level of political interest.[8] In *Voting,* the impact of cross-pressures is extended to the stability of vote intention during the campaign, its relation to the selective perception of political events, and to participation at the polls.[9] A major concern of *The Voter Decides* is cross-pressures—the consistency and conflict of political orientations and their impact on candidate preference and participation.[10]

With the development of the concept of party identification in the Michigan studies, we also see the impact of cross-pressures on voting in accordance with party affiliation. Indeed, cross-pressure remains one of the few concepts used to explain voting defection.

> . . . the influence of party identification on perceptions of political objects is so great that only rarely will the individual develop a set of attitude forces that conflicts with this allegiance. But when other antecedent factors lead to evaluations of the elements of politics that strongly contradict the individual's party identification, we anticipate that behavior will conform to these evaluations rather than to party allegiance.[11]

That people develop political attitudes which conflict with party identification should not startle us. Party identification is usually acquired early in life—well before more specific attitudes about policies or candidates.[12] Further, this identification is usually the most enduring of our political beliefs. Our more specific political values are vulnerable to change, to the influence of new peer groups created by social or geographical mobility. At some point the estrangement of political beliefs from party allegiance may become so great that consistency is regained by switching party allegiance. We can get a glimpse of this process in the South today. But even in the South, where mobility and the revolution in civil rights are straining traditional ties, identification with the Democratic Party remains strong.[13]

Following *The Voter Decides,* political orientations are here subsumed under three inclusive heads: images of parties, images of candidates, and policy beliefs. Each of a voter's attitudinal orientations is measured in terms of its compatibility with his party identification. For example, a pro-Republican candidate preference combined with a pro-Democratic party identification is classified as low candidate compatibility. Low compatibility is the adopted definition of cross-pressures. Similarly, indices of Party Compatibility and Policy Compatibility are constructed for each identifier.[14] The following hypothesis is to be tested.

H 1. Taken singly, each index of compatibility relates monotonically to voting defection—the higher the compatibility, the less the likelihood of defection.

The question remains as to how these indices will interact statistically in their effect upon voting defection.[15] It seems likely that there will be a tendency for attitudes that move the voter in the same direction to reinforce one another.[16] For example, if a voter has unfavorable views toward both his party's candidate and the policies he associates with his party, he may defect in rates above what one would predict from simply summing the independent effects of each of his political attitudes. Thus, the following hypothesis.

H 2. Taken together, certain combinations of the indices are mutually reinforcing. Voters whose indices consistently reveal high compatibility with party identification defect less than an additive model would predict. Conversely, voters with consistently incompati-

Sola Pool, Robert Abelson, and Samuel Popkin, *Candidates, Issues, and Strategies: A Computer Simulation of the 1960 and 1964 Presidential Elections* (Rev. ed.; Cambridge: The M.I.T. Press, 1965), pp. 9–15.

[8] Lazarsfeld, *op. cit.,* pp. 52–64.

[9] Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: The University of Chicago Press, 1954), pp. 129–32, 230–31, and 283–84.

[10] Campbell, *The Voter Decides,* p. 87.

[11] Campbell, *The American Voter,* p. 141. The prediction is successfully tested, p. 142.

[12] Herbert H. Hyman, *Political Socialization* (Glencoe: The Free Press, 1959), p. 46.

[13] Philip Converse, "On the Possibility of Major Political Realignment in the South," in Campbell, *Elections and the Political Order,* pp. 212–244.

[14] A section on specific measurement procedures follows.

[15] Statistical interaction is a change in the relationship between two variables over a range of a third. If the relationship is constant, it is said to be additive. See the Appendix for a lengthy discussion of the meaning of interaction and a method of measuring it.

[16] For an explicit statement of the proposition of interaction among attitudes, see Milton Rokeach and Gilbert Rothman, "The Principle of Belief Congruence and the Congruity Principle as Models of Cognitive Interaction," *Psychological Review,* 72 (Jan., 1965), 128–142.

ble indices defect more than an additive model would predict. Inconsistent indices combine additively.

II. STATUS INCONSISTENCY AND MOBILITY

We have to this point discussed the consequences of political orientations and cross-pressures for voting defection. Working back into the theory, we would like explanations of these intermediate variables. How, in other words, do orientations become incompatible with party identification? For possible answers, we explore status inconsistency and mobility.

Much has been made in recent years of the implications of a multi-dimensional status structure. A man's position in the eyes of others may be determined by several of his characteristics: his ethnic background, income, education, and occupation. Often he moves up and down his individual status ladders at different rates. For some the result is a high position on one status dimension and, concomitantly, a low position on another. This condition is called variously status inconsistency, status discrepancy, and low status crystallization. The effects of status inconsistency may be conceptualized as interaction: certain combinations of status positions affect other variables in ways not predictable by a knowledge of the positions viewed independently.[17]

Lipset and Bendix have theorized on the impact of status inconsistency upon cross-pressures. They state that "The mobile individual, who is in many ways a marginal man, retaining old ties and experiences, is more likely to be subjected to cross-pressure than the nonmobile person."[18] Here, they are speaking of the status discrepant individual. One such tie to the past may be party identification, a loyalty increasingly strained by attitudes formed in new situations.

The relationship of inconsistency to cross-pressures is supported by other literature. Several studies link status inconsistency with political attitudes of the left and the right. Lenski and, to a lesser extent, Kelly and Chambliss present evidence of an association of discrepancy with economic or welfare liberalism.[19] Goffman finds inconsistency associated with a desire for a change in the distribution of power in American society.[20]

Other research binds discrepancy to right-wing extremism. The notion of status politics, the subject of Daniel Bell and others in *The Radical Right,* is grounded to status inconsistency.[21] The research of Rush supports the radical right hypothesis.[22] Kelley and Chambliss reveal associations between inconsistency and unfavorable attitudes toward civil rights and civil liberties.

In short, status inconsistency is associated with extreme attitudes of the right and the left, attitudes which can come into conflict with durable party loyalties. This conflict should be manifest especially in the degree to which a voter's party identification is compatible with his image of the parties and with the policies he associates with them.

H 3. Status inconsistency increases political cross-pressures above the level an additive model would predict.

The same rationale which argues that status inconsistency produces cross-pressures applies to mobility as well. One might reasonably expect inter-generational mobility to be related to cross-pressures. Given the tendency to inherit party affiliation from parents and given the durability of party affiliation, one would predict the grown son of a Democratic skilled worker to be a Democrat also. If, however, the son were a professional man, his association with peer groups likely to hold Republican attitudes could create cross-pressures for the professional man. In statistical terminology, here again is an argument for the existence of interaction. When a

[17] This does not mean that empirically one's method of analysis will always reveal an inconsistency effect as statistical interaction. The Appendix discusses the method of analysis.

[18] Seymour Martin Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* (Berkeley: University of California Press, 1964), p. 69.

[19] Gerhard Lenski, "Status Crystallization: A Non-Vertical Dimension of Social Status," *American Sociological Review,* 19 (Aug., 1954), 405–413. See also Gerhard Lenski, "Status Consistency and the Vote: A Four Nation Test," *American Sociological Review,* 32 (April, 1967), 298–301. K. Dennis Kelly and William J. Chambliss, "Status Consistency and Political Attitudes," *American Sociological Review,* 31 (June, 1966), 375–382. For a dissenting view, see William F. Kenkel, "The Relationship Between Status Consistency and Politico-Economic Attitudes," *American Sociological Review,* 21 (June, 1956), 365–368.

[20] Irwin W. Goffman, "Status Consistency and Preference for Change in Power Distribution," *American Sociological Review* 22 (June, 1957), 275–281.

[21] Daniel Bell (ed.), *The Radical Right* (Anchor Books ed.; Garden City: Doubleday and Co., Inc., 1964), pp. 39–61, 63–80, and 259–312.

[22] Gary B. Rush, "Status Consistency and Right-Wing Extremism," *American Sociological Review,* 32 (Feb., 1967), 86–92.

person occupies a very different position in the status game than his father did, he may suffer a level of political cross-pressures substantially above what we would predict from a knowledge of the separate effects of the status levels of the father and son.[23] In fact, the sole impact of generational statuses is likely to be interactive, for there is little reason to suspect an additive relationship between the statuses and cross-pressures.

H 4. Inter-generational mobility increases political cross-pressures above the level an additive model would predict.[24]

Finally, geographical mobility may tear at the ties of a man to old loyalties. If a man grew up in, say, a predominantly Democratic area and later moved to a predominantly Republican area, his exposure to the political attitudes of new peer groups might place strains upon the party loyalty he had previously acquired.

H 5. Geographical mobility increases political cross-pressures above the level an additive model would predict.

III. MEASURES OF THE VARIABLES AND SOURCE OF THE DATA

Space does not permit an elaborate description and evaluation of the measures of the concepts previously defined. For that the reader is referred to the longer work from which this article is taken.[25] However, a brief explanation of the more complicated measures is in order. We may begin with voting defection.

It seems obvious that the defection of a strong partisan is of greater significance than that of a weak identifier. A measure of defection should reflect differences in the sum of the forces necessary to push a man to vote contrary to his party identification. This sum is gauged in the following manner. Since the 1952 election, the Survey Research Center has asked a standard set of questions designed to measure a man's long term attitudinal attachment to his party. The questions yield an index ranging from Strong Democrat to Strong Republican. To create a measure of defection, the average percentage of defections over the three elections since 1956 may be used as an indicator of the strength of the forces necessary to pull a voter away from his party. The formula $D = 2(P - 50)$ estimates this force, where D is the defection score and P is the average categoric percentage of non-defectors.[26] For example, if a Strong Democrat defects, he is given a score of 80, because the average percentage of Demo-

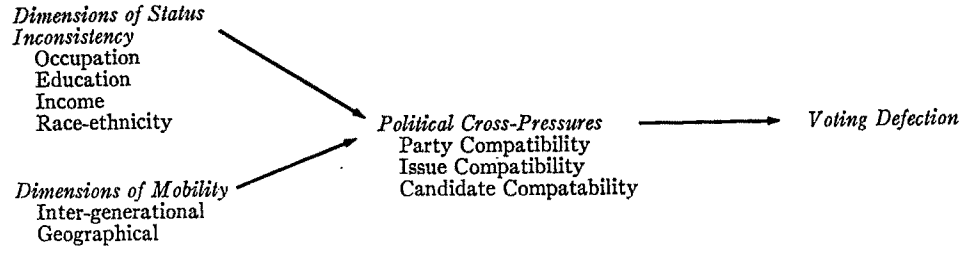
[23] For example, Blau notes that mobility affects a whole range of behavior, including voting. ". . . the upwardly mobile are more likely to vote Republican than people who have remained workers and less likely to do so than those who have originated in the middle class," Peter M. Blau, "Social Mobility and Interpersonal Relations," *American Sociological Review,* 21 (June, 1956), 291. Presuming that there is not a commensurate change in the disposition among the mobile to switch party identification, then the mobile would be more likely to be numbered among the defectors that the non-mobile.

[24] Analogously, to the degree that a man gains or loses in the status race during his mature years, attitudes formed in one social situation are likely to conflict with attitudes formed in another. Thus, if adequate measures were available in the SRC data, we would also test the hypothesis that intra-generational mobility increases cross-pressures.

[25] Richard W. Boyd, *A Theory of Voting Defection: Attitudinal Cross-Pressures and Political Alienation* (Unpublished Ph.D. Dissertation, Department of Government, Indiana University, 1967), pp. 35–43.

[26] The three elections are chosen because they are recent and because they cover the elections which serve as the tests of the hypotheses. The debt that this measure of voting defection owes to the concept of the normal vote should be obvious. See Converse, "The Concept of a Normal Vote," *op. cit.* In brief, the rationale for the averaging procedure is the assumption that across elections the balance of partisan forces changes, not the pressure required to make a voter defect. What is being assumed, then, is that individuals have threshold points beyond which they cannot resist partisan forces counter to their identification. Furthermore, all men within a given category of identifiers have approximately the same threshold point; i.e., they have a similar degree of allegiance to their party. Different percentages of defectors over different elections are assumed to reflect changes in the strength of the attacks that partisan forces mount against men's threshold points. While men may change their identification, the threshold remains the same—for the category which they leave and for the one into which they move. Within any given category the reason some defect while others do not is simply that some individuals are exposed to more intense partisan forces running counter to their identification than are others. Using the average of the three elections for the measure keeps this threshold point constant and allows the partisan forces of the day to change.
The formula $D = 2(P - 50)$ is itself somewhat arbitrary. The multiplication by 2 allows the theoretical range of the variable to extend from 0 to 1. The multiplication has no effect on the analysis.

FIG. 1. The Presumed Causal Order of the Concepts in the Theory



cratic votes among Strong Democrats is 90 percent. If he does not defect, his score is zero. A Weak Democratic defector receives a lesser score of 44. Republican identifiers are similarly scored. Independents and non-voters are excluded definitionally from the analysis.[27]

The Index of Party Compatibility is adapted from one used by Matthews and Prothro.[28] As many as five responses are coded to each of the questions, "Is there anything in particular that you (don't like) (like) about the (Democratic) (Republican) party?" A maximum of 20 responses, then, is coded for the set. Each favorable remark about the Democratic party or unfavorable remark about the Republican party is scored +1. Each favorable remark about the Republican party or unfavorable remark about the Democratic party is scored −1. To determine the degree of compatibility with party identification, the total score of each Republican is multiplied by −1. Eleven is added to all scores to yield a positive index ranging from 1 to 21. Similarly, Indices of Candidate Compatibility and Policy Compatibility are created from the respondents' views on the candidates and the issues. The issues selected (9 in 1956 and 1960, 10 in 1964) cover the three fields of domestic, foreign, and civil rights policy.[29] The issue index measures only those policy beliefs that are perceived by each respondent to have partisan implications.

There is no need to discuss the measures of status and mobility at length here, for such a description appears in the appendix. Each measurement is straightforward. All four of the dimensions of status that appear in the literature on status inconsistency are included. These are occupational prestige, racial-ethnic prestige, income, and education.

The hypotheses are tested on Survey Research Center data obtained from the Inter-University Consortium for Political Research. This secondary analysis includes the three Presidential elections of 1956, 1960, and 1964. Thus, the hypotheses are exposed to a set of elections which differ in outcome and political environment. If uniformity is found over these three elections, we can have substantial confidence in the generality of these propositions.

IV. THE TESTS OF THE HYPOTHESES

The first hypothesis makes a statement about the effects of cross-pressures upon voting defection. Again, cross-pressures are defined in terms of the degree to which a man's attitudes toward the parties, the candidates, and the policies he associates with the parties are compatible with his party identification. Low scores on any of the three indices of compatibility are said to reflect the voter's state of political cross-pressure.

H 1. Taken singly, each index of compatibility relates monotonically to voting defection—the higher the compatibility, the less the likelihood of defection.

The data amply support the first hypothesis. Table 1 lists the mean voting defection scores for each category of each index of compatibility.

As Table 1 shows, the defection rate drops off sharply as one moves from the categories of low compatibility with party affiliation to those of high compatibility with party affiliation. This is true for every election for each of the party, candidate, and policy indices.

[27] Independent Republicans and Independent Democrats are excluded along with Independents.

[28] Donald R. Matthews and James W. Prothro, "The Concept of Party Image and its Importance for the Southern Electorate," in M. Kent Jennings and L. Harmon Zeigler (eds.), *The Electoral Process* (Englewood Cliffs: Prentice-Hall, Inc., 1966), p. 159.

[29] It does not matter whether or not all these questions elicit responses which fall upon a single liberal-conservative continuum. All that is important is that the questions probe beliefs that are relevant to the voting decision. To the extent that issues ever affect electoral outcome, the questions that make up this index are important questions.

The amount of variance in voting defection accounted for by each of the indices is presented in Table 2. The entry in each cell is the unbiased correlation ratio, an estimate of the percentage of variance in a dependent variable explained by an independent variable.[30]

[30] For a description of the unbiased correlation ratio, see Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill Book Co., 1960), p. 267. These ratios are based on a series of one-way analyses of variance. The estimate of the percentage of variance explained is obtained by multiplying the ratio by 100. For example, party compatibility explains 5.8 percent of the variance in voting defection in 1956.

TABLE 1. MEAN DEFECTION SCORE WITHIN CATEGORIES OF COMPATIBILITY FOR 1956, 1960, AND 1964[a]

Index of Compatibility	Category of Compatibility		
	Low	Medium	High
1956			
Party	25.14[b]	10.09	5.18
	(74)[c]	(447)	(435)
Candidate	29.73	7.36	2.12
	(158)	(425)	(373)
Policy	29.61	9.82	3.42
	(93)	(455)	(408)
1960			
Party	31.73	9.41	2.02
	(82)	(495)	(522)
Candidate	28.83	7.17	0.90
	(162)	(448)	(489)
Policy	27.16	7.07	0.97
	(134)	(613)	(352)
1964			
Party	37.60	10.14	2.29
	(86)	(452)	(349)
Candidate	41.35	6.16	1.67
	(145)	(308)	(434)
Policy	25.22	9.21	2.04
	(185)	(351)	(351)

[a] The grand mean defection score for 1956 is 9.02; for 1960, 7.57; for 1964, 9.17.

[b] The entry is the mean defection score for a specific category of compatibility for a specific index for a single year. The higher the score, the greater the defection.

[c] The number within the parentheses is the number of respondents upon which the cell mean is based. In all cases, the category of low compatibility includes all respondents below the theoretical zero point of the index.

TABLE 2. ESTIMATES OF VARIANCE IN DEFECTION EXPLAINED BY INDICES OF COMPATIBILITY

Index of Compatibility	1956	1960	1964
Party	.058	.155	.187
Candidate	.196	.255	.386
Policy	.118	.158	.141

As the cell frequencies in Table 1 reveal, in 1956 and 1960 the largest numbers of voters are cross-pressured in their orientations toward the candidates. Table 2 supports the importance of attitudes toward the candidates. In every election, the Index of Candidate Compatibility explains the most variance in voting defection. The correlation ratio for this index increases each election to a high of .386 in 1964. In 1964, numerous voters had misgivings about the candidacies of both Johnson and Goldwater.[31]

In the two-way analyses of variance (where one variable is controlled for the effects of another), orientations toward the candidates consistently cut into the variance explained by party orientation. An example is Table 3. The marginal means of the two indices are fairly

[31] We might note in this regard the rather low amount of variance in defection explained by policy compatibility in 1964 relative to candidate and party compatibility. Perhaps the direct impact of issues upon voting in 1964 was not as great as is popularly supposed.

TABLE 3. MEAN DEFECTION SCORES WITHIN CATEGORIES OF PARTY AND CANDIDATE COMPATIBILITY IN 1964

Index of Party Compatibility	Index of Candidate Compatibility			
	Low	Medium	High	Marginal Mean
Low	50.36[a]	20.89	6.77	37.60
	(55)[b]	(18)	(13)	(86)
Medium	36.67	7.46	1.46	10.14
	(78)	(196)	(178)	(452)
High	30.50	0.62	1.55	2.29
	(12)	(94)	(243)	(349)
Marginal Mean	41.35	6.16	1.67	9.71
	(145)	(308)	(434)	(887)

[a] Mean Defection Score.

[b] Cell Frequency.

TABLE 4. ESTIMATES OF VARIANCE IN DEFECTION EXPLAINED BY EACH INDEX OF COMPATIBILITY, CONTROLLING FOR ANOTHER INDEX FOR THE PRESIDENTIAL ELECTIONS OF 1956, 1960, AND 1964[a]

Year Control Index	Independent Index		
	Party	Candidate	Policy
1956			
Party		.152	.076
Candidate	.014[b]		.038
Policy	.016	.116	
1960			
Party		.119	.088
Candidate	.050		.064
Policy	.085	.131	
1964			
Party		.224	.060
Candidate	.025		.024
Policy	.106	.268	

[a] The estimate of variance explained by the interaction of the two indices is not included in these totals.

[b] The entry is the unbiased correlation ratio (adjusted for unequal cell sizes). The ratio is taken from Hays, who calls the measure the estimated omega squared. William L. Hays, *Statistics for Psychologists* (New York: Holt, Rinehart and Winston), pp. 406–407. These ratios are adjusted for unequal cell sizes, on the basis of a rationale presented in the Appendix. For the adjustment procedure, see B. J. Winer, *Statistical Principles in Experimental Design* (New York: McGraw-Hill Book Co., 1962), pp. 224–227.

similar. The influence of party compatibility appears to be almost as great as that of candidate compatibility. The cross-classification tells a different story, however. The mean defection scores drop more precipitously within categories of party compatibility (looking across the rows) than within categories of candidate compatibility (looking down the columns). Thus, within levels of candidate compatibility, the impact of party compatibility upon defection is substantially less than when the influence of party compatibility is explored independently of the candidate index. That candidate compatibility soaks up much of the variance explained by party compatibility is evident by a comparison of unbiased correlation ratios for Table 3. Uncontrolled, party compatibility explains 18.7 percent of the variance in defection. Controlled for candidate compatibility, party compatibility explains only 2.5 percent. In comparison, candidate compatibility, uncontrolled, explains 38.6 percent of the variance in defection. Controlled for party compatibility, candidate compatibility still explains 22.4 percent of the variance in defection. Table 4 gives a breakdown of the results of the two-way analyses of variance for all indices and all years.

Thus, attitudes toward the candidates are the major statistical explanation of voting defection. The statistical importance of the candidate orientation may reflect in part an ability among voters to articulate attitudes toward candidates more easily than attitudes toward parties and policies. To state likes or dislikes about parties and policies may require a more sophisticated or abstract set of political concepts than those required to state preferences between candidates. Alternatively, disenchantment with a party or its policies may be displaced upon the candidates as visible symbols of the parties. In any case, our attitudes toward the candidates have a significant impact upon our loyalty to the parties they lead.

Having accepted the first hypothesis, we turn now to a test of the second. It concerns the interrelationships of the indices of compatibility in their effect upon voting defection. Interaction is predicted for individuals who are simultaneously high or low on each of two indices of compatibility. Specifically, the hypothesis reads as follows.

H 2′. Taken together, certain combinations of the indices are mutually reinforcing. Voters whose indices consistently reveal high compatibility with party identification defect less than an additive model would predict. Conversely, voters with consistently incompatible indices defect more than an additive model would predict. Inconsistent indices combine additively.

This is a hypothesis one would like to reject. It is a statement of the existence of interaction, and both theory and statistics deal fitfully with interaction. The world of additive relationships is a simpler world. If additive generalizations do not distort a description of the world too much, additive propositions are preferable to interactive ones.

The amount of variance in voting defection explained by the interaction of the indices of compatibility is small. There are three pairwise comparisons between the three indices in each of the three election years.[32] The range for the

[32] In any year the three pairs are party and candidate, party and policy, and candidate and policy.

nine estimates of variance attributable to interaction is .003 to .022. At maximum, then, only 2.2 percent of the variance in voting defection is explained by interaction among two of the indices. The range of the nine F ratios is 1.834 to 9.773.[33] Six of the nine F ratios are statistically significant above .01.

Despite their statistical significance, the magnitude of the F ratios is not great. Were it not for the consistent pattern of the interaction, the hypothesis of interaction could be rejected in favor of an additive proposition. But there is a pattern to the interaction, a pattern which holds for each pair of indices for each election year. Of the nine tables of pairwise cross-classifications, only one cell of one table departs from the pattern. The pattern (outlined below) involves three cells in each table for a total of 27 separate predictions. The predictions hold in 26 of 27 instances.[34]

The pattern itself differs somewhat from the one predicted in the second hypothesis. (Thus, the 27 predictions above are post-facto observations.) Hypothesis 2 correctly states that individuals who hold two orientations that are both incompatible with their party identification defect more than one would predict from just adding together the separate effects of the two orientations; rather the orientations have a reinforcing impact upon one another, and, thus, combine interactively to produce an additional probability of defection. There are no exceptions to this prediction in any of the three elections. (As the appendix argues, the deviation of the means predicted by an additive model from the observed means is an estimate of the interaction effect for each cell.)

On the other hand, the hypothesis predicts that individuals who hold inconsistent orientations—e.g., a candidate orientation of high compatibility and a policy orientation of low compatibility—conform to an additive model. In fact, voters with inconsistent orientations defect less than the additive model predicts. Table 5 shows

[33] The F ratio for interaction is the interaction mean squares divided by the total mean squares.

[34] When one is testing a proposition about interaction in specific cells, measures of association or F tests should not be a final determinant of acceptance or rejection in any case. These measures of association and F tests are not specific to certain cells. The impact of interaction in one cell is diluted by additivity in another. Thus, the variance explained by interaction may be small in spite of a large impact upon individuals in the interaction cell. This dilution is especially critical when, as is often the case, relatively few people occupy the cell manifesting the interaction effect. For this reason, we assign as much importance to a pattern the interaction displays as to either the variance the interaction explains or its statistical significance.

TABLE 5. THE INTERACTIVE RELATIONSHIP BETWEEN CANDIDATE COMPATIBILITY, POLICY COMPATIBILITY, AND VOTING DEFECTION IN THE 1960 ELECTION

Index of Candidate Compatibility	Index of Policy Compatibility					
	Means Predicted by an Additive Model[a]			Observed Means		
	Low	Medium	High	Low	Medium	High
Low	38.53[b]	24.42	20.77[d]	44.25 (56)[c]	23.65 (85)	8.67[d] (21)
Medium	20.56	6.45	2.79	20.81 (52)	6.95 (283)	1.42 (113)
High	15.89[d]	1.78	−1.88	3.08[d] (26)	1.46 (245)	0.00 (218)
Deviations of Predicted from Observed Means						
Low	5.72	−0.78	−12.10[d]			
Medium	0.25	0.50	−1.38			
High	−12.81[d]	−0.31	1.88			

[a] The prediction equation is

$$\hat{Y}_{ij} = -1.88 + 22.65C_1 + 4.67C_2 + 17.77P_1 + 3.66P_2$$

[b] The entry is the mean defection score, here predicted by an additive model.

[c] The number of respondents upon which the cell mean is based.

[d] Cells of inconsistent orientations.

that if we simply add together the effects of a low Index of Candidate Compatibility and a high Index of Policy Compatibility, we would predict a defection score of 20.77. The observed mean defection score for voters with this combination of orientations is a much lower mean of 8.67. Table 6 presents a slightly abbreviated version of Table 5 for all pairs of indices for every year. In all, there are nine such tables of pairwise comparisons—three tables for each of the three elections. The 18 extremely inconsistent cells from the nine tables are relevant to the test of the combination of inconsistent orientations. In only one of 18 cells is the observed mean defection score not lower than the score predicted by an additive model. What seems to be occurring, then, is a smothering effect of one orientation upon another. A position of high compatibility on one orientation more than offsets a low position on another. When one orientation smothers or countervails another, allegiance to party in voting is greater than one would predict on the basis of the separate effects of the two orientations.

In brief, interaction exists, but it is not the interaction predicted by the second hypothesis. For future tests, we alter the hypothesis to read as follows.

H 2.′ Taken together, certain combintaions of the indices of compatibility combine interactively. Voters whose political orientations reveal low compatibility with party identification defect more than an additive model predicts. Voters who occupy inconsistent positions on two indices defect less than an additive model predicts. All other combinations conform to an additive model.

To this point we have examined cross-pressures as a cause—their additive and interactive impact upon voting defection. We must now ask an additional question, the question of how cross-pressures arise. Earlier, we argued that rates of social mobility provide one explanation of voters acquiring political attitudes which conflict with party allegiance. People at times come to occupy different positions on different status hierarchies. This inconsistency is a source of anxiety and frustration, conditions which can lead to extreme and salient political attitudes. Since attitudes such as these have sources and supports apart from party identification, the hypothesis predicts that these attitudes often conflict with party identification. The effect of status inconsistency, then, is cross-pressure, the incompatibility of political orientations with party allegiance.

H 3. Status inconsistency increases political cross-pressures above the level an additive model would predict.

The data give no support to the hypothesis that status inconsistency increases cross-pressures, no support relative to any of the three indices of compatibility for any pair of the four dimensions of status.[35] Table 7 is a typical example of the lack of a relationship between status inconsistency and cross-pressures.

If the interaction hypothesis were true, in the inconsistent cells the observed mean Index of Candidate Compatibility would be lower than the predicted mean, yielding a negative mean deviation. While one cell mean deviation is negative, the other is positive. The magnitude of the deviations are in both cases small. This is the typical pattern.[36]

Table 7 indicates another finding consistent in the data. There is no additive relationship between the dimensions of status and political cross-pressures. Income, education, occupational prestige, racial-ethnicity, all are unrelated to a disposition to hold political attitudes contrary to party identification.

The importance of status inconsistency in current sociological theory justifies a pause to consider why it proved so impotent an explanation in this particular case. Any of the following four conclusions may be justified. 1) We might simply say that no one has ever demonstrated that status inconsistency is, empirically, a powerful concept. For example, Jackson and Burke explained only about 2 percent of the variance in symptoms of stress with status inconsistency.[37] In short, we could conclude that we have been seduced by the theoretical attraction of a concept which at this point should be discarded. I, personally, am hesitant to do this, simply because the concept still makes substantial intuitive sense to me. Certainly, alternative conclusions are possible. 2) For example, we might conclude that the concept was related to an inappropriate dependent variable. That is, we might say that, while status inconsistency does produce extreme political attitudes, it does not

[35] The four dimensions of status upon which Hypothesis 3 is tested are racial-ethnic prestige, occupational prestige, family income, and education.

[36] Because the tables yield negative results they are omitted. They do appear in Boyd, *op. cit.*, Appendix C.

[37] Elton F. Jackson and Peter J. Burke, "Status and Symptoms of Stress: Additive and Interaction Effects," *American Sociological Review,* 30 (Aug., 1965), 556–564. See also Elton F. Jackson, "Status Consistency and Symptoms of Stress," *American Sociological Review,* 27 (Aug., 1962), 469–480.

TABLE 6. INTERACTION AMONG THE INDICES OF COMPATIBILITY AND VOTING DEFECTION. DEVIATIONS OF PREDICTED FROM OBSERVED MEANS IN 1956, 1960, 1964

1956

	Candidate			*Policy*		
Party	Low	Medium	High	Low	Medium	High
Low	2.62[a]	1.28	−11.59	4.00	0.90	−13.61
Medium	0.96	0.42	−1.32	−1.89	0.24	0.14
High	−3.42	−0.88	1.30	−1.95	−0.52	0.53

1. $\hat{Y}_{ij}=0.86+10.73P_1+2.69P_2+25.22C_1+4.27C_2$
2. $\hat{Y}_{ij}=2.03+11.58P_1+2.87P_2+22.52I_1+5.37I_2$

	Policy		
Candidate	Low	Medium	High
Low	5.53	−1.22	−5.38
Medium	−2.94	1.21	−1.24
High	−16.78	−1.44	1.50

1. $\hat{Y}_{ij}=0.49+22.63C_1+3.50C_2+16.30I_1+3.46I_2$

1960

	Candidate			*Policy*		
Party	Low	Medium	High	Low	Medium	High
Low	3.73	−3.91	−7.78	2.18	0.50	−13.31
Medium	1.45	1.44	−2.81	5.05	0.18	−3.06
High	−9.17	−1.36	1.71	−9.51	−0.30	2.02

1. $\hat{Y}_{ij}=-0.89+18.67P_1+4.30P_2+22.18C_1+4.55C_2$
2. $\hat{Y}_{ij}=-1.47+23.07P_1+5.88P_2+20.28I_1+3.95I_2$

	Policy		
Candidate	Low	Medium	High
Low	5.72	−0.78	−12.10
Medium	0.25	0.51	−1.38
High	−12.81	−0.31	1.88

1. $\hat{Y}_{ij}=-1.88+22.65C_1+4.67C_2+17.77I_1+3.66I_1$

1964

	Candidate			*Policy*		
Party	Low	Medium	High	Low	Medium	High
Low	1.17	2.38	−8.25	3.49	−5.86	6.46
Medium	−0.23	1.24	−1.27	1.45	1.16	−2.65
High	−3.85	−3.05	1.37	−6.79	−0.45	1.73

1. $\hat{Y}_{ij}=0.18+14.84P_1+2.55P_2+34.17C_1+3.49C_2$
2. $\hat{Y}_{ij}=-0.87+28.41P_1+5.86P_2+16.02I_1+3.79I_2$

	Policy		
Candidate	Low	Medium	High
Low	3.54	−0.21	−17.58
Medium	−0.70	0.70	−0.73
High	−4.31	−0.71	1.26

1. $\hat{Y}_{ij}=-0.19+35.49C_1+3.19C_2+10.34I_1+2.12I_2$

[a] The entry is the deviation of the mean predicted by an additive model from the observed mean. From this entry and the prediction equation, predicted and observed means can be reconstructed.

[b] Prediction equations for an additive model. The higher the mean, the greater the defection. In the equation, "*I*" stands for the policy index.

TABLE 7. MEAN CANDIDATE COMPATIBILITY SCORES FOR COMBINATIONS OF RACIAL-ETHNIC STATUS AND INCOME IN THE 1964 ELECTION

Racial-Ethnic Status	Income					
	Observed Means			Deviation of Predicted from Observed Means[a]		
	Low	Medium	High	Low	Medium	High
High	13.39[b] (64)[c]	12.43 (91)	12.94 (87)	0.29[b]	−0.46	0.27
Medium	13.23 (64)	13.73 (120)	13.04 (149)	−0.36	0.35	−0.13
Low	13.83 (64)	13.53 (74)	13.26[b] (84)	0.08	−0.00	−0.06[b]

[a] Means are predicted by the equation

$$\hat{Y}_{ij} = 13.32 - .64R_1 - .15R_2 + .43I_1 + .21I_2$$

[b] Inconsistent Statuses.

[c] Cell frequencies.

act upon these attitudes in such a way as to create a discrepancy between these attitudes and party identification. The status inconsistent person might be more inclined than others to change his identification to coincide with his attitudes, because political attitudes are more salient for him than the average individual. 3) We might also conclude that some of the measures of status are wrongly applied to national populations. It is one thing, one might say, to rank all of the people in regard to income or education. It is quite another thing to attempt to construct an index of race-ethnicity that is equally applicable to all sections of the country. Being a Jew in New York simply does not affect the life-style of a person like being a Jew in, say, Plainview, Texas. Since some of the more interesting findings in the work of Lenski and of Jackson and Burke do relate to the dimension of race-ethnicity, we might infer that this particular dimension of status should only be applied to populations that live in homogeneous sub-cultures. 4) Finally, we might conclude that status inconsistency is more applicable to some political strata than others. That is, it may be that status inconsistency has its major impact on a small number of real political activists, rather than to a population of rank-and-file voters. Here, we might point to the political careers of highly educated members of various minority groups. In sum, there are a number of refinements we might make in our theories of status inconsistency that argue against its wholesale rejection. The fourth alternative seems especially plausible to me.

In addition to status inconsistency, we have other possible explanations of political cross-pressures. Earlier, I argued the reasonableness of Hypothesis 4.

H 4. Inter-generational mobility increases political cross-pressures above the level an additive model would predict.

A test does not, however, bear out the hypothesis that inter-generational mobility creates cross-pressures. When father's occupational prestige and respondent's social class when growing up are cross-classified, in turn, against the respondent's present occupation and income, in no case does a relationship between inter-generational mobility and political cross-pressures appear.

As a final test, the relationship between geographical mobility and cross-pressures is explored.

H 5. Geographical mobility increases political cross-pressures above the level an additive model would predict.

Two indicators of geographical mobility are available in the SRC election studies. The first is a question asking the length of time the respondent has lived at his present residence. The second indicator is a comparison of the area of the nation in which the respondent grew up with the area in which the respondent lived at the time of the election interview. Neither of these measures of geographical mobility bears any relation to the disposition to hold political attitudes conflicting with party identification. All indices of compatibility in all years conform to the pattern of Table 8.

If Table 8 is collapsed, the mean compatibility score is actually *lower* for those who reside in the area where they grew up than the mean

score for those who have moved away. This result holds for each index in each election, although in no case is the magnitude of the difference great.[38]

V. SUMMARY

1. Cross-pressures relate monotonically to voting defection—the greater the cross-pressures, the greater the likelihood of defection. The degree of cross-pressure is measured by three indices of compatibility, which, in turn, are measures of the extent to which a man's political beliefs support his party identification. A person may hold attitudes about a) the political parties, b) the nominees of those parties, and c) the policies he associates with the parties. If any of these sets of attitudes is in conflict with his party identification, the person is defined as being politically cross-pressured in regard to that attitude. The greater the cross-pressures, the more likely he is to defect, to vote against the candidate of his party.

Cross-pressures are not a wide-spread occurrence. Party identification is a cognitive anchor for our political attitudes. The member of a party is prone to look with approval upon his own party's conduct and to be suspicious of the conduct of the other. He has nice things to say about his party's candidate; he wonders about the other man. He has his notions about public policy, and he sees a difference between the parties in their support of his notions. In short, the influence of party identification upon political attitudes is great. As a rule, the individual holds attitudes that support his identification. To hold such attitudes is usually sufficient to prevent an individual from defecting.

At times, however, forces arise which move some men to acquire attitudes that conflict with their party identification. When they do, these political attitudes tend to prevail over this identification. The greater the conflict between a person's political attitudes and his party identification, the more likely he is to vote for the candidate of the opposing party.

2. The three sets of political attitudes combine interactively in their impact upon the voting act. For example, a man may have unfavorable attitudes toward both the candidate of his party and the policies he associates with his party. Alternatively, he may be favorable in regard to one attitude (such as the candidate of his party), but unfavorable in regard to another (say, his perception of his party's policies). If a voter's political orientations consistently conflict with his party identification, he defects in rates that are higher than one would predict simply from adding together the separate effects of each of the orientations. Orientations that consistently conflict with party identification, then, reinforce one another in their impact on his vote.

Interaction is also manifest when a voter has one favorable and one unfavorable orientation at the same time. In this case the favorable or compatible orientation smothers the impact of the incompatible orientation. The attitudes of such a voter push him in opposite directions, and he defects less than one would predict merely from adding together the separate effects of the two orientations. All other combinations of political orientations conform to an additive model. That is, for all other combinations of pairs of political attitudes, our prediction regarding the likelihood of defection is most accurate when we simply add together the separate impact of each of the two attitudes.

While cross-pressures prove to be an important source of voting defection, the causes of cross-pressures remain unknown. Status inconsistency was wrongly hypothesized as a cause. Alternative explorations of inter-generational status mobility and geographical mobility also failed as explanations of cross-pressures.

In closing, we might speculate upon the relevance of the cross-pressure findings to campaign strategies. The simple and obvious advice is to run attractive candidates. In each of the three elections, attitudes toward the candidates explain the most variance in voting defection. The 1956 and 1964 elections testify to the impact of the candidates. In these two elections a substantial proportion of the electorate seemed at-

[38] The same is also true for the relationship of mobility to voting defection in the three elections. The non-mobile defect more than the mobile in all three elections, though, again the magnitude of the differences is not great.

TABLE 8. MEAN POLICY COMPATIBILITY SCORES FOR COMBINATIONS OF GEOGRAPHICAL MOBILITY IN THE 1964 ELECTION

Area Where Respondent Now Lives	Area Where Respondent Grew Up			
	Northeast	Midwest	South	West
Northeast	*12.53*[a]	14.40	12.42	0.00
	(177)[b]	(5)	(12)	(0)
Midwest	12.00	*12.35*	14.00	13.00
	(10)	(210)	(34)	(1)
South	12.83	14.00	*12.73*	14.20
	(18)	(15)	(231)	(5)
West	11.25	12.94	14.70	*13.59*
	(4)	(32)	(23)	(82)

[a] The cell entry is the observed mean Index of Policy Compatibility. The higher the mean, the higher the degree of compatibility. The consistent cells are italicized.

[b] Cell frequencies.

tracted to the victorious candidate and repelled by the other. In spite of the fact that he represented a minority, the difference in the appeal of Stevenson and Eisenhower alone was probably sufficient to elect Eisenhower in 1956. In 1964, of course, the Goldwater candidacy produced an overwhelming majority for Johnson.

The importance of issues is more problematic than the importance of candidates. In the 1956 and 1960 elections, attitudes towards the policies that the public associated with the parties ranked second to candidate appeal in impact upon defection.[39] If not as much as candidates, issues do influence the outcome of elections. The impact of issues is problematic because issues seem to have significance beyond any one campaign. That is, it seems a reasonable hypothesis that candidates affect the outcome of specific elections, while issues and their relation to historical events affect the distribution of party identification. The minority does not seek just an occasional victory. Rather, it hopes to realign party allegiance, to become the majority party. To affect a realignment, a President from a minority party may have to identify his administration with attacks on problems the public deems critical.[40] This Eisenhower did not do, and his administration left the distribution of party identification unaltered. In regard to campaign strategy, it would not seem necessary for the candidate of a minority party to emphasize the critical issue or issues in the campaign of his initial victory. But in the course of his administration, he must identify his regime with these critical issues. Undoubtedly, then, future campaigns would be fought on these issues. In short, the impact of a candidate is substantial but of short duration. The impact of issues, while rarely great at any single moment, accumulates over a period of time. Overall, issues may outweigh candidates in affecting the outcome of elections, for issues have the capacity to alter the greatest single determinant of a vote, party identification.

[39] See Table 4. In the 1964 election, however, the rank-order of the importance of the three sets of orientations as judged by their ability to statistically explain voting defection was candidate, party, and policy orientation. The impact of party attitudes is not discussed in relation to its possible impact upon campaign strategy, because there seems little a party could do directly to alter its image with the public. Changes in attitudes toward the parties probably result from candidates and issues.

[40] Quite likely, the type of issue necessary to affect a realignment is a "position issue" rather than a "valence issue." For the distinction between the two, see Donald E. Stokes, "Spatial Models of Party Competition," in Campbell, *Elections and the Political Order,* pp. 171–172.

APPENDIX

THE CONCEPT OF STATISTICAL INTERACTION

In the theory presented in this paper, several of the relationships share a problem of analysis: The independent variables (the presumed causes) are said to combine *interactively* in their impact upon the dependent variable (the presumed effect). Since the exact meaning of statistical interaction is often misunderstood, a brief explanation of the term is perhaps in order.

In normal English usage interaction simply means factors mutually related. This is not its statistical meaning. *Multicollinearity,* the condition of highly related independent variables, is the statistical term that is closest to the English sense of interaction. *Statistical interaction* is a *change* in the relationship between two variables over different levels of a third. The way in which independent variables combine in their effect upon the dependent variable becomes an important datum. If statistical interaction is absent (i.e., if the relationship is *additive*), the relationship between two variables remains *constant* over various categories of the third variable. For example, economic status varies directly with conservatism on welfare issues. But the relationship between welfare conservatism and status may change over different categories of religion. That is, the relationship may be more pronounced with Protestants than, say, Jews. Religion and status, then, combine interactively in their impact upon welfare conservatism.

Multicollinearity and interaction are independent phenomena. As Figure 2 indicates, all combinations of interaction and multicollinearity are possible.[41]

The importance of interaction must not be underestimated. In the first place, there is every reason to think that the world is more complex than additive models suggest.[42] In the second, interaction submits but grudgingly to present

[41] For an illuminating discussion of the subject, complete with graphic examples, see James N. Morgan and John A. Sonquist, "Problems in the Analysis of Survey Data, and a Proposal," *Journal of the American Statistical Association,* 58 (June, 1963), 415–434.

[42] For just a few of the many examples of social phenomena explicitly or implicitly conceptualized in terms of interaction, see the following article and the notes cited therein. Hubert M. Blalock, Jr., "Status Inconsistency, Social Mobility, Status Integration and Structural Effects," *American Sociological Review,* 32 (Oct., 1967), 790–801.

FIG. 2. Statistical Interaction and Multicollinearity

		In their impact upon a dependent variable, the independent variables may combine: Additively	Interactively
The independent variables may be	Related (Multicollinearity)	Possible	Possible
	Unrelated	Possible	Possible

statistical methods. For example, an ordinary multiple or partial correlation coefficient is a weighted average of the relationship between two variables over the range of a third.[43] Such a coefficient assumes this relationship to be constant. If it is not constant, one cannot speak with fidelity to the world of the relationship between, say, A and Y, B controlled.[44] One may physically control for B, but this procedure may suffer a loss of cases. (Of course, it is possible to recover the loss of cases by averaging the measures of association from the individual contingency tables produced by the control from B. However, this procedure presumes that each of the measures is approximately the same, i.e., that there is not interaction.)

One means of handling statistical interaction is analysis of variance. Analysis of variance meets several needs in the tests of the hypotheses in this paper. First, the procedure yields estimates of the total amount of variance in the dependent variable attributable to the additive and interactive effects of the independent variables.

Second, the statistical technique is able to accept unequal cell frequencies. In analysis of variance, specific methods differ, depending on whether cell frequencies are equal, proportionate (which requires the independent variables to be uncorrelated), or disproportionate.[45] If the cell sizes are disproportionate one has to decide whether or not the inequality of cell sizes results from happenstance or from a correspondence of the sample to the population from which the data was drawn. If the latter is true, and it is in my case, then a least squares solution to the problem of unequal cell sizes is probably appropriate.[46]

Third, if one's hypotheses predict that interaction will occur in specific cells, then the technique must be able to locate the cells in which the interaction occurs. Following the lead of Duncan,[47] Jackson and Burke[48] and Treiman[49] have developed one least squares procedure that estimates the specific location of interaction. This procedure utilizes a dummy weighting technique whereby each category of variable A becomes a separate variable A_1, A_2, A_3, etc. For every individual the dummy variables have a value of either 0 or 1 depending on whether or not he falls into that category.[50] Below is an equation of this type specifying an additive model.

$$\hat{Y}_{ij} = a + b_1A_1 + b_2A_2 + b_3B_1 + b_4B_2$$

This is to say that the prediction of a dependent variable score ($\hat{Y}_{ij}$) may be computed from a knowledge of three components: one common to all individuals (a); another common to individuals sharing an attribute (A_i); and a final one common to those sharing a second attribute (B_j). Figure 3 is a graphic form of the equation.

One can see from Figure 3 why it is not necessary to include categories A_2 and B_2 in the equa-

[43] Blalock, *Social Statistics*. p. 332.

[44] As in the case of interaction, problems also crop up when multicollinearity is present. See Hubert M. Blalock, Jr., "Correlated Independent Variables: The Problem of Multicollinearity," *Social Forces*, 42 (Dec., 1963), 233–237.

[45] The remainder of the discussion applies only to multiple-way analysis of variance. One-way analysis of variance, which is used to test some hypotheses, does not require an adjustment of unequal cell sizes.

[46] Winer, *Statistical Principles in Experimental Design*, p. 224. The method has been around a long time. See F. Yates, "The Analysis of Multiple Classifications with Unequal Numbers in the Different Classes," *Journal of the American Statistical Association*, 29 (March, 1934), 51–66.

[47] Otis Dudley Duncan, "Residential Areas and Differential Fertility," *Eugenics Quarterly*, 11 (June, 1964), 82–89.

[48] Elton F. Jackson and Peter J. Burke, *loc. cit.*

[49] Donald J. Treiman, "Status Discrepancy and Prejudice," *American Journal of Sociology*, 71 (May, 1966), 651–664.

[50] Daniel B. Suits, "Use of Dummy Variables in Regression Equations," *Journal of the American Statistical Association*, 52 (Dec., 1957), 548–551.

FIG. 3. A Graphic Representation of an Additive Equation

	B_1	B_2	B_3
A_1	$\hat{Y}_{11}=a+A_1+B_1$	$\hat{Y}_{12}=a+A_1+B_2$	$\hat{Y}_{13}=a+A_1$
A_2	$\hat{Y}_{21}=a+A_2+B_1$	$\hat{Y}_{22}=a+A_2+B_2$	$\hat{Y}_{23}=a+A_2$
A_3	$\hat{Y}_{31}=a+B_1$	$\hat{Y}_{32}=a+B_2$	$\hat{Y}_{33}=a$

tion. The model uses these categories for the constant a. (If we had followed the procedure of Kempthorne and others of setting the constant equal to the grand mean, then a term for A_3 and B_3 would have been included.[51])

Once the regression coefficients in the equation are solved, the differences between the observed cell mean and the mean predicted by the additive equation is an *estimate* of the interaction effect for that cell.[52] If interaction exists, by including an appropriate interaction term in the equation, e.g., $b_8A_1B_3$, differences between the observed and the predicted means are reduced. In this manner one specifies a *form* the interaction may be taking, in this example a multiplicative effect for individuals concomitantly in category A_1 and in category B_3.[53]

To be sure, the knotty problems of statistical inference appear with the use of the least squares method just as with other methods. Several factors other than a status inconsistency effect or a mobility effect can produce statistical interaction. With no claim of being exhaustive, Blalock has noted eight alternative explanations for interaction grouped under the headings of sampling error, measurement error, and specification error.[54] This is just to emphasize that the analyst must always be on guard against the possibility that his results are dependent on (to quote another) "statistico-empiricalistic factors and antifactors." Conversely, it is also true that an interactive world can produce additive statistics under certain limited conditions. For example, if there are, say, mobility effects in opposite corners of a table that affect the dependent variable in

[51] Oscar Kempthorne, *The Design and Analysis of Experiments* (New York: John Wiley and Sons, Inc., 1952), pp. 79–87; George W. Snedecor, *Statistical Methods Applied to Experiments in Agriculture and Biology* (5th ed.; Ames, Iowa: The Iowa State College Press, 1956), pp. 338–391; and R. L. Anderson and T. A. Bancroft, *Statistical Theory in Research* (New York: McGraw-Hill Book Co., 1952), pp. 278–284.

[52] The assumptions which permit the determination of the coefficients are the following: (1) The sum of squares due to interaction shall be minimized.

$$\sum\sum n_{ij}(\overline{AB}_{ij} - a - A_i - B_j)^2 = \text{minimum}$$

(2) The sum of the interaction terms (the deviations from an additive model), weighted by the cell frequencies, shall sum to zero over each row and each column.

$$\sum n_{j.}A_i = 0 \quad \text{and} \quad \sum n_{.j}B_j = 0$$

In short, the additive pattern in each table is defined as that pattern which minimizes the sum of squares attributable to interaction. The only difference between the procedure used in this paper and the more familiar method of analysis of variance is that, in this procedure, the distribution of cell frequencies is allowed for theoretical reasons to influence the estimates of interaction. The program that computed the coefficients and the adjusted sums of squares was written jointly by C. Richard Hofstetter of Ohio State University and this author.

[53] The use of a least squares technique to estimate the location of interaction is not uncontroversial. In the first place, so long as one's cell means conform to an additive model, the pattern of cell frequencies has no effect on the size of the regression coefficients. However, if one's cell means do *not* conform to an additive model, the pattern of cell frequencies will affect the coefficients and, thus, the magnitude of the deviations from an additive model. Furthermore, because the technique is based on least squares, the largest deviations will tend to be found in the cells with the smallest cell sizes. To guard against the possibility that my results are an artifact of the method I used, I also ran the analysis, substituting equal cell sizes while retaining the original cell means. The same results obtained. In the second hypothesis, the direction of the deviations in the 27 critical cells remained the same in every instance. The size of the deviations did not vary greatly from those in Table 6.

[54] Hubert M. Blalock, Jr., "Tests of Status Inconsistency Theory: A Note of Caution," *Pacific Sociological Review*, 10 (Fall, 1967), 69–74.

FIG. 4. An Additive and an Interactive Equation Both Producing Additive Cell Means

	B_1	B_2
A_1	00	30
A_2	10	40

1. Additive equation:
   $\hat{Y}_{ij} = 0A_1 + 10A_2 + 0B_1 + 30B_2$
2. Interactive equation:
   $\hat{Y}_{ij} = 0A_1 + 20A_2 + 0B_1 + 20B_2 + 10A_1B_2 - 10A_2B_1$

opposite directions, the data can be quite consistent with an additive model.[55] In Figure 4 it may be seen that both the additive and the interactive equation produce the same pattern of cell means. Fortunately, opposite effects in opposing cells do not seem to be a likely complication in the hypotheses presented in this paper.

Analysis of variance requires that the dependent variable have an interval level of measurement. Except for education and income, this is true of no variable in this study. All other measurements meet ordinal assumptions. In this case it seems preferable to sacrifice strict adherence to assumptions for the power of the variance technique. Given our interest in measuring interaction, the alternative is no analysis.[56]

In addition, the sampling design of all three studies is a clustered design. In tests of significance, this design violates the assumption of a simple random sample. Also, the 1960 election is a weighted sample, with some respondents appearing as many as four times. Taking these individuals out would destroy the sampling design, but leaving them in violates the assumption of uncorrelated error terms in analysis of variance. Because of the clustered design and because of the weighted sample in 1960, we should be cautious in interpreting measures of statistical significance. Thus, all measures of significance reported here are viewed as estimates of the figures that a simple random sample would yield.

Finally, regression analysis can be misleading if substantial multicollinearity exists. To the degree that the independent variables are intercorrelated, sampling and measurement error begin to dominate the estimates of the regression coefficients.[57] As a check upon the amount of multicollinearity among the cross-pressure indices, each of the three indices was run against the other in an ordinary linear regression program. For the three elections, the range of the resulting product moment correlations was .34 to .52. While these figures did not seem excessively high, the precaution was taken to run the three indices against the dependent variable, voting defection, in a multiple regression program (the BMD 03R).[58] This program computed the standard error of the regression coefficients associated with each of the indices. Had multicollinearity been a problem, the standard errors would have been large enough that the coefficients would not obtain statistical significance. However, of the nine coefficients (three coefficients for three years), all were significant at the .01 level. A final bit of evidence against the charge that the results are unduly affected by sampling (as opposed to measurement) error is the fact that the findings are consistent over each of three elections. This consistency would be unlikely if the analysis were really dominated by random error.

THE MEASUREMENT OF THE STATUS VARIABLES

1. Occupational Prestige. The ranking of occupations follows those categories established in the ICPR codebooks.[59] These categories are consistent with those used by others who have ranked occupational groups according to their prestige.[60] These categories are collapsed into three groups:

[55] See the following works by Blalock, "Review Symposium," *American Sociological Review,* 33 (April, 1968), 296–297. "Status Inconsistency and Interaction: Some Alternate Models," *The American Journal of Sociology,* 73 (Nov., 1967), 305–315. "The Identification Problem and Theory Building: The Case of Status Inconsistency," *American Sociological Review,* 31 (Feb., 1966), 52–61.

[56] A few attempts have been made to partition chi-square into additive and interactive effects as in analysis of variance, but the technique seems as yet undeveloped. For example, diochotomization of the dependent variable is required. K. V. Wilson, "A Distribution-Free Test of Analysis of Variance Hypotheses," *Psychological Bulletin,* 53 (1965), 96–101.

[57] J. Johnston, *Econometric Methods* (New York: McGraw-Hill Book Co., 1963), pp. 201–207.

[58] W. J. Dixon (ed.), *Biomedical Computer Programs* (Berkeley: University of California Press, 1967), pp. 258–275.

[59] ICPR Codebooks, Survey Research Center, University of Michigan, Occupation Code.

[60] For example, see Jackson, *op. cit.,* p. 471. See also Albert J. Reiss, Jr., *Occupations and Social Status* (New York: The Free Press of Glencoe, 1961), pp. 109–161 and Robert W. Hodge, Paul M. Siegel, and Peter H. Rossi, "Occupational Prestige

a. Professionals, etc.; self-employed businessmen
b. Clerical and sales; skilled workers and craftsmen
c. Farmers; semi-skilled workers; service workers; and unskilled laborers.

All respondents are classified according to the occupation of the head of the household. The retired and the unemployed are assigned to their former occupations.

2. Racial-ethnic Prestige. Respondents are ranked according to the categories used by Jackson in his study of status inconsistency and psychological stress.[61] The basis of the ranking is both race and the birthplace of the respondent and his family.

a. Old English or Old American
b. Northern European
c. Southern and Eastern European, Jew, and Non-caucasian.

Race and religion have priority in the rankings. This is, non-caucasians and Jews are automatically assigned to the third category. If the respondent is foreign-born, he is assigned to the category of the country of his birth. If not, if his father is foreign born, the respondent is classified according to the country of the birth of his father. If his father was also native-born, the respondent is classified according to the statement of the country from which his family originally came. If the respondent doesn't know, he is omitted from the analysis. In the 1956 study, the question regarding the origin of the family is absent. Respondents are classified on the basis of the country of his own birth, his father's birth, or his father's father's birth.[62] A large number of respondents are not classifiable under this scheme. The respondent whose family had been in the country long enough to escape classification on the basis of his father's father being native born is classified as Old American.

3. Income. Respondents are ranked according to the income of the whole family.[63] It is this figure than the income of the respondent or the head of the family alone that determines a respondent's life style. The income is trichotomized into approximately equal thirds in the analysis.

4. Education. Education is measured in terms of the number of grades completed, including college.[64] The scale is collapsed into three categories. Following the procedure of Lenski and Jackson, slightly different rankings are established for people above and below the age of 45. The reason, as Jackson states, is that "an older person would not be expected to have as much education as a younger person of the same occupational and ethnic rank."[65] For the older respondents the following categories are set:

a. College education and attended college
b. High school graduate and attended high school—with or without other non-college training
c. Eight years of school or less.

To adjust for age, a respondent under 45 years of age who attended high school only and had no non-college training is assigned to the third category.

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in the United States, 1925–63," *American Journal of Sociology*, 70 (Nov., 1964), 286–302.

[61] Jackson, *loc cit.* For 1964, see ICPR Codebook, Deck 7, Col. 12, Deck 8, Col. 33, Deck 9, Cols. 11–13, 15–17, 21.

[62] For 1956, *ibid.*, Deck 6, Cols. 10, 20–22, 25, Deck 5, Col. 27.

[63] For 1964, *ibid.*, Deck 9, Col. 44.

[64] For 1964, *ibid.*, Deck 7, Cols., 27–28.

[65] Jackson, *loc. cit.*

CAMPAIGN STRATEGY AND PARTY LOYALTY: THE ELECTORAL RELEVANCE OF CANDIDATE DECISION-MAKING IN THE 1964 CONGRESSIONAL ELECTIONS*

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I. INTRODUCTION

The analyst of campaign strategy in American elections is usually confronted by the apparent impossibility of demonstrating that campaign behavior actually matters.[1] Although candidates possess numerous options in strategy and tactics, it is clear that incumbent members of the majority party (commonly joint phenomena) usually win and opposition candidates of the minority party usually lose.[2] Lewis Froman has even argued that ". . . the behavior of the candidates during the campaign is one of the least influential factors in determining electoral outcomes."[3]

*I am grateful to Russell Slade, who collected the data upon which this article is based, and to Warren E. Miller and Donald E. Stokes for their comments on an earlier draft.

[1] Cf. Stimson Bullitt, "Because no one knows what works in a campaign, money is spent beyond the point of diminishing returns. . . No one dares to omit any approach. Every cartridge must be fired because among the multitude of blanks one may be a bullet," in *To Be A Politician* (Garden City, N.Y., 1961), p. 72; Charles F. Cnudde, ". . . political scientists know little about how the campaign affects the vote, or in fact whether it influences it at all," in a book review in this REVIEW, 62 (1968) p. 1365; John W. Kingdon, "Right or wrong, most candidates believe that campaigns affect election outcomes," in *Candidates For Office: Beliefs and Strategies,* (New York, 1968) p. 109. There is, however, some evidence that party *organizations* may, by their efforts, affect the vote. See, e.g., Phillips Cutright and Peter Rossi, "Grass Roots Politicians and the Vote," *American Sociological Review,* 23 (1958), 171–179; Daniel Katz and Samuel J. Eldersveld, "The Impact of Local Party Activity Upon the Electorate," *Public Opinion Quarterly,* 25 (1961), 1–24; William J. Crotty, "The Party Organization and its Activities," in Crotty (ed.), *Approaches To The Study of Party Organizations* (Boston, 1968), pp. 247–306, esp. pp. 277–286.

[2] See Charles O. Jones, "The Role of the Campaign in Congressional Politics," in M. Kent Jennings and L Harmon Ziegler (eds.), *The Electoral Process* (Englewood Cliffs, N.J., 1966), pp. 21–41, esp. pp. 23–24; Milton C. Cummings, *Congressmen and the Electorate* (Washington, 1966), *passim.*

The effects, if any, of campaign behavior are the residue to be examined after the partisan distribution of the electorate, the nature of the times, the power of incumbency and any number of variables beyond the ready control of the candidates themselves have been employed to explain or predict the popular vote distribution. The perceived general ineffectiveness of candidate strategy and tactics is usually explained by the low public saliency of the typical election contest, in which most voters are unlikely to be aware even of the names of the candidates, much less their policy positions or, presumably, any other of their behaviors.[4]

But this circumstance may change when "ordinary" election contests are connected to contests of much higher saliency, especially that for the Presidency. It is at this time, at least, when the strategic decisions and behaviors of certain classes of candidates may attract the attention of voters otherwise preoccupied with that more dramatic battle for power.

For example, a recurring, if seldom widespread, phenomenon of Presidential-year campaigns has been the avoidance or refusal of some candidates to endorse their party's nominee for President. This deviation from the customary forms of campaign behavior—a crime occasionally punished[5]—presumably occurs because the candidate believes his behavior on this question is of importance to some flexible and observant

[3] Lewis A. Froman, Jr., "A Realistic Approach to Campaign Strategies and Tactics," in Jennings and Zeigler, *op. cit.,* pp. 1–20, at p. 4. See also, Charles O. Jones, "The Role of the Campaign . . . ," in *ibid,* pp. 33–34. Cf. Froman, *Congressmen And Their Constituencies* (Chicago, 1963), pp. 50–59.

[4] Warren Miller and Donald E. Stokes, "Constituency Influence in Congress," this REVIEW (1963), 45–56; Stokes and Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly,* 26 (1962), 531–546.

[5] E.g., in 1965, the House Democratic caucus stripped John Bell Williams and Albert Watson of their committee seniority because they openly supported the candidacy of the Republican Presidential nominee, Barry Goldwater. In 1969, John Rarick received the same punishment (from an unusual second caucus) for endorsing the third party candidacy of George Wallace.

portion of his electorate. Moreover, the infrequency of party punishment of disloyal candidates may be at least partly an unspoken acknowledgment of the necessity or at least comprehensibility of such action. The question then must be asked whether party loyalty may be so systematically costly as to *justify* evasion or violation of that norm.

I assume that a Congressional candidate will risk the consequences of disloyalty (which may include formal punishment, ostracism and alienation or disaffection of existing or potential support groups—in or out of the constituency) only in order to avoid the loss of votes consequent upon his being identified with his party's presidential candidate by his electorate.[6] If such behavior has any systematic effect, it must be that those legislative candidates most closely identified with their party's Presidential candidate will achieve (or suffer) the latter's electoral fate in a direct relationship to that identification.

In the United States, the clarity and ubiquity of behavior necessary to test such a proposition is a rare occurrence. When, as is usually the case, all or nearly all legislative candidates are decently loyal to their ticket, the necessary data are absent, by definition. In 1964, however, the conditions were met to an extent seldom before encountered. The Republican Party nominated Barry Goldwater, a conservative ideologue, for President. He was, moreover, a man with a clear and consistent public record and one whose electoral prospects were poor enough to prompt a wide variety of intra-party campaign behaviors, ranging from absolute endorsement and continuous support—the ideal application of the loyalty norm—to outright rejection and repudiation.[7]

The gross costs of the Goldwater candidacy are well known. He received 38.7% of the national two-party vote and, as part of a general Republican debacle, 39 of the 159 Republican Congressmen running for re-election were defeated. I hypothesize that the relationship between the Presidential vote and the patterns of first differences in the Congressional vote (i.e., between 1964 and 1962), across districts, is dependent upon the supportive association between Republican Congressmen and their party's Presidential candidate. Hence, where such support can be demonstrated, I predict a high positive correlation between the Presidential vote and changes in the Congressional vote. I will use the movement of the vote for Congress between 1962 and 1964 as the best available measure of the short-term forces swaying the electorate in these districts in the latter year. If the Congressman can dissociate himself from the Presidential contest, the correlation should diminish, approaching—in the ideal case of non-support—statistical (and empirical) independence. Thus, the absolutely loyal would be tied directly to the coattails of the head of their ticket while the variation in the results for those candidates intent on strategic maximization would tend to be unrelated to the vote for the Presidential candidate. The null hypothesis would imply that variation in this aspect of campaign behavior had no discernible effect on either voting trends or outcomes.[8]

II. THE EFFECTS OF LOYALTY

The techniques of expression or evasion of party loyalty take many forms. A candidate may identify himself with a slate or head of his ticket willingly and consistently; he may offer honest support but attempt to "run on [his] own record;" offer *pro-forma* support—occasionally or once—and then proceed to differentiate himself as much as possible; simply ignore his running-mates; judiciously praise *competing* candidates; publicly withhold endorsement; or repudiate a partisan colleague's candidacy. (These behaviors are not always mutually exclusive nor is this list necessarily inclusive: it is fixed only by the limits of political inventiveness.) The subtlety and ambiguity of the intermediate behaviors, as well as the unavailability of the type of information which might permit refinement of such categories, necessitate the use of a more limited discrimination among degrees of support. Happily, the results of the analysis lend support to the belief that my classification of candidates by the degree of their support for Goldwater was not in fact compromised by excessive errors.

I have divided incumbent Republican Congressional candidates in 1964 into two groups: signers and non-signers of a public pre-convention endorsement of Senator Goldwater's

[6] His personal motives may in fact vary but are here assumed to be exclusively fear of defeat.

[7] Among those who publicly refused their support—at the very least—were such Congressional party colleagues as Senators Scott, Javits and Keating and Congressmen Lindsay, Tupper, Fulton and Halpern.

[8] These hypotheses also permit, simultaneously, a test of the existence and direction of the coattails effect. In the absence of survey data one can only infer, as I do here, that it is a *Presidential* rather than a *Congressional* Coattail effect that is involved. See Warren E. Miller, "Presidential Coattails: A Study in Political Myth and Methodology," *Public Opinion Quarterly*, 19 (Winter, 1955–56), 353–368.

candidacy.[9] I assume that the signers were that set of Congressmen most willing to be publicly identified with Senator Goldwater and that this behavior was merely an early example of a consistent equivalent set which occurred later in the campaign. It is they, contrasted with the more behaviorally heterogeneous non-signers, whose vote change patterns should vary directly with the Goldwater vote.

This proposition is confirmed. Among the signers the correlation between the presidential vote and changes of the congressional vote is +0.49. In view of the many purely local influences on Congressional races, the correlation is impressive. Moreover, there are technical reasons for believing that it understates the true impact which the presidential contest had on the signers' changing fortunes at the polls.[10]

For the 109 non-signers,[11] the presumed hope for an "independent" electoral judgment was achieved, in the aggregate. The correlation coefficient was −0.01. That is, variation in the Goldwater vote is uncorrelated with and explains *none* of the variance in the vote differences between 1964 and 1962 for the non-signing Republicans.

The possibility remains, however, that these findings reflect other immediate stimuli or structural influences upon voting behavior. For example, there exists some evidence indicating that party column ballots tend to discourage split-ticket voting and that this phenomenon is, by comparison, encouraged by the office-bloc ticket.[12] It is thus possible, in principle, that candidates whose vote change tended to parallel the trend of the Goldwater vote were disproportionately likely to represent districts in party-column states and that independence of that trend occurred generally where the office-bloc ballot was employed.

The addition of controls for ballot form elicits clear results (see Table 1). Where signers ran in Party Column states, the correlation coefficient between the Goldwater vote and Congressional vote-change rises to +0.60. For the matching non-signers, the relationship remains almost perfectly independent ($r = +0.002$). But for signers appearing on office Bloc ballots, the correlation coefficient decreases to +0.22, while for the equivalent set of nonsigners, the relationship is again one of independence ($r = -0.05$).

[9] The endorsement, published in the *New York Times*, June 17, 1964, was signed by 54 Republican Congressmen, 49 of whom were candidates for re-election. It read, in part, "We are convinced that the nomination of . . . Barry Goldwater . . . will result in substantial increases in Republican membership in both Houses of Congress."

[10] Since the redistricting of seats between 1960 and 1962 (and the abnormality of the 1960 presidential election) prevents the use of *changes* of the presidential vote to measure the forces distinctive to 1964, the presidential measure entered in the correlation reflects traditional levels of party strength in the several districts as well as the influence of Goldwater's candidacy and other circumstances specific to 1964. Such an extraneous traditional component tends to reduce any non-vanishing correlation below the level which would be obtained were it possible to correlate changes of the congressional vote with *changes* of the presidential vote.

[11] Representative Sylvio Conte (Mass. 1) is excluded from this analysis, since he gained both Republican *and* Democratic nominations and received 100% of the 2-party vote in his district. Conte was one of those who repudiated Goldwater's nomination, but his inclusion would add spurious strength to the second hypothesis, since his vote could not be other than independent of Goldwater's.

[12] See V. O. Key, Jr., *Politics Parties and Pressure Groups* (New York, 4th edition, 1958), p. 609n, 691–695; Angus Campbell and Warren Miller, "The Motivational Basis of Straight and Split Ticket Voting," this REVIEW, 60 (1957), 293–312. See also Jack L. Walker, "Ballot Forms and Voter Fatigue," *Midwest Journal of Political Science*, 10 (1966), 448–463.

TABLE 1. THE EFFECT OF BALLOT FORM AND SUPPORT FOR GOLDWATER ON THE RELATIONSHIP BETWEEN THE GOLDWATER VOTE AND CONGRESSIONAL VOTE-CHANGE

	Signers		Non-signers		Totals	
	r	(n)	r	(n)	r	(n)
Party Column	.60	(33)	.00	(68)	.17	(101)
Office Bloc	.22	(16)	−.05	(41)	−.03	(57)
Totals	.49	(49)	−.01	(109)		

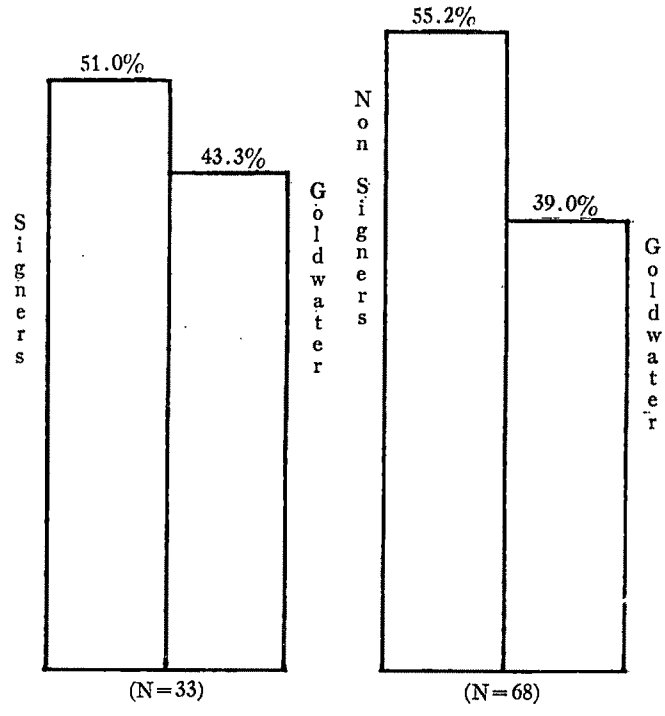


FIG. 1. Mean District Vote Percentage on Party Column Ballots for Republican Congressmen and Barry Goldwater, 1964, by Support for Goldwater.

Thus, the Party Column ballot had the effect of strengthening the relationship between the Goldwater vote and Congressional vote-change, but *only* for the group of early Goldwater supporters. The Office Bloc ballot, by contrast, permitted Congressmen to remain largely independent of the trend of the Goldwater vote.

It is in Party Column states where the divergent campaign strategies of Republican incumbents led to now predictably divergent results. The signers of the Goldwater endorsement were able to maintain, on the average, only a +7.7% margin over the Arizona Senator.[13] Among non-signers, the slippage was +16.2% (see Figure 1).

Faced with an even lower Goldwater vote, typically, than were their signing colleagues, the non-signers not only obtained a higher mean vote percentage, but also tended to suffer smaller losses, compared with 1962,[14] against a stronger tide. Their vote changed, on the average, by −5.3% while the mean change among signers was −6.8%.[15]

The political arithmetic becomes crucial when strategic vulnerability is linked to electoral marginality. Although, as would be expected, the overwhelming majority of defeats were sustained by Republicans occupying marginal seats (those who won with less than 60% of the vote in 1962),[16] the Party Column signers bore a disproportionate share of the burden (see Table 2). Comprising 30% of all marginal Congressmen, they suffered 47% of the losses; making up 35% of such candidates on Party Column ballots, they suffered 56% of the defeats.

There is also, in these figures, a touch of pa-

[13] The "power of incumbency" thesis has never been better demonstrated. Only one Republican Congressmen, Bruce Alger (Texas 5), trailed Goldwater.

[14] Calculations for Congressmen Clausen and Clawson (Calif 1 and 23) are based on the results of Special Elections in 1963.

[15] Whereas strategic difference explains systematic differences on Party Column ballots, the same operation accounts in gross terms for differences on Office Bloc tickets. Here, the mean slippage for signers and non-signers was +9.7% and +17.9%, respectively, and mean vote-change was —4.4% and —2.2%. The original hypothesis thus holds, but only between groups and without explaining internal variation among signers.

[16] Although 55% is commonly employed as the border between marginality and safety, in 1964 relative safety for incumbent Republicans existed on the positive side of 60% rather than 55%.

TABLE 2. MARGINALITY, SAFETY AND THE DISTRIBUTION OF LOSSES IN 1964

Vote in 1962

		<60%			60%+			Totals		
		%	(n)	(Losses)	%	(n)	(Losses)	%	(n)	(Losses)
Signers	PC	72	(24)	(14)	28	(9)	(2)	100	(33)	(16)
Non-Signers	PC	50	(34)	(11)	50	(34)	(5)	100	(68)	(16)
Signers	OB	44	(7)	(1)	56	(9)	(—)	100	(16)	(1)
Non-Signers	OB	39	(16)	(4)	61	(25)	(2)	100	(41)	(6)

thos. Those Congressmen holding marginal seats in Party Column states, and therefore *least* able to pay the penalty which endorsing Goldwater was likely to exact, were *more* likely to be among those endorsers. Forty-one per cent of these marginal Congressmen signed the Goldwater endorsement; only 21% of the safe ones took that risk. The tendency of these signers to hold marginal seats means that their higher *rate* of loss yielded even more actual numbers of signers who were put out of the House.

In short, since the presidential race went strongly against the Republicans in most districts, the displacement of popular hostility to Goldwater onto the Congressmen who supported him most strongly cost the party a good many seats. Among Congressmen who had won with less than 60% of the vote two years before, the proportion defeated in 1964 was well over half again greater among signers than non-signers (48% *versus* 30%). Among such Congressmen whose states employed the Party Column ballot, that proportion of defeats was nearly twice as great (58% *versus* 32%). This contrast is lent added significance by the fact that signers were notably more likely to hold marginal seats; the price was greatest for those who were, on the average, least able to pay it.[17]

[17] It is possible to suggest that the differences found between signers and non-signers arise from the fact that 1964 was a bad year for conservatives, which signers tended to be and Goldwater was. It is necessary to point out that such a hypothesis will not account for the lack of correlation between the Goldwater vote and the changes of support for non-signers, unless they are all uniformly unconservative (while signers are of varying degrees of conservatism, a strange notion). As it happens, the conservatism of signers—or of all Congressmen is unrelated to the change of their support between 1962 and 1964. Employing Congressmens' scores on a measure combining Congressional Quarterly's Conservative Coalition and Federal Role Indexes, the correlation coefficient between relative conservatism in roll-call voting behavior and change of support is —0.08 among signers, —0.11 among non-signers and —0.20 among all Congressmen.

III. DISCUSSION

The belief that campaign behavior is of exceedingly small importance in determining electoral outcomes is grounded primarily in the discovery that factors beyond the control of the candidates, such as incumbency, the partisan distribution of the electorate and general economic and social conditions tend to determine the results. The absence of a literature describing systematic or even widespread, if dissimilar, deviations from this standard expectation has merely served to reinforce its acceptability.

Yet, in at least one election year, 1964, a large number of incumbent Republican Congressmen were confronted by a strategic campaign choice—whether, when and how to endorse the Presidential candidacy of Barry Goldwater. The nature of the decision taken had demonstrable and often crucial effects on the proportion of the vote they themselves received in their districts.

Those Congressmen who offered early and absolute support for the then certain head of their ticket were destined, most clearly when running on Party Column ballots, to be firmly bound to his coattails on election day. Those who refused, qualified or simply withheld until a later date their support for Goldwater's candidacy tended, in the aggregate, to be judged by their constituents on the basis of other considerations or to be rewarded, in varying degree, for their "independent" behavior.

These findings should be most disturbing to some theorists, both positive and normative, of party behavior. For certain varieties of positive theory, they underscore the limits to the empirical usefulness of models that assume that party leaders always act in common. Anthony Downs, for example, assumes that parties are internally unified and forbids *by axiom* any attempt by a

party member ". . . to benefit himself at the expense of any other member of his own party team."[18] An awareness that political leaders (i.e., politicians) act for private motives ought to prompt an extension of models of rational behavior, just as an awareness that public authority is wielded for private motives led Downs, Buchanan, Tullock and other economists to become interested in politics in the first place.

The normative theorists of party government must in turn wrestle with the likely results had all Republican Congressmen put the party's welfare before their own. *That* cohesive and disciplined legislative party, responsible to the party platform[19] and loyal to the Presidential candidate who presumably embodies it, would have been far more vulnerable to defeat than in fact they were. The trends illustrated above and the demonstrated unpopularity of Goldwater in the electorate[20] leave little doubt that the Republican Party in the House would have been little more than a corporal's guard. It is difficult to believe that this is what the party reformers envisioned as the second responsible party.

There is, quite clearly, a sense in which it was both rational *and* responsible for Republican candidates in 1964 to dissociate themselves from the ticket and cut the party's grievous losses in Congress. It was, by some standards, inelegant of them to do so but, for politicians who must live in this world, the assumptions of political modelers need be of no concern—only the reverse is true—and the prescriptions of normative system-builders may be, certainly at the personal level, absolutely dangerous.

[18] Anthony Downs, *An Economic Theory of Democracy* (New York, 1957), p. 30.

[19] See, e.g., The Committee on Political Parties, American Political Science Association, "Toward a More Responsible Two-Party System," this REVIEW, 44 (1950, Supplement), pp. 21, 47, 48, 53, and 61; James M. Burns, *The Deadlock of Democracy,* (Englewood Cliffs, N.J., 1963). The broader assumptions underlying and the prospective consequences of the recommended reforms are discussed critically in Julius Turner, "Responsible Parties: A Dissent from the Floor," this REVIEW, 45 (1951), 143–152; Austin Ranney, "Toward a More Responsible Two-Party System: A Commentary," in *ibid,* 488–499. See also, Austin Ranney and Willmoore Kendall, *Democracy and the American Party System,* (New York, 1956), pp. 527–533; and Nelson W. Polsby and Aaron Wildavsky, *Presidential Elections* (New York, Second Edition, 1968), pp. 218–228, 250–268.

[20] Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," this REVIEW, 60 (1966), pp. 19–28, esp. pp. 22–23.

THE DESIRED POLITICAL ENTROPY

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I. INTRODUCTION

In an article published in the December 30, 1967 issue of the Dutch weekly magazine *Vrij Nederland*, Mr. Frans Grosfeld suggested the following modification of the procedure of parliamentary representation of political parties. Instead of the conventional method of allocating to each party a number of representatives proportional to the number of votes cast for the party, he proposed to allocate a number of representatives which is proportional to the square of the number of votes. Thus, when there are n parties and when $p_1, \cdots, p_n$ are the proportions of the total number of votes obtained by these parties, the idea is to allocate a fraction q_i of the representatives to the ith party determined by

$$(1.1) \qquad q_i = p_i^2 / \sum_{j=1}^{n} p_j^2 \qquad i = 1, \cdots, n$$

Mr. Grosfeld made this suggestion for the Dutch political system, because in his view n is so large and even the largest p_i belongs to a minority party, so that on many occasions several months of negotiations were necessary to obtain a coalition cabinet of one form or another. By squaring the p_i's one obtains much larger relative differences between the larger and the smaller parties, and one party or a pair of politically related parties then has a much better chance of obtaining a parliamentary majority. Also, the system (1.1) provides an incentive for parties to merge, which contributes to a reduction of the degree to which the country is politically divided. This system should be contrasted with another system which is applied on a rather large scale, viz., that of declining any seat to those parties whose p_i is less than a pre-assigned fraction θ (for example, $\theta = .03$). The fractions in the parliament are then:

$$(1.2) \qquad \begin{array}{ll} q_i = 0 & \text{if } p_i < \theta \\ p_i/(1 - A) & \text{if } p_i \geq \theta \end{array}$$

where $A = \sum p_j$, the summation being extended over those values of the subscript j for which $p_j < \theta$.[1] The cut-off system (1.2) also discriminates against small parties, but the system (1.1) proceeds in a more continuous manner.

It is interesting in this connection that the International Federation of Operational Research Societies (IFORS) faces a similar problem but in precisely the opposite direction. The problem is that the national member societies are of very unequal size, so that IFORS could be dominated more or less permanently by the societies of one or two countries if proportional representation based on national membership were applied. To prevent this, a rule has been adopted which gives each member society a number of votes proportional to the square root of the number of members:

$$(1.3) \qquad q_i = p_i^{1/2} / \sum_{j=1}^{n} p_j^{1/2} \qquad i = 1, \cdots, n$$

where n is the number of member societies, p_i the ith society's share of total membership, and q_i its share in the IFORS voting procedure. Thus, if the ith society has four times as many members as the jth, the number of its votes is only twice as large.

The system of ratios squared (1.1) and the square-root system (1.3) are both special cases of

$$(1.4) \qquad q_i = p_i^{\alpha} / \sum_{j=1}^{n} p_j^{\alpha} \qquad i = 1, \cdots, n$$

A specification $\alpha > 1$ serves to raise the size of the larger groups and to reduce that of the smaller groups, whereas $\alpha < 1$ has the opposite effect. The latter variant is sometimes used to protect geographical minorities. The IFORS case is one example; the U.S. Senate is another, since it gives equal representation to all 50 states independent of their population. Thus, if p_i stands for the number of voters of the ith state measured as a fraction of the national total and q_i for the proportion of Senators allocated to this state (1/50), (1.4) applies to this case when we specify $\alpha = 0$.

[1] Since the number of representatives allocated to each party must be a nonnegative integer, the actual q_i's will be slightly different from those defined in (1.1) and (1.2). This feature will be disregarded in the present note, but it should be stressed that the criterion used in Section 2—the minimization of the information expectation defined in eq. (2.4)—can be used to handle this problem. The approach is then to minimize the information expectation subject to the constraint that each q_i be a nonnegative multiple of $1/N$, where N is the total number of parliamentary seats.

There are thus several examples of the representation system (1.4): $\alpha=0$ (U.S. Senate), $\alpha=\frac{1}{2}$ (IFORS), $\alpha=1$ (proportional representation), and $\alpha=2$ (Mr. Grosfeld's proposal).[2] This makes the system (1.4) interesting by itself, but it is clear that this particular one-parameter family is only one choice out of many. In this note I shall show that the family of representation systems (1.4) has certain optimal properties. In Section 2 I shall derive this family by means of a constrained optimization technique based on some elementary concepts from information theory. In Section 3 I will derive the same family from some simple axioms, after which some concluding remarks follow in Section 4.

II. AN INFORMATIONAL APPROACH

There can be no doubt that the representation system $q_i=p_i$ is the simplest and most straightforward, and the reason for choosing a different system is basically the same in all cases discussed above: One is dissatisfied with the degree to which the total representation is divided into parts, and the "dividedness" is either regarded as too large, in which case one would like to reduce it (by choosing $\alpha>1$ in the system (1.4)), or it is regarded as too small. Therefore, what is needed in the first place is a measure which describes the degree to which the total is subdivided. A natural and well-known measure, based on concepts from information theory, is the *entropy*.[3] For the fractions $p_1, \ldots, p_n$ the entropy is defined as

$$H_p = -\sum_{i=1}^{n} p_i \log p_i \tag{2.1}$$

which is nonnegative and which vanishes if and only if $p_i=1$ for some i (and hence $p_j=0$ for $j\neq i$). The maximum is $\log n$, which is attained when the p_i's are all equal to $1/n$. Thus, the entropy takes the smallest value when the scene is dominated completely by one group (which amounts to a minimum of "dividedness") and the largest value when all groups are of equal size; and this maximum, $\log n$, increases in turn when the number of groups (n) increases. We shall exclude the possibility that H_p is equal to either limit:

$$0 < H_p < \log n \tag{2.2}$$

If $H_p=0$, then some p_i is equal to one and all others are zero. Clearly, there exists no non-arbitrary way of assigning parliament seats to parties which did not receive any vote at all. If $H_p=\log n$, all n parties received the same number of votes, and there exists no reasonable way of allocating parliament seats to these parties on another than equal basis.

But if H_p satisfies the constraint (2.2), as is normally the case, one can conceive of a set of parliamentary fractions $q_1, \ldots, q_n$ whose entropy

$$H_q = -\sum_{i=1}^{n} q_i \log q_i \tag{2.3}$$

differs from H_p. Obviously, if we fix H_q at a pre-assigned level, we still have freedom as to the choice of $q_1, \ldots, q_n$ when $n\geq 2$,[4] and we would like to choose the q's as closely to the corresponding p's as is possible. Given that the informational concepts H_p and H_q are used to measure "dividedness," a natural way of specifying "as close as possible" is the following. Consider $p_1, \ldots, p_n$ as the prior probabilities of n events and imagine that a message arrives which indicates that the odds in favor of these events have changed to the extent that the new (posterior) probabilities are $q_1, \ldots, q_n$. The expected information of this message is defined in information theory as

$$I(q:p) = \sum_{i=1}^{n} q_i \log \frac{q_i}{p_i} \tag{2.4}$$

where q and p on the left stand for $q_1, \ldots, q_n$ and $p_1, \ldots, p_n$, respectively. The information expectation is never negative. It vanishes if and only if $p_i=q_i$ for each i, and it takes larger and larger values when the p's and q's are pairwise more different.[5]

[2] Plus the case $\alpha=\infty$ (i.e., allocating all seats to the ruling party), a procedure used in some of the democratically less advanced countries.

[3] For a brief introduction to the informational concepts used in this section, see e.g. Chapters 1 and 2 of the author's *Economics and Information Theory* (Chicago: Rand McNally and Company, and Amsterdam: North-Holland Publishing Company, 1967). The idea of using the entropy for this particular purpose is due to Mr. B. M. S. van Praag of the Econometric Institute in Rotterdam.

[4] Even for $n=2$ the entropy does not determine the q's uniquely, because $(\theta, 1-\theta)$ and $(1-\theta, \theta)$ have the same entropy for any θ in the interval (0, 1). It is obvious, however, that the present discussion loses much of its interest when $n=2$ instead of $n>2$.

[5] This becomes particularly clear when $I(q:p)$ is expanded according to powers of $(q_i-p_i)/p_i$. If we use natural logarithms (which is done throughout this paper) and disregard third and higher powers, the result is

$$I(q:p) \approx \frac{1}{2}\sum_{i=1}^{n} \frac{(q_i-p_i)^2}{p_i}$$

provided that the p's and q's are pairwise suffici-

The information expectation (2.4) is a measure of the degree to which we are surprised when we are informed that the prior probabilities $p_1, \ldots, p_n$ are replaced by the posterior probabilities $q_1, \ldots, q_n$. Interpret now the p_i's again as the proportions of the votes cast for the various parties and the q_i's as the corresponding proportions of parliamentary seats.[6] Obviously, the electorate ought to be surprised as little as possible. Consider then:

THEOREM 1. *Let $p_1, \ldots, p_n$ be the proportions of the total number of votes cast for the n parties and suppose that* (2.2) *is satisfied. Then the parliamentary representation $q_1, \ldots, q_n$ which minimizes the information expectation* (2.4) *subject to the entropy constraint* (2.3) *is given by eq.* (1.4), *where α is the solution of eq.* (2.11) *below. This solution exists and is unique for any preassigned value of the entropy H_q in the interval* (0, log n) *if exactly one party has the largest number of votes.*

The remainder of this section is concerned with the proof of this theorem. We note first that under condition (2.3) the information expectation (2.4) can be written as

$$I(q:p) = -H_q - \sum_{i=1}^{n} q_i \log p_i \tag{2.5}$$

so that we should minimize $-\sum q_i \log p_i$ subject to the constraints (2.3) and $\sum q_i = 1$. So we form the Lagrangian expression:

$$-\sum_{i=1}^{n} q_i \log p_i - \lambda\Big(-\sum_{i=1}^{n} q_i \log q_i - H_q\Big) - \mu\Big(\sum_{i=1}^{n} q_i - 1\Big) \tag{2.6}$$

where λ and μ are Lagrangian multipliers. We differentiate with respect to q_i and equate the result to zero:

$$-\log p_i + \lambda(1 + \log q_i) - \mu = 0 \qquad i = 1, \cdots, n \tag{2.7}$$

If $\lambda = 0$, this amounts to $\log p_i = -\mu$, which implies that all p_i's must be equal to $1/n$ and hence $H_p = \log n$. This, however, is excluded by (2.2). So $\lambda \neq 0$ and hence:

$$\log q_i = -1 + \frac{\mu}{\lambda} + \frac{1}{\lambda} \log p_i \qquad i = 1, \cdots, n \tag{2.8}$$

or equivalently:

$$q_i = \frac{p_i^{1/\lambda}}{C} \quad \text{where } C = e^{1-\mu/\lambda} \tag{2.9}$$

We now write $\alpha = 1/\lambda$, so that $q_i = p_i^\alpha / C$ in accordance with (1.4), and C obviously satisfies

$$C = \sum_{i=1}^{n} p_i^\alpha \tag{2.10}$$

in view of the sum constraint $\sum q_i = 1$.

Next substitute p_i^α / C for q_i in the entropy constraint (2.3):

$$H_q = -\sum_{i=1}^{n} (p_i^\alpha / C) \log (p_i^\alpha / C) = \frac{-\sum_{i=1}^{n} p_i^\alpha \log p_i^\alpha + \Big(\sum_{i=1}^{n} p_i^\alpha\Big) \log \sum_{i=1}^{n} p_i^\alpha}{\sum_{i=1}^{n} p_i^\alpha}$$

or

$$H_q = \log \sum_{i=1}^{n} p_i^\alpha - \frac{\alpha \sum_{i=1}^{n} p_i^\alpha \log p_i}{\sum_{i=1}^{n} p_i^\alpha} \tag{2.11}$$

For given H_q, $p_1, \ldots, p_n$ this is a nonlinear equation in α. The righthand side is equal to $\log n$ for $\alpha = 0$ and to zero in the limit for α increasing indefinitely, provided that one party has the largest p_i.[7] To prove that (2.11) gives a unique solution for any H_q between these limits it is sufficient to show that the righthand side is a monotonically decreasing function of α. So we consider its derivative with respect to α, which turns out to be:

$$-\frac{\alpha \sum_{i=1}^{n} p_i^\alpha (\log p_i)^2}{\sum_{i=1}^{n} p_i^\alpha} + \frac{\alpha\Big(\sum_{i=1}^{n} p_i^\alpha \log p_i\Big)^2}{\Big(\sum_{i=1}^{n} p_i^\alpha\Big)^2} = -\alpha \sum_{i=1}^{n} q_i \Big(\log p_i - \sum_{j=1}^{n} q_j \log p_j\Big)^2 \tag{2.12}$$

The expression on the second line is a multiple $-\alpha$ of the weighted variance of $\log p_1, \ldots, \log$

---

ently close to each other (so that the expansions converge). The right-hand side of this approximation is proportional to a chi-square with the p's as theoretical probabilities and the q's as observed frequencies.

[6] To make the discussion more concrete, we will talk about parties and parliamentary seats rather than about both these and the geographic representations such as those of IFORS and the U.S. Senate.

[7] Write p_1 for the largest p_i, then q_1 converges to 1 as $\alpha \to \infty$, so that $H_q = 0$ in the limit. If $p_1 = p_2$ and if all other p_i's are smaller, then $q_1 = q_2 = \frac{1}{2}$ in the limit and hence $H_q > 0$ even when $\alpha = \infty$. In the case of a k-fold multiple maximum, $p_1 = \ldots = p_k > p_{k+1} \geq \ldots \geq p_n$, the interval for H_q mentioned in Theorem 1 should be confined to (log k, log n).

p_n with $q_1, \ldots, q_n$ as weights.[8] The derivative is therefore negative for all positive α except when the variance vanishes, and this variance vanishes only if $p_i = 1$ and hence $q_i = 1$ for some i ($H_p = 0$) or if $p_i = 1/n$ for each i ($H_p = \log n$).

III. AN AXIOMATIC APPROACH

In the system of proportional representation the number of parliamentary seats allocated to each party, measured as a fraction of the total number of seats, is determined uniquely by the number of party votes relative to the total number of votes. In fact, the rule is simply $q_i = p_i$, $i = 1, \ldots, n$. Other systems such as (1.4) do not have this property, but a weaker variant, which may be called the condition of *weak proportionality*, certainly seems reasonable. Consider any two parties i and j; then the number of parliamentary seats allocated to i relative to the number of seats allocated to j should be determined uniquely by the ratio of the number of votes cast for i to that cast for j, and the underlying relationship should be the same for all pairs (i, j) and it should be nondecreasing. This means:

$$(3.1) \qquad \frac{q_i}{q_j} = f\left(\frac{p_i}{p_j}\right) \quad i, j = 1, \cdots, n$$

where $f(\ )$ is a nondecreasing function which takes positive values for all positive values of the argument.

Consider then any triple of parties, (i, j, k). We obviously have

$$(3.2) \qquad \frac{p_i}{p_k} = \frac{p_i}{p_j}\frac{p_j}{p_k} \qquad \frac{q_i}{q_k} = \frac{q_i}{q_j}\frac{q_j}{q_k}$$

which means $f(p_i/p_k) = f(p_i/p_j)f(p_j/p_k)$. Since this equation holds for any positive values of the arguments, we may write it as

$$(3.3) \qquad f(x)f(y) \equiv f(xy) \quad x, y > 0$$

Write $\xi = \log x$ and $\eta = \log y$, so that

$$(3.4) \qquad \log f(x) \equiv \log f(e^{\xi}) \equiv \phi(\xi) \quad \text{say}$$

Taking logarithms on both sides of (3.3) we obtain

$$(3.5) \qquad \phi(\xi) + \phi(\eta) \equiv \phi(\xi + \eta) \quad -\infty < \xi, \ \eta < \infty$$

from which it follows immediately that, if $f(\ )$ and hence $\phi(\ )$ are continuous functions, $\phi(\xi)$ must be of the form $\alpha\xi$ for some constant α.[9] Hence:

$$(3.6) \qquad f(x) \equiv e^{\phi(\xi)} \equiv (e^{\xi})^{\alpha} \equiv x^{\alpha}$$

so that we obtain:

$$(3.7) \qquad \frac{q_i}{q_j} = \left(\frac{p_i}{p_j}\right)^{\alpha} \quad i, j = 1, \cdots, n$$

This condition determines the ratios of all q's. Since they add up to one, they are thus individually determined and it is immediately obvious that they are of the form (1.4). The condition that $f(\ )$ be nondecreasing implies $\alpha \geq 0$. We have thus proved:

THEOREM 2. *Let $p_1, \ldots, p_n$ be the proportions of the total number of votes cast for the n parties and $q_1, \ldots, q_n$ the corresponding parliamentary representation. If weak proportionality is imposed in the sense of* (3.1), *where $f(\ )$ is a continuous nondecreasing function satisfying $f(x) > 0$ for any $x > 0$, then the q's are determined by the p's as described in eq.* (1.4) *for some $\alpha \geq 0$.*

IV. CONCLUDING REMARKS

The results obtained in the two previous sections indicate that, if one is dissatisfied with proportional representation, there are rather cogent reasons for choosing a member of the family (1.4). The next problem is then the specification of the parameter α. In Mr. Grosfeld's proposal the value 2 is chosen a priori, but it may be argued that the voters should have a say in the matter. In fact, a plea can be made for the idea that the citizens of a country should have the right to vote on the degree to which they will be politically divided in their own Parliament. This would give the voting procedure a two-dimensional character: Each citizen votes for the party of his preference, thus raising the corresponding p_i at the expense of all other p's, and he also votes for a particular numerical value of the parliamentary entropy,[10] thus indicating how in his view

[8] By substituting $\log q_i$ for $\alpha \log p_i - \log C$ in the second line of (2.12) we obtain a result that can be written as follows:

$$\frac{\partial H_q}{\partial(\log \alpha)} = -\sum_{i=1}^{n} q_i(-\log q_i - H_q)^2 \quad \alpha > 0$$

which means that a small relative increase in α reduces the entropy H_q by k times this increase, where k is the variance of the distribution whose values are $-\log q_1, \ldots, -\log q_n$ and whose probabilities are $q_1, \ldots, q_n$. Obviously, the mean of this distribution is the entropy H_q.

[9] For details, see e.g. *Economics and Information Theory* (quoted in footnote 3), p. 7, or B. O. Koopman and G. E. Kimball, "Information Theory," Chapter 9 of *Notes on Operations Research 1959*, assembled by the Operations Research Center, Massachusetts Institute of Technology (Cambridge, Mass.: The Technology Press, M.I.T., 1959).

[10] This will of course require the teaching of logarithms at an early stage. Mathematics teachers should regard it as a challenge to teach a subject with immediate political science applications.

$p_1, \ldots, p_n$ should be transformed to the parliamentary fractions $q_1, \ldots, q_n$. In theory, there will be as many different desired entropy values as there are voters; an obvious choice is to substitute the median for H_q in the entropy constraint (2.3). One may also argue that the voters collectively decide on the fractions $p_1, \ldots, p_n$ and that, therefore, H_p is the implied desired entropy, which is normally different from the median of the "directly" desired entropies. This conflict can be resolved by substituting for H_q in (2.3) the average of H_p and the median desired entropy. Clearly, there are many possibilities.

It is also important to reflect on the degree of association between the two components of each citizen's vote. A priori, one may be inclined to speculate that, if a person votes for a large party, there is a large chance that he will vote for a small entropy. Specifically, let us arrange the parties according to decreasing size:

$$p_1 \geq p_2 \geq \cdots \geq p_n \tag{4.1}$$

and let us write π_i for the probability that the person votes for an entropy $<H_p$, given that his vote goes to the ith party. The above speculation in more specific form is then:

$$\pi_1 \geq \pi_2 \geq \cdots \geq \pi_n \tag{4.2}$$

The proportion of votes for an entropy $<H_p$ is then $\pi_1 p_1 + \ldots + \pi_n p_n$, which may be quite high because the p's and π's are positively correlated according to (4.1) and (4.2). Thus, the presumption indicates that there will normally be a tendency for the median desired entropy to be on the low side, which favors the largest party.

The underlying assumptions are questionable, however. First, there are many situations in which it is far from certain which party will be the largest. If i and j are the leading contenders, and if a voter favoring i tries to help this party by voting for a small entropy, his efforts will be in favor of j rather than i if p_j turns out to be larger than p_i. Second, even if a voter prefers a party which in his view will definitely be the largest, he may nevertheless be in favor of a fairly large parliamentary entropy when he is violently against the second-largest party and when he wants a coalition cabinet of his preferred party with one or two smaller parties. Voting for the parliamentary entropy requires that the citizen consider the political structure as a whole. This is an appeal to his intelligence, which has its merits in and by itself.

COMMUNICATIONS

To the Editor:

I had looked forward to reading Ernest A. Duff's and John F. McCamant's "Measuring Social and Political Requirements for System Stability in Latin America," in the December Review, since this is an area in which we are now in a position to make real progress, and since McCamant had previously written with some insight on political development theory as applied to Latin America. The article itself was a great disappointment, however, and I feel I should point out some of its limitations, because readers unfamiliar with Latin America, and impressed by the authors' tables and their "prediction" that revolution was imminent in Cuba in 1968, might otherwise be inclined to take the article's conclusions seriously.

As it happens, this is rather difficult to do, since major predictions made in it were being falsified even while the article was in press. A military *coup* imposed a dictatorship on Panama, rated by Duff and McCamant the second highest country in Latin America in system stability, and predicted to enjoy "continued democratic stability with minor difficulties;" Brazil, in which "a semi-democratic system should be able to survive the minor manifestations of discontent," experienced a shift within the ruling group which put an end to the "semi-democratic" facade and imposed a thoroughly repressive system.

If the other predictions made hold, it will be fortuitous. Some are clearly doomed on their face, such as that for the Dominican Republic: "The surface manifestations of discontent of 1965 will diminish. The military will continue to play a large political role but without need for extensive repression of political freedoms." And, notwithstanding last fall's demonstrations and their repression, Mexico remains a marvel of stability, even though Duff and McCamant only rated it 15th out of 20 countries.

This is not merely bad luck. The predictions are poor because there are several key weaknesses and discontinuities in the argument from which they are derived; weaknesses which, in fact, make it difficult to understand why the editor and the readers of the manuscript accepted it as it stands. These weaknesses go beyond mere factual inaccuracy, such as the nonsensical description of Ecuador as without institutionalized parties.

This is all highly unfortunate, because the basic theoretical premise of the article seems sound, that instability derives from an excess of demands, growing out of social mobilization, over satisfactions. But this sound beginning is vitiated by clumsiness in the construction and use of the two key indices. Despite the air of statistical exactness, this is still thumb-on-the-scale measurement. ("The authors decided that the political capabilities should be given equal weight to the environmental scores. The societal welfare minus social mobilization score was given twice the weight of the score on recent economic growth." p. 1130). In this kind of work, one is well advised either to maintain the chastity of one's numbers intact or alternatively to forgo using numbers altogether, and rely on judgment born of experience. *Demi-vierge* status is not viable.

More important, the measures used to construct the social welfare and social mobilization indices happen to measure fundamentally the same dimension—wealth—rather than two separate ones. Exasperatingly enough, the authors almost know this (". . . the correlation coefficient between the two indexes is .92." p. 1132). One can see this even *a priori:* the literacy rate measures social welfare, but newspaper circulation measures social mobilization. But we do have factor analysis to tell us to what extent measurements indicate separate dimensions, after all; thus the indicators best adapted to measuring a given dimension could have been discovered empirically. The authors' procedure is especially to be regretted, again, since they knew the danger they were running: "We restricted the two indexes as far as possible to criteria that were distinct." (p. 1132). They could have done this more effectively by consulting an indicator which they didn't use, the voting turnout figure, which happens to be the indicator of social mobilization least determined by the society's income level.

The upshot of all this is that the figures Duff and McCamant arrive at for the differential between welfare and mobilization are quite without significance. Despite their brave subsequent attempts at interpretation, which involve the injection of a little more subjectivity into the individual country profiles at the end of the article in the attempt to get them "right," these are therefore quite unreliable. Again, this is highly unfortunate, but also rather unnecessary, since an understanding of the stability picture in any one country, and estimates of future stability, are probably most reliably produced, without particular difficulty, by old Latin America hands who know the country in depth.

What we are really in need of, however, is not dubious predictions that are instantly falsified, but clarification of the relationships among social, economic, and political variables that lead to stability or instability. What should have been the starting point of the article became its conclusion. What should have been the conclusion was assumed away. Perhaps the APSR's manuscript readers will point some of these things out the next time an article like this is submitted, and we will all be better off.

MARTIN C. NEEDLER
University of New Mexico

TO THE EDITOR:

Professor Needler's use of language distracts from a useful scholarly dialogue, but a few comments on his critique might help to bring the discussion back into perspective. He claims (1) that our predictions have been proved wrong, (2) that our social welfare-social mobilization differential is useless because both measure wealth, and (3) that "old Latin American hands" can better estimate future stability.

Our predictions have not been proved wrong by recent events. The coup in Panama did not manifest any basic system instability as shown by the relative absence of violence, repression or social controversy. Our information on Brazil is not complete, but the present regime is certainly not a "thoroughly repressive system" by Latin American standards, and it now manifests its moderate instability through coercion instead of inflationary monetary and fiscal policy. In the Dominican Republic, the surface manifestations of discontent have very significantly diminished, as we predicted. If we had been able to quantify the political party situation and included it in the final measurement, Mexico would be ranked somewhat above 15th, but at any rate the demonstrations of last summer as well as periodic outbreaks of rural violence do not indicate that it is a "marvel of stability."

Yes of course, social welfare and social mobilization are both related to wealth and are highly correlated, but what is more significant is that in a few countries welfare is relatively higher than mobilization and in a few others mobilization is relatively higher than welfare. In these six or eight countries the differential is important. A non-synchronic comparison would reveal far greater variation. All Latin American countries along with other countries which have begun their economic progress in this century are in a much more difficult situation than were those countries which began development in an earlier period. We hold that the social welfare-social mobilization differential is a large advance in precision over the widely accepted notion of the demonstration effect.

Are "old hands'" impressions still better than a more systematic approach? We have had considerable experience in a few countries, but we do not consider our own impressions of these countries adequate in making any firm conclusion about their stability. In any case the comparative study that specifies and operationalizes its variables has the advantage of furnishing a base from which better theory can be built. But whose predictions have been better than ours? No "old hand" predicted the Cuban revolution, and we have seen no better *ex post* explanation of it than ours. The ranking we achieved of Latin American political systems does to a great extent correspond to the manifestations of their system stability/instability. Previous studies, instead of showing the important differences between Latin American countries, tried to lump them all together as unstable when clearly not all of these countries are equally confronted with "chaos," "revolution," "the eleventh hour" and the like. The weighting of factors is a problem, as Professor Needler rightly points out, but we can only repeat what we said in the article: more sophisticated mathematical manipulation can be carried out only when we have an independent measure of the manifestations of system stability/instability. We are working on that measure now and plan to publish a more complete work which will overcome many of the weaknesses of the article (most of which Professor Needler missed).

ERNEST A. DUFF
JOHN F. MCCAMANT
Randolph-Macon Woman's College
University of Denver

TO THE EDITOR:

In his recent lengthy article in the REVIEW (June '68) entitled "The House and the Federal Role . . ." Michael J. Shapiro developed an elaborate model to simulate voting in the Congress of the United States. We have no reason to question his model, nor even to question most of the methods and tests used to validate the effects of the model, but we do question his use of certain words and phrases that strongly imply a level of statistical proof which his own data do not support.

Perhaps our point can best be made by several quotations from the Shapiro article: "The *fact* that every alternative formulation of the model undertaken thus far has decreased some aspect of the model's performance lends evidence in support of the contention that the relatively good match obtained between the actual roll-call voting in the House . . . and in our sim-

ulation . . ." (p. 517, our emphasis). "The micro level performance or average percentage of representation correctly simulated by the 'just party' model is 78 as compared with the 84 percent obtained with the original model. *It is thus clear* . . . that party plays an important role in the performance of the simulation model, . . ." (p. 511, our emphasis). "The memory effect is thus *a slight aid* in the micro level performance . . . (p. 515, our emphasis). "In terms of the manner in which theories can be evaluated, this theoretical model has performed well on the basis of a prediction criterion." (p. 517, our emphasis).

At the risk of being accused of taking words and phrases out of context, we contend that the quoted statements give an illusion of confidence that highly acceptable tests of statistical significance simply do not bear out.

In order to check the effects of model formulations, we subjected Shapiro's data to analysis of variance to determine which formulations were more accurate predictors than others. The difference between "percent correct" of the model and the actual vote was used as the variable in the test of effects. Percentages were transformed by arcsin method.[1] The means thus found are expressed as geometric means in percentage terms. The particular technique of analysis of variance used in our test was that commonly known as "blocks design," a system developed to validate successive tests on a body of data.[2] One of the reasons for using this particular technique is Shapiro's own discussion of the correlation between effects on pages 508 (note 29) and 513.

The analysis of variance *demonstrates no significant* differences between model formulations —F-ratio is 1.45 with 6 and 120 degrees of freedom, $p > 0.5$—but does indicate a significant difference between the bills—F-ratio is 5.76 with 20 and 120 degrees of freedom, $p < .001$. The geometric mean for the various models is: Original model—84.1; Just Party—78.7; Without Party—80.0; Without Constituency—82.1; Without Region—82.7; Without Memory—83.6; and Without Communication—79.1. Geometric means of the individual bills range from a low of 58.8 to a high of 90.8 and a difference of means test among the means for the twenty-one bills shows significant differences (at $p = .05$) in almost one-half of the tests.

Our tests show that the model was significantly more accurate in its predictions on certain bills than on others. The data available to us do not permit any analysis of why this difference occurred but we suggest that Professor Shapiro examine into the question.

In summary, we are not challenging the Shapiro model but on the basis of the data presented in the REVIEW article we do not believe that he has proved his case. By quoting Pool, Abelson and Popkin on the problems of evaluating a complex simulation model (p. 510), Shapiro opens the question of validation to review. Our review suggests that validation has not been shown. Shapiro makes no exaggerated claims for his model, but his caveats do not, we think, totally excuse his failure to carry validation tests to a more acceptable level.

FRANK K. GIBSON
JAMES E. PRATHER
University of Georgia

TO THE EDITOR:

It is unfortunate that the concept of statistical significance, whose meaning relates quite unambiguously to problems of sampling error, has become an approbative term which many social scientists are eager to apply in varying and often inappropriate contexts. Gibson and Prather were unable to find a claim referring to "statistical proof" in my article (the "certain words and phrases" they cited were remarkably free of the language of statistical inference) because there was no appropriate way to consider the representatives, the roll-calls, or even the models tested in my study as samples. Thus the differences I found between my simulation model and each alternative formulation of it could not conceivably have been a result of sampling error. Each difference is a difference whose significance should be evaluated on the basis of its magnitude, not with respect to sampling distributions.

MICHAEL J. SHAPIRO
University of California, Berkeley

TO THE EDITOR:

Apparently the purpose of the exercise by Pulsipher and Weatherby "Malapportionment and Party Competition, and the Functional Distribution of Governmental Expenditures" (this REVIEW, December, 1968, pp. 1207–1219) was to show that ". . . apportionment patterns and levels of political competition are more potent influences than some of the recent literature . . . would lead one to believe" (p. 1218–19). Yet nowhere in the eight tables and assorted findings in the article does one find any evidence to support this conclusion.

Nowhere in the article do the authors assess the independent impact of their political vari-

[1] George W. Snedecor and William G. Cochran, *Statistical Methods* (Ames, Iowa, Iowa State University Press, 1967), p. 327. This transformation is known more familiarly as angular transformation.

[2] For a full discussion of this technique see *ibid.*, pp. 299–301.

ables on public spending, or compare the impact of political and socioeconomic variables. At no point do the authors specify the contributions of their malapportionment and party competition measures to the multiple R^2's for their expenditure measures. At no point do the authors compare the contributions of malapportionment and party competition with the contributions of their socio-economic variables to the multiple R^2's. All they do is discuss the significance of adding the variables in a multiple equation. A commonly acknowledged aspect of multiple regression equations is that one can produce a statistically significant result without making any real gain in predictive ability. (See, for example, William Hays, *Statistics for Psychologists*, Holt, Rinehart and Winston, 1963, p. 536.) The elementary question to raise is what gain in R^2 occurs through the addition of party competition and malapportionment taken separately. These two authors do not tell us, nor do they consider only these two variables taken together. For all we know the social variables rather than political variables could have produced the main increases in R^2? The findings reported in Tables 3 and 5 certainly give indications that this is so.

Moreover, there are a number of strictly technical aspects of their exercise that are difficult to comprehend. For example, are the numbers under the column heading "Constant" in Table 5 referring to the intercept value of Y when X is zero? If so, why are significance levels calculated for these values? Since the value of two intercorrelations determines the necessary lower limit for a third intercorrelation and this lower limit may be greater than zero, why determine whether all intercorrelations are zero? Finally, since a vector rather than individual variables were used, why calculate signifiance levels for each variable? It is relatively easy to obtain significance levels when *t* tests are run on a number of intercorrelations independently.

If the authors would compare the contribution of their malapportionment and party competition measures to the multiple R^2's, with the contributions of per capita income and their other social variables, by means of standardized *b* values or other commonly accepted techniques, they would very likely find that the contributions of these political variables are not as great as the contribution of socio-economic variables. Moreover, if they read with care the earlier works of Hofferbert, Dye, Dawson and Robinson, and others cited in their introduction, they would find that this is precisely what these earlier researchers said.

THOMAS R. DYE
JAMES W. DYSON
Florida State University

TO THE EDITOR:

Apparently the language of our "exercise" was not instructive enough to prevent some misunderstanding of its purpose—at least by Professors Dye and Dyson. The substantive section of their letter (the "nowhere-and-at-no-point" part) suggests a misconception which should be corrected.

Apparently Dye and Dyson interpreted our exercise as an attempt to ascertain whether economic and demographic differences between states were "more important" or "less important" than political differences with respect to "predicting" inter-state variations in expenditure patterns. "At no point" in our exercise do we imply that we were concerned with this question and frankly, we fail to understand why Dye and Dyson concluded that we were. Perhaps they were misled by the skeptical tone of the introduction to our exercise. This skepticism was directed, however, not at the importance of economic and demographic factors, but at what we feel is a *non sequitur* implicit in the *methodology* used by Dye, Hofferbert, and the others referred to in our introduction—namely, that if economic and demographic factors are "more important" than political factors, then political factors are unimportant. Perhaps our rhetoric would have been improved if we had been more explicit on this point, but we just don't find the burning of straw men a very exciting pastime.

As we clearly stated our objective was to "find those expenditure functions which are consistent with [our] hypotheses as we have interpreted them." It should be emphasized that if, as Dye and Dyson suggest, we were to calculate standardized beta values our testing procedure would produce exactly the same results as those in the original article. "Standardizing" regression coefficients changes neither their sign nor their significance level and these two characteristics were the criteria used to accept or reject the hypotheses with which we were concerned.

We are sorry that Dye and Dyson apparently experienced some difficulty in comprehending "a number of strictly technical aspects of [our] exercise." We think we can help them with both of those that they mention. Their first difficulty is comprehending why we included the significance level of the constant term of our regression equations in Table 6. The reason that this was done is that it was an output of the program we were using. It is a meaningful datum which can be used to make inferences. We did not make use of it in this exercise but we have in others. Its calculation and inclusion in the table has no effect upon our results. Apparently Dye and Dyson's second difficulty is the result of their misunderstanding of our use of the term "vec-

tor." We used this term in the way it is traditionally used in statistical analysis. The individual social and political variables are merely the components of the vector. We calculated the significance level of the vector and of each of its components because the testing procedure we used stipulated that both these values exceed a specified value.

ALLAN G. PULSIPHER
Southern Illinois University
JAMES L. WEATHERBY, JR.
University of Minnesota

TO THE EDITOR:

The practice of using a book review as a vehicle for the presentation of views on topics not necessarily concerned with the book being reviewed has been consecrated by immemorial practice. It is unfortunate perhaps in the light of the information explosion, but a fact of life nevertheless. The reviewer of my book *Governing the Commune of Veyrier: Politics in Swiss Local Government* (APSR, Vol. LXII, No. 4 [December, 1968], pp. 1374–1375) has gone too far, however.

According to the rules I cannot complain that he has used my small book as a vehicle for the presentation of highly debatable impressions of Swiss political life. Note for instance the shocking statement: ". . . Swiss politics are today dominated at every level by a concern with the maintenance of precarious religious, linguistic and political equilibria in the face of burgeoning Catholic birth rates and immigration by foreign workers . . ."

Under the rules of the game I probably do not even have the right to complain that despite the generous length of the review he missed completely the point of the book and the series of which it is a part. There was no intention to make my book a comparison of the Swiss local governments. It is, as amply pointed out in Professor Buechner's introduction, a descriptive case study of the administration (and politics) of a specific local government in Switzerland, nothing more. It is our feeling that at present there are too few case studies of local government. Only when we have collected enough such studies can a meaningful comparative study of local governments be made. We believe in starting at the beginning.

I do take strong objection, however, to the reviewer taking words out of context, that put the book in a completely false light. The keystone to the entire review is his statement that I chose the commune of Veyrier "as a 'political microcosm' of Switzerland (p. 23)." This statement is patently false. No such statement is made on page 23 or anywhere else in the text including Professor Buechner's Foreword. All that is said in page 23 is that: "In many respects the political party system in Veyrier is a microcosm of that found in the canton of Geneva and even of that in the nation as a whole." I then go on to spell out some of the similarities and the *differences* between the Veyrier political party system and the larger units. Using this misrepresentation as the key to his review, the reviewer then proceeds to make a number of criticisms. The criticisms are not necessarily criticisms of my book, however, and should be labeled as such in order not to give the reader of the review a completely false impression of what was attempted and what was accomplished.

GEORGE A. CODDING, JR.
University of Colorado

TO THE EDITOR:

It seems making altogether too much out of Professor Codding's modest little book to write a counter-critique of his critique of my review of his monograph. If he did not wish his book to be reviewed comparatively he perhaps should have chosen a less suggestive title than *Politics in Swiss Local Government,* he probably should have abstained from the controversial analogies hazarded from time to time in the text (the word 'microcosm,' whatever the context, remains provocative), and he certainly should have dissuaded John Buechner from introducing his book as "the first in a series of publications dealing with comparative public administration" in a Foreword which, while it does not contain Codding's phrase "descriptive case study," does employ the word comparative eleven times. His own rejoinder characterizes his approach as one of "spelling out some of the similarities and differences between the Veyrier political system and the larger units"; surely this is the very essence of comparative political analysis.

In any case, the point of my review was not to scold Professor Codding for failing to accomplish what he apparently did not intend to undertake, but simply to regret—in light of the undeserved negligence with which Switzerland has been treated by American political scientists —that the results of his careful research were so cautiously circumspect, and the scope and thrust of his generalizations so exceedingly limited.

BENJAMIN R. BARBER
University of Pennsylvania

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BOOK REVIEWS

Political Research and Political Theory. Ed. by Oliver Garceau. (Cambridge: Harvard University Press, 1968. Pp. 256. $7.95.)

The late V. O. Key in whose memory the essays in this volume were written was a "data man." One might expect, therefore, that his memory would be honored by writing and research in the empirical tradition that he did so much to advance, not by preaching but by practicing it. Yet, only two of the chapters, "Micro- and Macro-politics: Notes on Research Strategy," by Douglas Price, and "State Party Systems and Policy Outputs," by Duane Lockard, satisfy the criteria of an empirical politics that guided Key's own work. Incidentally, Price and Lockard are the youngest contributors to the volume. Two other essays, "The Concept of 'Party'," by Austin Ranney, and "The Comparison of Western Political Parties," by Leon D. Epstein, though more conceptual than empirical in content, are at least inspired by Key's work. And these two are, in age, the next youngest. I leave it to the reader to draw his own conclusions about Key's influence on contemporary political science.

Now, what bothers me about this volume is not what bothers most reviewers of symposia, namely, that they are more often than not grab-bags of unrelated, heterogeneous intellectual goodies. In fact, I like both grab-bags and goodies, and I enjoyed reading all the goodies in this particular grab-bag. What bothers me is that some of the distinguished authors here assembled know quite well what V. O. was all about and then proceed as if it made no difference. At least some admit their intellectual discomfort with uncanny candor. Avery Leiserson, for instance, informs us that "both in his professional work and personality, V. O. Key exhibited a 'briny irreverence' toward empiricist and rationalist pretensions alike, as well as to the moral agonizing of the value-oriented schools of political philosophy, that is a healthy antidote to premature integration in theory-building." (p. 24) But this understanding does not save Leiserson from the temptation of doing just what V. O. would not have done in a rather ethereal essay on "Empirical Approaches to Democratic Theory."

M. Brewster Smith, the only non-fraternity man in the volume, reports being "keenly aware of Key's rare talent for grasping the Elephant whole without recourse to such arid conceptual baggage. It was part of Key's artistry to keep much of his conceptual sophistication implicit, as he brought empirical analysis shrewdly to bear on theoretical and normative problems." (p. 79) But Smith's contribution, "Personality in Politics: A Conceptual Map, with Application to the Problem of Political Rationality," is a formidable excursion into the labyrinth of conceptual mishmash. What, let us wonder, would V. O. have thought about a paragraph like this?

> The remaining functional basis of attitudes, mediation of self-other relationships, is neutral in rationality from this processual perspective. The motivational agenda that it implies has nothing intrinsically to do with how the person appraises the bearing of a focal object or issue on his interests and values, but neither does it necessarily operate at cross-purposes with rational object appraisal, as is the case with externalization and ego defense. And we shall see that there are conditions under which it may lead to decisions which, as products, must be judged as rational. (p. 95)

Even Douglas Price seems uncomfortable. Referring to the second part of his piece that deals with the problems of relating individual, group and aggregate data, and reductionism, Price points out that "the latter two topics are doubtless rather more abstract and methodological than Key would be enthusiastic about." (p. 104) And Leon Epstein, in his gutsy article that suggests testing some of the hypotheses about American parties and elections in European nations, implies that V. O. might have taken a dim view of the enterprise: "Understandably, Key's criterion was simply what seemed relevant in the United States. His subject matter, after all, was virtually all American." (p. 168)

Equally bothersome to me is the editor's apparent assumption that the problem of the electorate's "rationality" was of enduring, pervasive and compassionate concern to Key, in commissioning two essays dealing with the question of rationality. *The Responsible Electorate,* though V. O.'s last work, was also his weakest work, and I doubt that he will be remembered for it rather than for *Southern Politics* or his influential textbook. That J. Roland Pennock, in "Political Philosophy and Political Science," and Frederick M. Watkins, in "Natural Law and the Problem of Value-Judgment," come out with opposite interpretations of Key's "philosophy" is not surprising. In opening his essay, Pennock asserts that Key's "posthumous book, *The Responsible Electorate,* underlines what all who were familiar with the man or his work well knew; that his concern for the scientific study of politics was paralleled by an equal concern

for a particular set of political values, those we associate with liberal democratic institutions." (p. 39) But Watkins writes: "Political theory was a branch of political science in which V. O. Key, to judge at least by his publications, was not much interested. This was no personal idiosyncracy, but was typical of the times. . . . No man with the talents and interests of a V. O. Key could have been expected to pay more than passing attention to problems of this sort." (pp. 58–9)

Finally, it seems to me unfortunate that some of the authors dig up some of Key's most dubious epithets to make their own intellectual stance more palatable. For instance, Merle Fainsod, in "The Dynamics of One-Party Systems," finds comfort in Key's statement that "the aspirations which find their focus in one-party rule are rooted in deep-seated human drives." (p. 222) Even Duane Lockard repeats the bromide that research on the social determinants of voting "threatens to take the politics out of the study of electoral behavior," (p. 211) only to be contradicted by Smith to the effect that "social determination is by no means incompatible with political rationality." (p. 100) V. O., of course, rarely permitted himself to become the prisoner of his own cliches. Over the long haul, I think, he will not be remembered for his *obiter dicta* but for his formulation of novel, imaginative and empirically tested hypotheses about political behavior and processes.

All this is not to say that this is not a worthwhile compendium. On finishing it I asked myself, "of what use to me is a book like this?" The virtue of this book consists in one's seeing one's colleagues grapple "in public," as it were, with very private scholarly concerns. This is especially true of the essays by Leiserson, Price and Ranney which I found most interesting precisely because they remain "open-ended." Pennock and Watkins reassured me that, their own efforts notwithstanding, the fact-value problem is still pretty much where it was when I went to school. Price struggles with the unresolved problem of units and levels of analysis, and Lockard quantifies with a healthy respect for nonquantifiables. Fainsod makes a heroic effort to compare five probably non-comparable "cases" (not types, as he says) of one-party rule, and Epstein sticks his neck out by not indulging in the verbal gyrations of the functionalists. In short, we have a long way to go. Political scientists are still riding off in many directions, evidently on the assumption that if you don't know where you are going, any road will take you there.

HEINZ EULAU

Stanford University

TANU Yajenga Nchi, Political Development in Rural Tanzania. By GORAN HYDÉN. (Lund, Sweden: Scandinavian University Books. 1968. Pp. 282.)

The title of this study reflects the fact that we are now faced with a "third wave" of political studies in Africa, and perhaps in other developing parts of the world, as well. It is fitting that concern with micropolitics in Tanzania should be heralded by a slogan, "TANU builds the country," in Swahili, the *lingua franca.* The author, a Swedish political scientist, also performs a much-needed service in the same vein, by supplying a Swahili summary at the end of his book. For it is among the villagers of the new states, the non-speakers of European languages, that investigators must seek better understanding of new political institutions and new directions in policy. The first "wave" of scholarship broke the paths and cleared the ground, so to speak. (Coleman, Apter, Morgenthau, Wallerstein and the three volumes edited by Carter), while the second posed several critical questions and began a process of re-assessment (Zolberg, Kilson, Sklar). Both "waves" proceeded largely at the elite, national, and even international level—macro-studies. The micro-studies are just beginning to emerge. Indeed Tanzania will be the first African state probably to be blanketed by studies of village politics after independence.

Mr. Hyden brings skills and ambition to his task, much to be admired. He knows Swahili, although he employed an interpreter in interviewing, he lived in Bukoba, the "metropolis" town for the five rural parishes he visited and investigated, he immersed himself in the local history, he developed and supervised an attitude survey in inauspicious surroundings, and he attempted to link his case study to extant hypotheses and approaches of contemporary students of political development. The result, however, is not woven together very well, although an overall design is apparent, derived from the Shils-Eisenstadt approach to political development in terms of center and periphery and from the analysis of developmental processes via a focus on system capabilities and political culture, worked out by Almond and Verba. (On page 22 Hyden offers us one of those marvelous diagrams which outlines everything going on in a political system.) In laying out the political history of Buhaya (the area of traditional habitation in question), in the report of the attitude survey, and in the sections testing and referring to more general hypotheses and analytical suggestions, one is reminded of a comment by A. J. P. Taylor on American Ph.D. theses: "so much raw meat, not enough cooking." Hyden writes clearly, but woodenly—of

course, English is not his native language. He seems to have made a deliberate choice not to give us "the feel" of Buhaya. Certainly, manifestations of Tanzanian politics do not come alive in this rural north-west corner of the country, the way they do in the writings on Tanzania of several of the authors cited in the extensive notes and bibliography.

But Mr. Hyden's book is noteworthy for its devotion to incremental gains in political science, for the challenges it throws up to a number of grand generalizations about the nature of political development, and for his completely open and honest effort to do what has to be done if we are ever to know much about the political behavior of exotic peoples.

Basically, Mr. Hyden shows that TANU, the monopolistic party-government of Tanzania, has been an agency of political development, as far as the input side of the emergent political system is concerned. Although generalizations must be restricted to the linkages between this corner of the country and a national Tanzanian political system, it appears that the national government is increasing its capacities to deal with internal challenges. Among the Haya people a strong feeling of identity with the nation exists, the regime is regarded as legitimate, and elements of a participant culture are now recognizable. The relationship between the Haya and the Tanzanian nation seems to be illuminated by analyzing their political behavior in terms of a subject culture, retaining elements of weak parochialism and developing forces of participation. While there is some interesting corroborative evidence here, a reader might suspect that the frame of reference shaped the conclusions. Mr. Hyden is modest and attractively uninsistent, but his desire to incorporate wholly the threads of systems capability analysis sometimes obstructs a convincing conclusion, at least for me.

There are, however, other valuable aspects of the study. Although my presentation may telescope a somewhat more complicated argument, Mr. Hyden offers more evidence for the notion that parochialism *per se* need not undercut "nation-building." A degree of cultural fragmentation before serious social differentiation can even facilitate national assimilation, if a national mobilizing force, such as the monopolistic party, enters the scene at the "premature" moment. In other words, one can argue that cultural secularization proceeding out of phase, so to speak, with the forces of commercialism and social restratification, can yield raw material for the nationalizing of sentiment, if the effective instrument is wielded by nationalizing leaders. At a lower level, here is also an explanation for the success of TANU. It entered the picture before economic and social differentiation was very serious, so it was able to capitalize on inevitable discontents and transfer emergent loyalties.

Concerning the whole question of the putative gap between tradition and modernity, Mr. Hyden provides material for arguing in a different vein. At least for the Haya, the thinking of the young and the old on a variety of matters of political significance is not so far apart; nor are there such great differences between the political reasoning of villagers and the national spokesmen on certain questions of policy. Villagers are resistant to the build-up of extractive strength of the national government and are making demands for more rewards—and more obvious ones—but, as Mr. Hyden deftly shows, for certain poorer farmers, it is "rational" to oppose improvement schemes and the like until there is further progress on the welfare front.

Among a few other suggestive points, one stands out. The mass media do not seem to be the principal progenitor of increasing expectations of life. The evidence here, coupled with the quoted evidence of a similarly-focused short study based in Uganda, seriously challenges earlier hypotheses about the "passing of traditional society."

The overall picture, nevertheless, remains one of descriptive and analytical austerity, and needlessly so, since the author has published at least two articles on political behavior in Buhaya, which enrich our understanding of matters such as, the relationship of families of chieftainship to present-day politics, and some social variables in leadership recruitment and support for TANU. In the present study, a number of possibly revealing inquiries are left dangling. Why, for instance, was there no relationship between Haya in faraway towns to the emergence of a Haya "tribal" identity before the rise of nationalism? Other groups, such as the Chagga, displayed significant inter-connections between emigrant and local modernizers. Does it have something to do with the "consummatory" value system attributed by Hyden to the Haya? But if that is the case, and this relates to the Apter thesis of consummatory values hindering modernization, why is there not more difficulty in absorbing the Haya into national political patterns?

My point here is not to try to invalidate Mr. Hyden's arguments, because I don't think anyone can fully grapple with them without enrichment that he probably could have provided by broader discussion of the issues raised by his own evidence and deductions. I believe, for example, that he overestimates the mass character of TANU in the middle 1950's, and that he underestimates some of the coercive nature of

"monopolitical" conditions in Tanzania today. Perhaps a broader-based discussion of politics in Buhaya at the time of the 1958 election and development of the problem of the political role of the Church, admittedly important, would have helped.

Whatever the omissions, this is a piece of basic and genuine research, and is much to be applauded.

HARVEY GLICKMAN
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Legislative Issues in the Fifty States: A Comparative Analysis. By WAYNE L. FRANCIS. (Chicago: Rand McNally & Company, 1967. Pp. 129. $4.95.)

One should not be deceived by the brevity of this volume. In it Professor Francis has compressed an intricate report of research, substantial theoretical insight, and considerable methodological reflection. In its breadth of focus the study represents a notable departure from most legislative analysis and must occupy a central place in the growing body of genuinely comparative studies of American state politics.

In addition to its comparative focus, the study departs from most analyses of legislative behavior by training the lens on the policy issues of state legislative activity. Studies of roll-call behavior and most interview-based inquiries have usually concentrated on the *processes* of state legislatures rather than upon their *policies*. Professor Francis does not ignore processes, but rather he views them in terms of their consequences for specific areas of public policy.

The data for the study were obtained from questionnaires mailed to a 1,600 member sample of the more than 7,000 state legislators serving in 1963. Useable replies were received from 838 legislators. To the extent that the sample could be validated (in terms of partisan composition and balance between senates and houses) it conformed quite nicely to the actual composition of the universe of legislators. The author is sensitive to the limitations of his data base without being unduly inhibited by them. The extent to which readers may entertain doubts about the reliability of the information for descriptive purposes should not detract substantially from the contributions of the study to theory construction.

The respondents were asked to characterize "important issues" (of their own selection) in terms of several dimensions: *interest* on the part of significant actors (the governor, news media, pressure groups, etc.); types of *conflict* (partisan, factional, pressure group, regional); the *resources* involved (money, level of government, length of public commitment); the expected *impact* (on numbers of people, prior policies, and future commitments); and the fate of the issue (passed, amended, defeated). In effect, the book unfolds as an explication of the matrix of interrelationships formed by the legislators' perceptions of these dimensions of issue characteristics. If for no other reason, the volume would be worth careful examination for the interesting and imaginatively constructed indices used to measure these characteristics.

Another useful aspect of the book is the manner in which Professor Francis is able to differentiate between policy areas in terms of the characteristics or, if one likes, the *structures* of policy. Thus certain types of policy are marked by partisan conflict, e.g., election laws, labor issues, and land policy. Other types of policy are found more frequently to display intra-party factional conflict, e.g., liquor laws, constitutional revision, and agricultural policies. Although the findings and analysis suggest several interesting alterations in current knowledge of the various dimensions of legislative behavior, they are sufficiently consistent with the theory presented to bolster confidence in the reliability of the data and the soundness of the interpretation.

From one standpoint, the major qualification of the study is a matter of research design rather than of data. It has the shortcoming of nearly all legislative studies in that it tells us little of the relevance of the legislature, as such, vis-à-vis other policy-making instrumentalities. In an effort to identify the points of genesis of major issues, Professor Francis asked his respondents, "In your legislature, where would you say the most significant decisions are made?" The alternatives provided in the questionnaire included party caucus, floor, substantive committee, pre-legislative session, policy committee, governor's office, and "other." If one's concern is with the genesis of policy rather than with the infra-structure of legislatures, this mode of inquiry may unduly inflate the relative role of the legislature. First of all, the fact that legislators are the subjects being queried biases the responses in favor of legislatively related arenas of policy initiation. Secondly, the alternatives do not explicitly provide for assigning initiatory status to any of several extra-legislative instrumentalities such as the bureaucracy, interest groups, party mechanism, or the federal government.

The final chapter, "Some Tentative Theorizing on Legislative Policy-Making," moves the analysis into an integrated context. In the framework of coalitional theory, Professor Francis examines the determinants of partisan conflict, factional conflict, and policy success. In the course of this explication, he is able to offer

some suggestive refinements of current comparative state studies as they concern the relevance of political system variables to policy content. His analysis suggests that we may find certain short-term (but all the same consequential) effects of such system characteristics as partisanship and competitiveness on the content and fate of public policy. If his suggestions are followed out in further research, there may yet be modest solace for those who are discomfited by studies such as Thomas R. Dye's *Politics, Economics, and the Public* which discount the relevance of political variables compared to socio-economic factors in policy determination.

Throughout this volume, Professor Francis assumes a high level of technical and methodological sophistication on the part of his readers. Those willing to stay with it will discover an on-going movement between theory, propositions, and data which provides a rich and challenging context for each subsection. One is continually prodded to think at least two or three steps beyond the data at hand. This process has the advantage of providing the reader with a sustained sense of the import and empirical relevance of that with which he is confronted. The price is paid in occasional inundation by hypotheses and sub-hypotheses. Comprehension, however, is facilitated by useful summaries at the end of each chapter. The book does not make for easy reading. It is devoid of rhetorical frills. Six words are never used where three will do. But despite its astringency, this volume must be considered both a significant contribution to our understanding of legislative policy-making and a stimulating guide to further inquiry.

RICHARD I. HOFFERBERT
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Comparative Politics: A Developmental Approach. By GABRIEL A. ALMOND AND G. BINGHAM POWELL, JR. (Boston: Little, Brown and Company, 1966. Pp. 348.)

This book is an elaboration of the structure-function paradigm first applied in 1960 by Gabriel Almond and James S. Coleman to the study of non-Western politics. In this latest formulation, Almond and G. Bingham Powell, Jr., postulate three levels of functions performed by all political systems: 1) system capabilities, 2) conversion functions, and 3) system maintenance and adaptive functions. System capabilities are the set of all relationships of a political system with its environment; they are the "outputs" and include extraction, regulation, distribution, symbolic output, and responsive capability.

Conversion functions operate prior to system capabilities, converting inputs into outputs, demands and supports into capabilities. They include articulation of interests and demands, aggregation of interests into policy alternatives, conversion of policy alternatives into authoritative rules, application of rules, adjudication of rules, and political communications.

System maintenance and adaptive functions include political socialization and recruitment, the mechanisms by which the political system adjusts itself to varying inputs and demands from the surrounding environment.

These thirteen functional concepts are joined by another key concept: political culture. While political culture is not a function as such and not logically linked with the thirteen functions, its inclusion by the authors serves to remind the reader that attitudes and desires of individuals, either as single actors or as "the public," are still legitimate objects of study even for the student of system-level phenomena.

Equally as interesting as the postulation of thirteen political system functions is the authors' discussion of political development. Political development, Almond and Powell state, characterizes a political system undergoing structural differentiation, cultural secularization, and increased sub-system autonomy as it meets the problems of state-building, nation-building, participation, and distribution. By hypothesizing that structural differentiation and cultural secularization tend to covary, the authors are able to create a two-dimensional space with these two properties describing the vertical axis and sub-system autonomy describing the horizontal axis. On this space are arrayed a wide variety of political systems from primitive bands through bureaucratic empires to high autonomy democratic systems. In the accompanying text one finds a more detailed description of sixteen types of political systems grouped as primitive, traditional, and modern.

A useful feature of this scheme is that it accommodates totalitarian systems in the developmental context: while both democratic and totalitarian systems are high in structural differentiation and cultural secularization, the totalitarian system is distinguished from the democratic by its low level of sub-system autonomy.

Having attempted in 1967 to apply the Almond-Powell scheme to Hong Kong politics, this reviewer concludes that it is adequate as an inventory of significant descriptive topics but inadequate as a rigorous analytical framework. The scheme seems vulnerable to the following criticisms.

First, the thirteen functions and the two key concepts of political culture and development are not systematically related to one another.

The book falls easily into chapters on the functions and concepts, each of which can stand by itself, but the "Overview" fails to provide the logical cement to hold together the functions and concepts in an articulated, comprehensive theory.

Second, the thirteen functions are inadequately related to the notion of system survival. The authors do not satisfy the reader that each function is equally vital or that the number of functions could not be increased or decreased. Further, the authors avoid offering a definition of system survival, thus leaving the reader unable to varify the authors' assertion that the given thirteen functions—and no others—are necessary for system survival.

Third, given Almond and Powell's guidance the separation of a given function from the structure which performs it proves to be difficult in practice. This is not surprising in view of the fact that Almond, in arriving at his original list of functions, looked first at working institutions in Western nations and asked himself, "What functions do these structures perform?" With this starting point it is hard to abstract and discuss a function without reference to structures which perform it, for the structures are the concrete indicators by which the abstracted function is detected in the real world.

Fourth, the authors fail to provide accurate, unambiguous, and parsimonious specifications of the dimensions of each postulated function and concept. For example, they describe political culture in terms of types of orientations, intensity of orientations, directions of orientations, frequency of orientations, objects of orientations, congruence of orientations with structures, degree of secularization, and so on. Do these dimensions, already so numerous as to be cumbersome, exhaust the possibilities?

Fifth and finally, the authors supply too few demonstrations of how their concepts could be operationalized for use in empirical research. This reviewer relied on his intuition in operationalizing such concepts as degree of cultural secularization and mood of political communications, receiving little guidance from the creators of the scheme.

To be fair, one must see the above criticisms in light of Almond's 1965 caveat that his scheme is

> more than a classification of variables and less than a theory of political systems. . . . [It is] a proposed first step towards constructing a theory of the political system and of the development of political systems.

The Almond-Powell scheme is useful as a statement of ideals even though it falls short in application. It reminds researchers of the necessity to acknowledge the interrelatedness of political phenomena and to see particular phenomena in the context of the larger environment. And it draws attention to the non-Western and developmental political processes. For these reasons it deserves consideration . . . and invites improvement.

STEVE HOADLEY
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Expansion and Coexistence: The History of Soviet Foreign Policy 1917–67. By ADAM B. ULAM (New York: Frederick A. Praeger, Inc., 1968. Pp. 775. $12.95.)

Since 1930 when Louis Fischer provided us with a full scale history of Soviet foreign policy no one has been bold enough to write an up-to-date treatment of the subject. And no wonder! Fifty years packed with events in many of which the Soviet Union played an important role; the absence of all but the meagerest Soviet documentation and an imperfect understanding of how Soviet internal policies relate to foreign policy—all seemed like insuperable obstacles. Mr. Ulam is to be admired for his courage and to be congratulated for the success with which he has carried off the undertaking. *Expansion and Coexistence* is comprehensive, based on a clearly articulated theoretical structure, and, what has become so rare, interesting to read.

Although the book is comprehensive it is not exhaustive. If Mr. Ulam had imposed the condition upon himself of reading everything relevant, he could not have completed the task and we should all be the poorer for it. On some matters others will inevitably disagree with Mr. Ulam's interpretations, but the basic structure of the book is sturdy and will survive the revisions to be produced by further research or new revelations. The book is solidly based on a theory about the relationship between ideology and national interest and the interrelationship between foreign and domestic policies. Mr. Ulam avoids the familiar but sterile opposition of national interest to ideology. Instead at appropriate points he describes the amalgam of belief, experience, acuteness and misperception which underlay Soviet calculations. For example, he describes quite convincingly how the destruction of the political system of Europe in World War I convinced the Bolsheviks that they could afford to be much more optimistic about the demise of capitalism than Marx had been. It was not necessary to wait for a proletariat superior in strength to achieve power. War could so shake the old society that a weak proletariat could take power as indeed they had in Russia. On another level, Mr. Ulam argues that the

tactics dictated by Moscow to the German Communist Party before Hitler's accession to power derived from the Bolshevik's own experience when out of power. Since the Bolsheviks had reserved their hardest blows for the other parties on the left and had later come to power, the German communists should, they believed, obviously emulate that example. That men tend to universalize the applicability of their own successes is a familiar idea, and hardly peculiar to socialist societies but eminently serviceable nonetheless. Herein lies the power of Mr. Ulam's analysis. His Bolshevik sees the world through the lens of communist theory about the world (whose permutations over time are taken into account) and acts out of habit, hope and fear. Mr. Ulam's Soviet decision maker is very human and very convincing.

Mr. Ulam is also very good in dealing with the relationship between internal and external policy. At times, he argues, the threat from abroad is artificially inflated to justify a harsh policy at home. Mr. Ulam argues interestingly, and uniquely, if memory serves, that in 1945 and 1946, Stalin was quite relaxed about the danger from the West (*vide* radical demobilization) but was concerned about his ability to maintain control within the country without a reimposition of severe controls. Hence an artificial war psychology. The analogue is the artificial war scare of 1927 which was Stalin's preparation for the campaign of collectivization and industrialization. (p. 165) (Yet on p. 439 we read that the "Soviet leaders were on the point of convincing themselves that . . . Baldwin and Chamberlin were about to launch a new intervention in Russia." This is the only such slip I noted.)

It is precisely because Mr. Ulam has the advantage of the long view that he knows what is missing in periods where the documentation is poor. This makes it possible for him to avoid the crude errors that arise from focusing on a single period, and permits disciplined speculation at other times. Thus he rejects the question of who was "responsible" for the cold war. Given the state systems that then dominated the world, (pp. 398–399) it seems inescapable to Mr. Ulam that these systems came into conflict. What interests him is the particular nature of this conflict. His main thesis is that in the early period of the cold war Stalin was so conscious of his own weakness, that he presented a bristling front to his opponents which had the virtue of keeping them off balance but the defect of causing them to consolidate their forces. Mr. Ulam suggests that at particular points Stalin tried to signal an intention to parley but he was so afraid of revealing weakness and, thus, so indirect that the opponent missed the cue. On the other side, in one such instance in 1950 Acheson asked "for welcome and tangible proof that Soviet intentions have changed." "Thus," remarks Mr. Ulam, "near the end of his days, Stalin was called upon to perform a rite he had so often inflicted on others: confession and recantation." Mr. Ulam does not try to make the vulgar point that the cold war could have been liquidated if one or the other party had been psychologically more astute and forthcoming. His point is rather that differing perceptions and internal necessities created the modalities of the cold war which in turn was determined by a basic conflict of interests.

On only one major point does the reviewer differ with Mr. Ulam, namely, in his assumptions about the balance of military force between the Soviet Union and the United States. In my opinion, Mr. Ulam places the Soviet achievement of rough nuclear parity with the United States far too early. For example (p. 498) he firmly states that the Western position vis-à-vis Russia would never be as strong as it had been in 1950. As a matter of fact in 1950 it would have been at least several months before the United States could have bombed the Soviet Union very extensively with nuclear weapons. As subsequent congressional hearings revealed, the United States had very few nuclear weapons before the Korean War and very indifferent delivery vehicles. By the end of the Korean War part of the thirty-six billion dollar annual increase in the budget had gone into a nuclear arsenal *and* improved aircraft. Mr. Dulles's statement of the American capacity in January 1954 to bomb the Soviet Union massively and instantaneously was no less than the precise truth. Since the Soviet Union was not in a position to retaliate in kind until at least six or seven years later, the Soviet military theorists were quite naturally preoccupied with subject of surprise attack and active air defense.

But Mr. Ulam is correct when he writes that in the immediate post war years the United States exaggerated Soviet military strength and therefore her proclivity to make war. However, he anticipates the improvement in the relative position of the Soviet Union and thus distorts his analysis of the Cuban missile crisis. Mr. Ulam's reconstruction, which he describes with his customary discrimination about the basis of his judgments as "entirely conjectural" (p. 644) will probably cause more controversy than anything else in the book. Mr. Ulam's thesis briefly is that Khrushchev wanted a favorable resolution of the Berlin crisis and a guarantee against the West German acquisition of nuclear weapons. By offering to remove the missiles which had been installed in Cuba, he could gain these

two objectives and proceed to a third on the basis of this victory; to persuade or force China to renounce nuclear weapons and make a general settlement with the United States. As usual Mr. Ulam has seized the right end of the stick. He demonstrates the utility of the concept of the triadic Soviet-Chinese-American relationship which is surely one of the keys to Soviet foreign policy in the sixties. Perhaps Mr. Khrushchev was sanguine enough to wave aside historical experience and to believe that a mere crisis rather than a war could produce such a global package deal. But Khrushchev knew, as Mr. Ulam does not, how poor the relative Soviet military position really was. For him the improvement of the military balance was a *sufficient* reason for the installation of intermediate range missiles in Cuba. Despite the missile gap talk of the election campaign of 1960, in 1962 the Soviet Union was still far behind the United States in intercontinental missiles. Of course, Khrushchev hoped and Mr. Kennedy feared that political changes would follow the shift in the strategic balance but the heavy pressure for the Soviet Union to get a short cut to "parity" was enough reason for the Cuban gamble.

Although every reader will have particular points of difference with Mr. Ulam, the whole can only excite admiration and, it is to be hoped, emulation.

HERBERT S. DINERSTEIN
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110 Livingston Street: Politics and Bureaucracy in the New York City Schools. By DAVID ROGERS. (New York: Random House, 1968. Pp. 584. $8.95.)

Contemporary empirical research offers a choice between two general strategies. On the one hand, the investigator may test hypotheses, focus sharply, collect specified data, and concentrate on explanation, showing relationships among only a few aspects of several situations or systems. On the other hand, he may explore thoroughly, focus more diffusely, collect as much information as possible, and concentrate on description, illuminating many aspects of a single situation or system. David Rogers has chosen the latter strategy to examine politics and bureaucracy in the New York City school system and has demonstrated that the case study, however unfashionable and whatever its limitations, is still an extremely productive means of analysis. Given the nature of his objectives, the complexity of educational politics, and the inscrutability of the target, Rogers adopts an eclectic approach. Instead of pursuing one theoretical path, he concentrates on several sets of factors that shape the politics and administration of school desegration in the city and treats each of them from a different perspective. After years of personal observation and more than one thousand interviews, he analyzes a vast amount of information with great care and skill. The result is the most exhaustive and thoughtful study of school politics to date.

One of the author's principal concerns is describing the strategies, tactics, and effects of interest groups that pressure the school system. Examination of the activities of these groups reveals much about pluralism, factionalism, and stasis in New York. With regard to school desegregation, status-quo forces were advantaged. The neighborhood school movement had only to mobilize local support on a few occasions to exert a lasting impact on the anticipations and behavior of educational leaders. Civil rights organizations, by contrast, had to sustain grass roots support and press continuously. This was exceedingly difficult, because of a lack of unified leadership, overly complex goals, and a conflict strategy lacking appeal to a wider public. White liberal groups were ineffective. Their leaders, practicing traditional logic, were unable to conceive practical solutions and their members, threatened by Negro demands, were less than enthusiastic. The balance of group power was held by moderate organizations. But they were unwilling to risk sacrificing quality education for equal education, and consequently did little. This description of competition among, and within, groups in a pluralistic and politically fragmented city lends additional support to an important proposition concerning organizations. The more highly differentiated, and the more unstable, the social setting, the less salient it will be, and the less salient the setting, the more salient the organization itself.

If this is the case, then those within an organization retain significant discretion in the formulation and administration of policy. Describing the exercise of such discretion is the second principal concern of Rogers. There is a tendency among political scientists to concentrate on the adoption of policy, to the neglect of its execution. Yet, as Murray Edelman suggested several years ago, there are differences between the symbolic, nontangible values allocated by policy makers and the material, tangible values allocated by administrators. Rogers, in what I consider the most important part of his study, treats these differences in considerable detail. Despite advanced policy statements promulgated by the school board, implementation was customarily delayed or vetoed by professionals inside the system. Virtually every program adopted at the higher levels, the author frequently points out, failed as lower levels exer-

cised their negative powers. Such subversion could occur repeatedly, mainly because of the rigidity and defensiveness of a bureaucracy in a system of arbitrary and chaotic centralization, undercontrol and decentralization by default, vertical and horizontal fragmentation, parochial codes and compulsive rule-following, and an extreme diffusion of effective power.

It is difficult to fault Rogers' descriptions of the New York City school system. But his evaluations are necessarily more controversial. One reason is that the wealth of reported information lends itself to somewhat varying interpretation, depending largely on one's professional judgments and social values. He is critical of the board of education for operating in isolation, urging that it become more responsive and permit the public greater power to set policies. Nevertheless, he takes the board to task for studying plans and holding public hearings, on the grounds that such delays reveal the board's own indecision and stimulate cross pressures. At one point, he suggests the utility of private discussions between the board and reform groups, rather than ceremonial public hearings held to give the appearance of consultation. At another, he criticizes the private nature of meetings between the school superintendent and civil rights groups, paying little attention to how their negotiations led to reformulations of a desegregation plan. Although he acknowledges the liberal intentions of most board members and presents arguments in their defense, the thrust of his evaluation is that they did very little to overcome opposition and even engendered it. Yet, another investigator, studying the same school system at about the same time, could evaluate things quite differently. "One fact is clear," Bert E. Swanson writes, "school officials are willing to try various methods and workable plans for integration" (*The Struggle for Equality*, p. 7). Certainly, school officials had options, but I disagree as to whether the board had more freedom to innovate than it cared to exercise. The board went as far and as fast as it reasonably could. In view of demographic factors and private economic decisions, the city's pluralism and political fragmentation, the always latent and occasionally active conservatism of neighborhood groups, and the strenuous opposition of the school bureaucracy, the board was actually more venturesome than might have been expected.

Still, the board has been less than successful in its attempts to innovate. How much less? One difficulty with the case study approach is that there are no convenient standards for evaluative use. In the New York City school system change has not been accomplished rapidly or easily, but some progress has been made. In addition to those subverted policies on which Rogers dwells, a number of other changes have been adopted and tried—the beginnings of a program-performance budget, the promotion of increasing numbers of Negroes to supervisory positions, a revision of the structure and procedure of the board of examiners, human relations training for school staff, greater stress on early-childhood programs, and curriculum revision. Do these changes, among others, denote some degree of effort and, if so, how much? Subjectively, the relevant question is whether more or less has been attempted than our social values dictate. The author's conclusion is that efforts have been minimal compared to the level of social needs and the legitimacy of civil rights demands. Objectively, the relevant question is whether more or less has been attempted in one school system than in others. The reviewer's guess is that efforts in New York have been considerable compared to those in most other cities.

It would be surprising if a reviewer's judgments consistently coincided with an author's. My few reservations are not meant to challenge the contributions of this remarkable work. The book adds to our theoretical and empirical knowledge of politics and bureaucracy, and for this I am personally indebted. It contributes to discussion and understanding of a major social problem, and for that we should all be grateful.

ALAN ROSENTHAL

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The Interpretation of Agreements and World Public Order: Principles of Content and Procedure. BY MYRES S. MCDOUGAL, HAROLD D. LASSWELL, AND JAMES C. MILLER. (New Haven: Yale University Press, 1967. Pp. 410. $9.75.)

Anyone likely to read this review is probably familiar with the wider professional context of the book, such as Myres McDougal's other writings, the policy-oriented jurisprudence at Yale, and the fact that now, in the late 1960's, is the first time in the history of international law that governments are trying to lay down a world-wide set of rules for the making of treaties and for the handling of treaty-related problems. In 1966, the U.N. International Law Commission completed many years of work on the law of treaties and adopted a set of 75 Draft Articles (ILC Draft), submitting it to the U.N. General Assembly and thereby to all Member States. The ILC Draft impulsed a world-wide professional debate in governments, law schools, political science departments and relevant learned societies. The U.N. General Assembly convened a

Conference on the Law of Treaties to consider the ILC Draft. The first session of the Conference was held under U.N. auspices in Vienna in 1968. More than 100 States were represented by plenipotentiaries. The second and final session is scheduled there for 1969. The first-named author of the book, Myres McDougal, was a member of the United States Delegation to the 1968 Conference. He had previously served on the special study group on the law of treaties appointed by the American Society of International Law to feed academic opinion into the process by which the State Department formulated American policy toward the ILC Draft. And aside from its perfect timing in this whole context, the book also happens to be the first major monograph on treaty interpretation published in English since World War II. So, obviously, the setting could hardly be more appropriate.

For all these reasons, the book is a must for the specialist in international law, and it will stimulate professional thinking in several adjacent fields. It is an ambitious book, as the title correctly suggests. It aims far beyond the immediate prompting of the ILC Draft. It starts from scratch in asking what men mean when they say or think they "agree" on something. The search goes into fields as far apart from traditional legal research as neuro-psysiology, linguistics, semantics, non-verbal communication, game theory, symbolic logic and others. The book designs its own system of inquiry and defines its own terminology. It also covers relevant legal sources from the classic authors of the 17th and 18th centuries to the latest court decisions. Conclusions and policy recommendations tie in nicely with McDougal's general and well-known concept of "minimum world public order." In this context, minimum world public order calls for a system of interpretation that aims at finding what the book calls the "genuine shared expectations" of the parties to a given agreement. How should the interpreter find the genuine shared expectations? According to the book, no single source should be taken as exclusively authoritative, not even the treaty text itself. The text is important, certainly, but so are the preparatory work, ancillary negotiations, subsequent practice, and the whole legal-political ambience before, during and after the merging of two or more national policies in one legal instrument.

Certainly, it would be a better world if governments and courts were to interpret treaties by the rules which the book suggests. Cheating would be harder. Ambiguities would be rarer. International transactions would be more predictable. Global purposes would enter and pervade the system. But how do we get from here to there? Which is the cart and which is the horse? Is there not a threshold yet to cross *before* interpretation can do these things?

Most of us who are writing about international law face the threshold problem in one form or another. We want to look toward a better future and even move things in this direction, but we also want to remain relevant to the ugly present. Each of us mixes the dosages as best he can at any one time for any one topic. This book, in my view, assumes too much when it offers technical advice to States in how to get at the "genuine shared expectations" or the "real meaning" of treaties. Such advice necessarily assumes that States want the real meaning to appear. But what if they don't? What, in fact, if this is what States try to avoid just because they know there is political power in interpretation, and because this power increases with the obscurity of the meaning?

This should not belittle the importance of interpretation in general or the book in particular. Quite to the contrary. It would not belittle Chief Justice Marshall to remember that the United States Constitution came first and his interpretation came second. He, in turn, reminded his audience that it was a "constitution that we are expounding." But when we are thinking about world public order in the mid-20th century we should remember that it is *not* a constitution that we are expounding. Unlike the U.S. Constitution, world public order is an academic abstraction. As such it is useful. It helps us think. It is elegant. It satisfies a need for professional shorthand. We don't have to go through cumbersome qualifications in writing about the better world toward which international law points. But in all this we must beware of reification. Terms like "world public order" and "constitutive processes" may, through repetition, acquire a misleading semblance of reality. Of course, this was not the authors' intention. They themselves are aware of this danger, and say so (page 395):

> "We are under no illusion, of course, that establishment of appropriate goals for interpretation . . . will necessarily bring about the realization of a world public order of human dignity."

And yet the book as a whole leaves a much more optimistic impression. It is almost as if the authors were only paying occasional tribute to this aspect of reality, such as in the above quotation, but were otherwise satisfied with some assumptions about State behavior, especially the one that States really want to maximize predictability of treaty performance and hence stability of the international system.

The harsh fact of the matter, as I see it, is that predictability is only one side of the seesaw. The other side may be called flexibility. States want predictability for others, and flexibility for themselves. In my view, this have-and-eat-cake spirit pervades international law. Therein lies its central paradox, and its timeless fascination. It manifests itself wherever we probe deeply enough into the logic of international legal rules, for instance here in treaty interpretation. State practice shows this ambivalence. Predictability and flexibility are both desired, and yet mutually exclusive. As the one goes up, the other must go down. Therefore, there is no technical solution. The real question is political: how much predictability *at the expense* of how much flexibility? States do not ask how to maximize one, but how to optimize the mixture of both.

We are back at the cart-and-horse problem. States must first be willing to move toward greater predictability, and thereby to change the international system itself. Then (but only then) will the time have come for the experts to suggest instruments to move the State system from here to there, and thereby to change it. Many instruments are possible, and some may be better than others, but unless States are *willing to use* the instruments, it makes little difference how good any given instrument may be.

The book fails to face this basic problem squarely. Instead it tries to solve the problem by assuming it out of existence. The last paragraph of the book begins with this sentence:

> "Proper principles of interpretation, therefore, are indispensable instrumentalities of change in the direction of a more perfect public order."

This eventual reification of what started out as nothing but an assumption is, in my view, the major shortcoming of this otherwise excellent book.

Whether or not one agrees with the central thesis of the book, there is no question that it constitutes a major contribution to the current world-wide discussion on the law of treaties. There is virtually unlimited food for thought (and stimulation for further research) in the very sweep of its design and in the radical questions it raises about the act of agreeing and about the fact of agreement. Without any doubt, our profession has been enriched and our thinking has been invigorated by this book.

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Little Groups of Neighbors: The Selective Service System. By JAMES W. DAVIS, JR. AND KENNETH M. DOLBEARE. (Chicago: Markham Publishing Co., 1968. Pp. 276.)

Selective Service: Conflict and Compromise. By HARRY A. MARMION. (New York: John Wiley & Sons, Inc., 1968. Pp. 168.)

Despite a strong similarity in concluding recommendations, and presentation of some of the same government data, these two books represent a thoroughgoing contrast in approaching Selective Service policy.

The ultimate concern of Davis and Dolbeare is to display the potential of policy analysis for both value choices and empirical theory; their orientation is self-consciously professional. Selective Service thus becomes the vehicle for a variety of theoretical expeditions on several levels of analysis; and the vehicle is fueled by an equal variety of data-gathering techniques. Marmion, an executive of the American Council on Education, follows the issue-oriented approach to political choice: "My intention is to convey the excitement surrounding the draft debate [of 1967] as well as some of the frustration that results when one realizes that well-thought-out reforms are not being accepted." (p. x) Marmion's data are derived from previous writings on the subject and observation of Congressional hearings and debates. From almost any point of view, the contributions of the Davis and Dolbeare study are far greater.

Little Groups of Neighbors, at the simpler levels of analysis, deals with the questions of organization, variability in impact and individual attitudes raised publicly in regard to draft policy. In the process, the study effectively challenges both the system's characterization of itself as "little groups of neighbors" and its assertion that draft support rests on widespread support for decision-making by local boards. The data are tied together to form a different picture, in which deferment policy and organizational characteristics mesh to provide lowered military liability for those in higher socioeconomic strata "while conferring the management of deferments and inductions upon community influentials drawn from the same strata. . . . Small wonder that they then are willing to tolerate if not affirmatively support . . . [the system's] continuation." (p. 4)

The accumulated data then serve as a base from which Davis and Dolbeare embark on three separate quests for practical and theoretical significance. The first of these is an attempt to make draft policy recommendations in a professional capacity. The procedure involves, first, estimating future consequences of alternative policies in light of findings about the present system, and second, choosing among projected consequences on the basis of the nominal values of current policy—efficiency, equity and public support.

Second, the authors examine the Selective Service experience for gains in understanding conflict and convergence between citizen participation goals and organizational goals. Although concluding that conscription is an area in which the two sets of goals cannot be attained simultaneously, they offer a speculative set of conditions which might promote such convergence in some other areas.

Finally, Selective Service is placed in the descriptive context of the Eastonian "feedback loop" in order to estimate extent of support, feedback and possible change within the system. From this descriptive transfer Davis and Dolbeare derive a few tentative hypotheses relevant to systems theory itself.

All in all, *Little Groups of Neighbors* is an ambitious undertaking marked by a reasonably high level of success. In relation to their policy recommendation aim, the authors have done well in narrowing the available alternatives, and for the most part have resisted the temptation to overrun their data limitations in projecting policy consequences. The point on which their projections are most hesitant—public support—is also the one most open to question. If, as seems to be the case in *The American Soldier* studies, perceptions of unfairness are correlated with perceptions of deprivation relative to the experience *of one's friends and associates,* rather than to those of the whole population, draft randomization might actually increase bitterness among draftees and their families. In short, even high draft liability, if shared with most of one's friends, might be more tolerable than a random fate.

Any discussion of public support must also take into account distributions at different levels of political activity, as the authors themselves well realize. The problem is that values may also vary by these levels; and view of the draft among national policy-makers may be determined largely by values related not to efficiency, equity or general support, but to the costs of change for possible tradeoffs on other policies. Davis and Dolbeare recognize this in brief examples; there is also implicit recognition in their suggestion that goal attainment and adaptation to change may not be as central a motivation as systems theorists have thought. But the recognition does not eliminate the difficulties involved in choosing only three values upon which to base policy decisions.

The chapters presenting the researchers' search for theory in relation to participation and feedback are laudable both in content and in willingness to venture beyond the last table of cross-tabulations. In fact, the book as a whole stands as a splendid example of the inductive steps to theoretical development. At the same time, it demonstrates the limits of this process; for the last chapters, though theoretically the most significant, are also very brief. Davis and Dolbeare sought to display the *potential* of policy analysis for theory, and have done so admirably. Full development, however, depends equally on deductive reasoning, and that is not provided in *Little Groups of Neighbors.*

Without wishing to seem unduly critical of a highly instructive work, I would add a methodological note. A certain amount of naivete in data handling appears in several places in the work; for example, no measures of association are used, even in the case in which a scattergram is presented. Such omissions constitute a minor annoyance, but in one case failure to calculate percentages for a controlled variable from the correct base changes the results. The table is used to show that approval of local boards is a function of education; properly calculated, it makes clear that approval is instead a function of knowledge about the boards, consistent across all educational levels. In view of the positive relationship between such knowledge and occupational status, it may well be that the effect of occupation would wash out also. Either by itself or in conjunction with a knowledge-occupation check, the discovery requires some re-evaluation of contentions regarding the draft and local elites.

Marmion's study of the Selective Service has much less to recommend it; indeed, it illustrates perfectly why policy studies fell into disfavor among political scientists. It is composed of a short history of the draft; a critique of the present system; a series of arguments against alternatives to conscription, and an account of the passage of the 1967 Selective Service Act.

The Marmion study is rather less persuasive than that of Davis and Dolbeare from the point of view of policy recommendations; and it has no theoretical perspective beyond advocacy. Potentially, it could have pointed up the alternative values of policy-makers mentioned as a problem in *Little Groups of Neighbors;* or it could have provided additional understanding of legislative processes. In fact, it does neither. Nominally a study of conflict and compromise, it tells us very little about these vital political concepts. Mr. Marmion has produced a highly readable book, and an honest (if somewhat overheated) appraisal of draft policy alternatives. But it is not a work which will be of great interest to most political scientists.

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Economic Policy-Making and Development in Brazil, 1947–1964. By NATHANIEL H. LEFF. (New York: John Wiley and Sons, 1968. Pp. 201. $7.95.)

Professor Leff has written a provocative, original and important study of Brazilian political and economic development. One of the central problems to which he addresses himself is: what do case studies of major economic policy decisions reveal about interest aggregation patterns?

Much of the literature on Brazilian development has argued that the President is unable to implement a coherent development policy because he is both pressured into yielding to a high level of demands and is checked by powerful veto groups. Leff argues, however, that the President has a substantial degree of freedom and that private-sector interest groups, whether domestic or foreign, did not in fact strongly influence the formulation of government economic policy. To support this thesis he offers a series of case studies of economic policy-making based on open-ended and anonymous interviews with former cabinet ministers, government experts (the técnicos), and representatives of interest groups that participated in the policy-making process.

In the case of the major export sector, the coffee planters, Leff argues that this interest group, traditionally assumed to have substantial political power, consistently failed to obtain their major policy goals in the period under examination. He demonstrates that they were unable to block the steady increase in the export taxation rate, which rose from 23% to 55% of the total value of coffee exports between 1954 and 1962. Similarly, demands by coffee interests that the protective tariffs for industrialists be kept low and that the dollar-cruzeiro exchange rate be undervalued were equally ineffective.

Leff similarly shows that despite the hypothesis that rapid industrialization has resulted in new political powers for the private-sector industrial interest groups, the public-sector's share of all Brazilian long-term investments actually doubled between the 1947–1949 period and 1958–1960. Even the Development Bank, originally set up as a major source of long-term financing for private industry, gave 84% of its total allocations to public-sector projects between 1952 and 1964.

From these and similar studies, Leff argues that not only has the power of interest groups been overestimated, but that the interest group approach to political analysis imparts a serious perceptual bias in a system where actors from the same "interest group" directly compete for preferential treatment and personal favors at the level of policy implementation, rather than aggregate their demands at the level of policy formulation.

Whereas most commentators have viewed this "clientelistic" style of politics as developmentally dysfunctional, Leff claims that the very weakness of interest groups has the functional effect of allowing the President and his expert advisors a "substantial degree of autonomy" to determine policy according to technical and national considerations. This reviewer would add, however, that the presence of strong interest group demands can also be a source of political *support* for governmental programs, and the absence of such demands a source of Presidential weakness rather than power. For example, largely because of their "clientelistic" nature, the peasant leagues and the trade unions were not able to effectively aggregate their demands and mobilize the support that President Goulart needed to make Congress accept his program of "Basic Reforms."

This reviewer also feels that the image of the "autonomy" of the President would have been further reduced if more consideration had been given to other sources of constraints on the President besides the relatively formal interest groups of the private-sector. For example, the limits imposed on the President by the decentralized public-sector agencies and large government corporations, owing to their high degree of revenue and budgeting independence, and their tradition of bureaucratic entrepreneurship and pressure group activities, needs more research. The fact that Populist Presidents felt that they had to avoid antagonizing mass opinion, not merely elite opinion or interest groups, was also an important factor in the rapid softening of initially firm stabilization policy-decisions to freeze wages or to eliminate subsidies of wheat and bus fares.

Given Leff's position that the Presidents in the period between 1955–1964 did not experience significant political obstacles to effective economic policy-making, what is his analysis of the constraints on the economy that reduced the very high growth rate of the 1950's to near stagnation by 1963–1964? This is another major theme of the book. While not disputing the fact that the growing political polarization and the constitutional crises contributed to inflation and to the 1964 coup, which installed the first military government in the twentieth century in Brazil, Leff argues that the basic cause of the economic slowdown was a constraint on imports due to export stagnation. Between 1948 and 1962, the volume of Brazilian exports increased by only 7%. Prices fell even more sharply, however, so that foreign exchange earnings actually

declined. Thus while the GNP rose by 61% between 1954 and 1963, the capacity to import did not increase at all. Since Brazil's rapid industrial growth rate was largely the consequence of an import substitution process that was more import intensive in terms of raw materials and equipment than was realized at the time, Brazil's low capacity to import finally checked further development.

A common theme of development politics is the desire of developing countries to escape dependent or colonial economic status by creating modern industrial and service sectors. Less well documented, however, is the often unconscious hostility that exists towards the agricultural sector and specifically to traditional exports, which are seen as symbolic of dependent status. This hostility towards developing traditional areas of the economy frequently leads to serious underinvestment in and underutilization of resources that could help finance the desired diversification of the economy. In Argentina, for example, Díaz-Alejandro has shown that only 1% of the net increase in capital formation between 1927 and 1955 took place in the agricultural sector, and that this contributed to the 26% decline in the total quantum of exports between the 1925–1929 period and that of 1957–1961.

One of the strengths of Leff's book is his interesting discussion of the psychological and ideological origins of the export stagnation in Brazil and the bottleneck in foreign exchange that resulted from it.

On the basis of his own extensive interviews, Leff concludes that the Brazilian economic administrators had a "strong bias against exportation, either as a source of aggregate growth or as a means of dealing with the country's balance-of-payment problems." Part of this bias stemmed from their "exportable surplus" theory of trade which viewed exports as desirable only if the domestic market was completely supplied. Disregarding comparative advantage trade considerations, the exchange rate for exports was overvalued and export licenses were often denied. Another subjective consideration involved was the fear that expansion of exports would increase dependency on world trade and perpetuate an agrarian economy in Brazil. Leff argues convincingly that given the relatively high elasticity of supply, Brazil could have satisfied domestic demand while simultaneously expanding exports and then using the exchange earnings to help finance industrialization.

Leff also has valuable discussions of the causes of the infiuence of "técnicos" in the system, the Brazilian process of opinion formation, and the informational requirements of effective policy-making. In all, Leff has probed some of the more difficult and central questions about the Brazilian political and economic system. Despite some differences of opinion, this reviewer feels that more than any other recent book on Brazil, Leff has opened up new areas for research and forced a re-evaluation of some basic hypotheses.

ALFRED STEPAN
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The Third Reich of Dreams. With an Essay by Bruno Bettleheim. By CHARLOTTE BERADT. (Chicago: University of Chicago Press, 1968. Pp. 177. $4.95.)

This book deals with a crucial aspect of political life that has been too often ignored in political science—the intra-psychic mechanisms whereby individuals collaborate in their own control by a totalitarian regime. Analyzing the dreams of over 300 Germans who lived in Nazi Germany, Charlotte Beradt (a journalist and collector of these dreams between 1933 and 1939) shows how many individuals unconsciously made the regime more systematic in its terrorization than it actually had been. The dreams show how, "in their blind fear of the hunter, such people would assume his role and hunt themselves—how they inadvertently helped set and spring the traps into which they were meant to fall." (p. 50)

Beradt organizes her presentation of these dreams around their central themes. There was, for example, a widespread apprehension that nothing could be hidden from the government Thus one dreamer heard a loudspeaker blaring out a decree on the "Abolition of Walls"; another was detected in a thought associating Hitler with the devil by an electronic machine; others dreamt of Dutch ovens, mirrors and the like which recorded their private conversations or innermost thought for the benefit of the police. The dreams—replete with items such as a "Monitoring Office," or a "Regulation Prohibiting Residual Bourgeois Tendencies Among Municipal Employees," and "nose-suspects"—also showed a general anticipation of the bureaucratization of terror and the fear that one might become a suspect. Most significant was the inability of the dreamer to rebel, even in his sleep. One dreamer, e.g., carried out a contemplated letter of protest by putting a blank sheet of paper in an envelope; another faulted in the telling of a forbidden joke; one man dreamt in triangles, rectangles, and octagons, telling himself that it was forbidden to dream. Other dreamers were admired or sought after by Hitler, Goering, or Goebbels. Even among the

Jews—most of whom tried to cross borders or sail ships in their dreams—there were a few who tried to join in with the regime: e.g., one man dreamt that Hitler intervened on his behalf against the slurs of a lower official, saying, "Our Jews are not like that." Only one person in this entire group ever dared dream of an attempt on Hitler's life, and he was outside Germany at the time of the dream.

In seeing these dreams as specific adaptations to twentieth century totalitarianism and the Nazi regime in particular, Beradt needlessly limits their usefulness. As Bruno Bettleheim points out in an excellent essay accompanying this edition of the book (which was first published in Germany in 1966), the totalitarian regime does not mold a new human nature—rather it plays upon and exploits common psychic weaknesses. Arrogating to itself the paternal position, the dictatorial regime drives the subject back, unconsciously, to a state which all have known—the helplessness of infancy. Governmental authority then takes on for the subject individual the omnipotence, the omniscience of the original authority; and insofar as this authority thereby becomes inescapable, the boundaries between self and other are erased and the identity threatened. The result is a splintering of the ego.

In treating the regressive possibilities apparent in these dreams as a potential for all human beings under certain circumstances, Bettleheim gives Beradt's work a utility for the whole of political science that it would not otherwise have. Viewed in this light, the material gives support, e.g., to Lasswell's argument (in A. H. Stanton and S. E. Perry, *Personality and Political Crisis,* 1951) that governmental lies and other acts that undermine faith in the public authority and information sources may not result in a growing criticism of the government; rather, it is apt to lead to a widespread atrophy of the ego among the populace and the development of a public life which is characterized by the cue and the ratlike response, a clinging to the threatening but all-powerful authority.

If one were to employ the ordinary criteria for assessing the techniques of inquiry and validation used in this work, the author could be criticized on several counts. The choice of individuals for study was not systematic—Beradt (with the assistance of some friends, including one physician) simply interviewed those with whom she came into routine contact. Background characteristics were in no way controlled: she talked to her dressmaker, her aunt, the milkman, as they were available to her. Her sample was not representative of the German population: "yes men" and individuals who directly benefited from the regime were not interviewed. Nor was the authenticity of the dreams guaranteed through tapes or signed statements by their originators. (The dreams were recorded by Beradt, using code words for key items such as "Hitler" and "Party," then hidden in book bindings, and ultimately mailed out as letters to her friends throughout the world.)

Yet neither the subject matter nor the times would have permitted Beradt the usual research techniques. She had the courage and the intelligence to inquire into an extreme political situation in which she had been caught and her material seems to have been collected with as much imagination and care as was possible under the circumstances. To hold her to the usual standards of research would be to unduly restrict investigation in political science. It is in just these most difficult cases (methodologically speaking) of social crisis or political aberration that some of the most interesting political material becomes available. In varying the former "givens" of a political system, these circumstances make possible the investigation of the parameters of ordinary political life.

Two methodological criticisms can nevertheless be made. A more systematic presentation of the data she does have would make it possible for the reader to assess her interpretations of her data. Furthermore, a review of current research on dream interpretation and content (e.g., the works of E. Erickson, D. R. Hawkins, M. Ullman, K. Altshuler, P. Giovacchini) would have been useful. As it is, her decision to deal exclusively with the manifest content of dreams, and to treat them as relatively clear statements of current conflicts with the regime, while at the same time prophetic, raises some serious questions. As Bettleheim points out, the dreamer's insight into the inner nature of the totalitarian regime can only be explained in terms of certain tendencies present in him, tendencies due to unresolved conflicts arising out of his own experiences in infancy. Beradt's dreamers were torn, because of their own inner motivations, between affirming and denying Hitler's regime.

Despite these problems, this work provides some very significant data for the political scientist. When compared with other available studies on the dreams of resistance fighters (a few of which are given here) and those imprisoned in concentration camps (e.g., as reported by L. Bandura in 1948), further insight may be developed into those very central and too often ignored unconscious processes which tie rulers to ruled in a variety of political circumstances.

BETTY GLAD

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Political Ideology in Malaysia: Reality and the

Beliefs of an Elite. By JAMES E. SCOTT. (New Haven, Yale University Press, 1968. Pp. 302. $8.75.)

Sociopolitical transition has provided the principal matrix for analyzing the formation of political beliefs in new nations. The book under review suggests that we should be giving more attention to the impact of economic growth.

Scott is concerned with how existential factors shape top officials' perceptions of nature, man and, peripherally, the political arena. "Ideology" here conforms to Robert Lane's notion of an overall pattern of beliefs held by an individual, a pattern usually more observable to the interviewer than to the respondent himself. To pursue the Malaysian ideologies Scott adopted Lane's technique of in-depth interviews, open-ended questioning, and a relatively small sample. During 1965–66 Scott held long interviews with seventeen top ranking civil servants holding positions of considerable responsibility in the Malaysian Federal Government. He supplemented this small sample with written questionnaires submitted to a larger group of civil servants.

Formal ideology, public and politicized, may or may not coincide with an individual's personal and perhaps latent ideology. The degree of coincidence between the two is Lane's central concern in *Political Ideology* (1962). Scott is less concerned with the "fit" between public and private ideologies in Malaysia. Malaysian political leaders themselves have always taken pride in their pragmatic eschewance of ideological enthusiasm, though in practice they have adopted many liberal democratic forms and procedures. Instead, Scott concentrates on the degree to which his respondents fit an abstract concept of democratic ideology, one which is considerably broader and more coherent than any Malaysian public orthodoxy. The book sidesteps the analytical trap into which so many western observers have fallen: measuring a society's political well-being in terms of its consensus on democratic norms. In fact, one of Scott's chief propositions is that deviation from democratic beliefs—which is common among these Malaysian officials—is not, *a priori,* evidence of systemic disfunctionality. Quite the contrary. In Malaysia or in any country which is economically poor, socially heterogeneous, and in the throes of unprecedented change such departures from democratic beliefs may be evidence of perceptual *realism,* a requisite for effective decision-making. On the other hand, despite Scott's obvious sensitivity to the pitfalls of western parochialism, structuring the analysis around a democratic scale in itself undermines his own intent. The seventeen civil servants do not come off as pathological, but they remain deviants nonetheless.

This book is less about Malaysia than about the process of belief formation. Scott singles out Edward Banfield's *The Moral Basis of a Backward Society* (1958) and Lucien Pye's *Politics, Personality and Nation Building* (1962) as alternative explanations of this process. Between Pye's essentially psychoanalytic thesis of identity crisis and Banfield's environmental thesis of a poverty culture, he finds Banfield's more valid and useful and, one might venture, more optimistic. While Malaysian officals are plagued by the conflicts so characteristic of societies undergoing accelerated social mobilization, their expression of fatalism and social distrust are not fully explained by identity crisis and its accompanying "ego weakness." Their conviction that the world is relatively immutable and that the ordinary man is driven by a Hobbesian self-interest also derives from their acute awareness of the external conditions. Thus their fatalism and social distrust is witness to their realism, not simply to their internal instability.

The external realities having the most profound impact of the respondents' perceptions and expectations—and dampening their enthusiasm for democratic politics—are 1) the peasant social base which, in the midst of urbanization and modernization, remains the core of contemporary Malaysian society, 2) the ethnic communalism which generates violence and mutual suspicion, and 3) the scarcity of material and capital resources necessary for growth. Of these three factors, Scott assigns greatest weight to the latter.

The basic belief shared by these ethnically diverse (Malay, Chinese, and Indian), British-trained, English-speaking, urbanized bureaucrats is that rewards are relatively fixed in quantity; consequently, men must compete for larger slices of what is, in Scott's words, a "constant pie." It is this seemingly inescapable scarcity which generates men's selfishness and warrants cynicism toward public spiritedness and preference for paternal authority.

Scarcity and the "constant pie" syndrome are salient in newly independent nations and in older but similarly poor nations (or parts of them) as well. The existential repressants of democratic beliefs should be as common to Banfield's Italian village and Oscar Lewis's Spanish Harlem as to Scott's Malaysia. Scott's hypothesis, then, has relevance for more than just the post-colonial societies. There is a real need for comparative analysis which bridges the new nation-old nation dichotomy, and Scott makes a contribution in his direction.

In a country study one is always torn be-

tween detail and generality. Excessive attention to national peculiarities limits its theoretical utility, while concentration on general traits shared with other countries risks glossing over variables which should be taken into account in subsequent comparative analysis. Despite Scott's Malaysian expertise, he pitches his study at so general a level as to leave certain important questions unanswered. For instance, what is the precise nature of scarcity in Malaysia? Even affluent societies have been unable to escape guns-versus-butter debates. Decision-makers' sensitivity to the restrictions of scarcity, in fact, may increase as development proceeds. And a second question: how has the Malaysian bureaucracy related these civil servants to the nation's poverty or scarcity? Scott discusses the ideological consequences of the respondents' British training, but he might have gone further in investigating the impact of the decision-making role itself on the bureaucrats' beliefs, particularly in light of the Malaysian government's emphasis on economic growth. One would expect officials under the Alliance regime to differ from ordinary peasants and from bureaucrats in less growth-oriented governments in their attitudes toward the expandibility of resources.

Scott's exploration into the interplay between beliefs regarding scarcity and growth suggests that we should be giving more attention to personal time perspectives as a variable in ideological formation, since skepticism or optimism about the expandability of resources hinges so largely on whether an individual has a short-range or long-range frame of reference. Scott posits the Pye and Banfield approaches as principal options in analyzing ideology in underdeveloped socieities. But a focus on the implications of personal time references may bring the psychoanalytical and environmental explanations much closer together.

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The Eclipse of Citizenship: Power and Participation in Contemporary Politics. By ROBERT J. PRANGER. (New York: Holt, Rinehart and Winston, Inc., 1968. Pp. 110. $2.95.)

Action, Symbolism, and Order: The Existential Dimensions of Politics in Modern Citizenship. By ROBERT J. PRANGER. (Nashville: The Vanderbilt University Press, 1968. Pp. 225. $6.95.)

The first of these books is a clear, brief summary treatment of a large, important, and neglected subject, the philosophic theory of "democratic" citizenship. In that light, what the book gives us will be gratefully received. However, it is as inevitable that its shortcomings will be openly regretted.

The second of these two books, which treats mainly the fundamental defense of active citizenship in philosophic and/or psychological terms, goes far towards filling a gap left by the first but in ways so specialized as to be of probable interest only to some readers.

The first book steps off from a distinction between "the participation of citizens as an enterprise with values and distinctive forms of action" and, on the other hand, "the more ordinary struggle for power and authority usually associated with politics." (p. vii) This distinction becomes the basis for the book's two main, explicit arguments, the definition and description of "participatory" politics as a viable ideal and the definition and description of "power" politics, especially as revealed in the traditional "representative" democracies of Britain and America. The thesis is that the traditional representative democracy never did, and in recent decades, has increasingly failed to provide meaningful opportunities for "much spontaneous civic experience."

> The participatory spaces between authority and anarchy are neglected . . . Authority is promoted, maturity stymied. The adult citizen is almost dead, and with him will vanish the human being, autonomous and social, we in the Western political tradition have come to respect. (p. 102)

Behind these explicit arguments is another, less strongly stated but more general in its implications, and it sustains the whole book. This is the book's notion of what constitutes politics, or, better, political life. Whether done well or ill, political life, in this view, is a distinctively human process in which actors share membership in patterns of interaction and through these express to and with each other the hopes and fears of their communal life. The conception is spelled out in considerable detail, covering both objective and subjective aspects, and constitutes a distinctive and almost separable feature of the book.

It is a feature, of course, which is directly related to the book's other outstanding feature, its bias in favor of what in other circles these days is loosely referred to as "participatory democracy" (a phrase Professor Pranger abjures because its second term, "democracy," has come to cover a multitude of sins some of which he wishes to condemn). The two features complement each other admirably and together they result in a sharply defined restatement of an ancient political ideal which many praise and few practice. We should be grateful to have such a forceful and clear restatement at this time and can all hope that the book which contains it will

have a wide readership among, especially, our students.

But it is still a *re*statement and it can be asked whether Professor Pranger's efforts have taken us much farther than where, for example, A. D. Lindsay left us with his *The Essentials of Democracy* published more than thirty years ago. Is "participatory politics," with its heavy methodological emphasis on politics as a process of communication, anything more than a new name for "government by discussion"?

For this reviewer, the advocacy of participatory democracy which began with T. H. Green, and came to the United States in the works of John Dewey, as Professor Pranger notes, still lacks an American prophet. To fill that role, more than restatements of the ideal are needed. In the light of that ideal, American democracy must be condemned, not cursorily, superficially, and generally, but specifically, fundamentally, and at length.

In this connection, Professor Pranger's book can be seen as suffering from a further limitation. He appears to subscribe to the view that America is a nation peculiarly free of ideology (p. 101). He also appears to subscribe to the ideal of participatory citizenship almost as an end or value unto itself. In both cases, his vision would seem to be too narrow. Our system of representative government in this country is an aspect of our national commitment to liberalism and could not and would not be otherwise sustained. To the same scale, a conception of a truly active citizenry in which, to use Professor Pranger's own phrasing, "the citizen's primary role is to create a politics, not to function as an appendage, or mere participant, for the structure of power" (p. 89), is but an aspect of a larger and more fully comprehensive political philosophy, and one of a profoundly socialistic and anti-liberal sort. Professor Pranger's opposition between "participatory" and "power" politics is thus but an aspect of a much larger collision between liberalism and socialism. The persistent failure of Americans to bring this collision at least into honest view constitutes what W. A. Williams has called our "great evasion," and Professor Pranger is no more guilty of perpetuating the fault than a host of others. But it is depressing to see the situation unrelieved, even in the face of an unusual opportunity for doing so.

The failure to extend the argument into its ideological dimensions is even more obvious in the second of Professor Pranger's volumes, but in view of what this book does do it would be unfair to harp on what it omits. What it does is to search through a vast philosophical literature, including most prominently the works of modern existentialists, for an argument for the ideal of active citizenship which can be grounded in the realities of human experience and not just in personal preference or national heritage. In its own terms, the search is wide ranging, for it attempts to enlist novelists, theologians, and philosophers into common cause with social scientists and to have all of them find a common theme which will join the vocation of citizenship to that of the whole man, that is, politics to "joy, sex, and sleep," a phrase borrowed from Lawrence Durrell.

The search is for something more than the assertion that through political activism with fellow citizens the individual can discover, both socially and psychologically, a personal fulfilment and sense of identity not otherwise available to him. It is for an absolute ground for such an assertion, however tentatively its final discovery may be expressed. But it will take a special kind of reader to wish to follow Professor Pranger through this search to the end. This is not because, or, as the case may be, in spite of the density of the argument and the frequency of the citations. It is rather because many readers will suspect from the outset that the search is self-defeating. Either we are committed to the ideals of citizenship or we are not, and to ask why, either way, is simply to push the argument, as it were, into another room. The search for the uncommitting commitment is a chimera which Professor Pranger is no more likely to pull to earth than was Sarte or Merleau-Ponty.

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The Chinese World Order: Traditional China's Foreign Relations. Edited by John K. Fairbank. (Cambridge: Harvard University Press, 1968. Pp. 416. $10.00.)

There is a great deal to commend this volume to political scientists. An encyclopedia of factual statements concerning China's external behavior, *The Chinese World Order* comes close to being unrivalled for its probing case studies and scope of presentation. Historian John Fairbank and twelve of his associates and former students have summarized their researches into Chinese policies toward Korea, the Ryukyu Islands, Vietnam, Inner and Central Asia, and the West. Several essays treat the best known operational modes of Chinese foreign relations, particularly trade, the tribute system, treaty relations, and the *chi-mi* or "loose rein" form of diplomacy. Since Professor Fairbank summarizes each essay in the introduction to the volume there is no need to repeat his excellent overview of the book's contents.

In theory, China's foreign policy-makers

acted in accordance with a set of interlocking assumptions. As has been written many times before, these assumptions involved beliefs that the Chinese were naturally superior, that all outside "barbarians" were culturally inferior but differentially capable of harming China, that foreign states were ranked according to their potential threat and cultural affinity to China, and that states in each of three rankings or "zones" were considered equal among themselves by the Chinese. The three zones were a nearby Sinic or culturally Chinese zone, an Inner Asian zone where populations presented the dominant military danger to China, and an Outer zone. The Chinese world order thus revolved around China itself but was global in its extent. It was "unified and centralized in theory by the universal preeminence of the Son of Heaven," and drew on the political theory, kinship terminology and rituals used by the Chinese within their own land. According to Professor Fairbank this order has been passed on to the Nationalists and Communists and "bears upon Chinese policy thinking today."

The essays themselves thoroughly undermine the notion that one can meaningfully talk about "the Chinese world order" beyond the theoretical and psychological levels. The editor notes at the outset that this "order was a unified concept only at the Chinese end and only on the normative level, as an ideal pattern." Although one by one the authors struggle with the divergence between this ideal pattern and imperial practice, an essay by Wang Gungwu of the University of Malaya seems to cast doubt on the wisdom of continuing that struggle at all. Deceptively labelled a "background essay," Professor Wang's piece constitutes a fundamental criticism of the whole view that there was an all-embracing Chinese approach to world order from the Ming Dynasty (1368–1644) onward. If I understand his argument correctly, Wang is saying *inter alia* that there might have been such an approach prior to the Mongol conquest in the 12th Century, but that the total subjugation of the Mainland by the armies of Genghiz Khan and his successors over a hundred-year period invalidated the above-noted assumptions of Chinese world order. Then after the defeat of the Mongols and the restoration of the all-Chinese Ming Dynasty, the Emperor's historians deliberately sought to erase the century of humiliation by an even stronger assertion of the vitality of the Chinese world order. When prophecy fails, the prophets march forth.

A few of the authors in this volume, particularly Professor Benjamin Schwartz, seem to perceive how critical the points raised by Wang are. Schwartz indeed virtually dismisses the operational significance of the Chinese world order for modern times and instead discusses the important questions that arise from a Chinese perception that bears only tangentially on state practice. As Schwartz seems to recognize, this is more than a question of matching theory to practice, and cannot be understood as a problem of ends versus means. Here one gets principally into the realm of beliefs and domestic politics rather than of international behavior (although the two are obviously related). In this sense, as contributor David Farquhar suggests at one point, the Chinese world order had more to do with the internal authority of the Emperor than with matters of foreign relations. By exaggerating external issues and his own international prominence, for example, the Emperor could silence domestic critics and limit internal political conflict to his advantage. In addition Truong Buu Lam shows how recourse to the rhetoric of Chinese world order could sometimes play a significant part in the saving of face and in masking political vulnerability in diplomatic negotiations. The Chinese thus manipulated their view for the purposes of conducting a winning political strategy or making it appear to be a winning one to critics in the imperial court. In this fact-filled volume the explicit theory thus bears little relation to the rich data of foreign relations which deserve a reanalysis in comparative and systemic terms.

In his magnificently written essay, Wang Gungwu makes an additional point that seems to escape the majority of authors whose point of departure is the Chinese world order as such. He presents the best case I have seen on the importance of sorting out the many different "Chinas" that have arisen on the Mainland. China's relative power position has made a crucial difference to its policies; the leadership's ability to perceive and adjust to this position in turn has changed with varying patterns of external communications and domestic political circumstances. By fastening on the grand theme of Chinese world order, the more usual tendency in this volume is to homogenize the evidence so that the crucial distinctions for political analysis are lost. Wang's point in arguing about the Mongol conquest as a watershed is that Chinese policies must be understood in space and time and that any theory concerning Chinese political behavior must accept variation as normal rather than consider it to be a deviation from a preconceived world order.

Finally, Professor Schwartz' short essay at the end of the volume raises equally fundamental questions concerning the persistence of the Chinese world order since the collapse of the Ch'ing Dynasty in 1911. What Mr. Schwartz

says is not only that the question of world order was one of perception but also that the underpinnings of this perception were struck down with the collapse of the Empire. The apparent persistence of certain aspects of this perception, of course, can be explained as the transference of expressions and concepts dealing with traditional political attitudes to the new ideas of nationalism. But, the argument does not rest on the superficiality of historical continuity alone. The survival of bits and pieces, Schwartz argues, denies what was essential in the traditional perception: its integrated totality. The China of Chiang Kai-shek and Mao Tse-tung is different. The internal and external bases of the old order have irretrievably crumbled.

The Chinese World Order then is an important addition to a very thin field. It significantly contributes to our knowledge of the major dimensions of traditional China's external political behavior. These dimensions can now be studied more comprehensively by diplomatic historians and specialists in international politics and in much more concrete terms.

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NOTES

POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

Explanation in Social Science: A System Paradigm. By EUGENE J. MEEHAN. (Homewood, Ill.: The Dorsey Press, 1968. Pp. 128. $1.95.)

Constructing Social Theories. By ARTHUR L. STINCHCOMBE. (New York: Harcourt, Brace & World, Inc., 1968. Pp. 303. $3.95.)

Students of the method of science conventionally have asserted a distinction between what is called the context of scientific discovery and the context of scientific justification. Briefly put, it is held that methodology is concerned to explicate the logic of explanation and certain related epistemological problems. Where those explanations come from (how they are "thought up") is a problem for empirical investigation, and in principle does not bear upon methodological issues. Both Meehan and Stinchcombe seem to be arguing that philosophers of science have, perhaps unwittingly, misled practicing social scientists. We have accepted unrealistic criteria for being "scientific"; the business of doing science is not what philosophers of science seem to imply that it is.

Meehan argues that the "deductive paradigm" of explanation "currently accepted by most philosophers of science" is the villain. In Meehan's view, control is the central purpose of scientific explanation; therefore, we should consider any explanation a good one if it facilitates the desired control (he does not, of course, argue that all attempts to control are "good"). Meehan characterizes the deductive paradigm as follows: "An event is explained when it can be related to an established 'empirical generalization' or 'general law' according to the canons of formal logic. . . ." (p. 9) Such a paradigm is unduly restrictive and, indeed, impossible to achieve since "no proposition can be both empirical and general." Moreover, we cannot get out of the difficulty by concentrating on the confirmation of empirical generalizations "as Karl R. Popper and others suggest. No amount of empirical testing could possibly establish a general proposition. . . ." (p. 11) Meehan spends a little more time demonstrating that induction cannot justify a universal generalization and, therefore, that deduction from such generalizations is unwarranted. It makes little difference what Meehan says about that, though, since he is only constructing a straw man. Karl Popper—the very man Meehan singles out to attack—is most famous for his consistent argument that the only thing scientists can do is *reject* (disconfirm) generalizations; I cannot imagine how Meehan managed to read into Popper the contrary view.

What really puzzles me, however, is Meehan's alternative paradigm. He says that what we need for explanation is (1) a formal system (a calculus) which (2) is "loaded" with a given set of empirical terms (concepts) such that (3) the phenomenon to be explained appears as a formal entailment. Now, if Meehan thinks he has found an alternative paradigm of scientific explanation, he is flatly wrong. That just *is* the deductive paradigm, whatever he chooses to call it. If, on the other hand, Meehan thinks he is suggesting a new strategy for discovery, he is wrong again. May Brodbeck, in her widely reprinted article, "Models, Meaning, and Theories," speaks at some length to such a strategy. To provide empirical interpretation for the axioms of some formal calculus is not quite the piece of cake that Meehan seems to think, but it is being attempted all of the time, and it has long since been suggested to methodologists who have done their homework. In short, Meehan attacks the deductive paradigm with arguments from the context of discovery; he

fails to say anything very useful about scientific discovery itself.

Stinchocombe's book is all about scientific discovery. He is attempting to teach the reader strategies conductive to constructing explanations. Why should we believe that he knows how to construct explanations—that his strategies are any good? Well, Stinchombe gives us several kinds of reasons. First, he spends quite a lot of time assuring us that he is great buddies with all sorts of brilliant theory-builders (e.g., Coleman and Duncan). Second, many of us are not at Berkeley, and we know that, so naturally we will accept any direct-from-Berkeley knowledge Stinchcombe cares to dispense (". . . I evidently learned a good deal that was new to people not at Berkeley" [p. vii]). Third, he promises pretty spectacular results: "One way of describing this chapter is that in it we will try to teach everyone to write such books [as Union Democracy]." (p. 58) After that, I wish I could pan the book; actually, however, I think it is very good. It may indeed inspire some—if not "every"—one to write a book like *Union Democracy*.

If I read him correctly, the heart of Stinchcombe's strategy is in learning to conceptualize causally. To a methodologist, the results occasionally are shocking (". . . every concept must be . . . a *hypothesis* that specified phenomena . . . are causally operative;" [p. 40] ". . . variables may be measured either by their *causes* or by their effects;" etc.). But this is not a methodology book; it is a book about the construction of social theories. Stinchcombe adopts an intuitively appealing definition of cause (it may be methodologically unsound, but I believe that it corresponds fairly closely to the way most of us *think* about things), and restates all sorts of theoretical frameworks (e.g., structural-functionalism) in his causal terminology. He does not put great emphasis upon the need for operationalizing concepts (although it is clear that he means for concepts to refer to the world of experience), but he puts great stress upon the notion that concept formation starts with ideas about causal connections. Methodologists will recognize this as a strategy for assuring theoretical significance in scientific concepts.

After preliminary work of this sort, Stinchcombe takes the reader through stages of theory-construction. In a chapter appendix he treats linear directed graphs as a particularly appropriate technique for causal analysis (he doesn't say much about partialling techniques, etc.); other chapters deal with demographic variables, the conceptualization of power, environmental effects and activity-structure concepts. The examples are laregly sociological, but so, for that matter, is much political science specialized sociology.

Although the methodologically unsophisticated reader may not notice it at first, Meehan has written a bad book; although it may offend good methodologists, Stinchcombe has written an excellent book. Neither author, however, has eased my growing concern that the social sciences are in a state of crisis. With the advent of the scientific method, a world-view has insinuated itself upon us. To be sure, science implies control. All around us, social systems small and large convulse from overdoes of control, but I suppose I shouldn't worry. Stinchcombe is going to go back to the concrete and particular, "to try to wrestle it into sufficiently general form that it can be conveyed economically to our colleagues and descendants." (p. 293)—RODERICK BELL, *The University of Texas at Austin.*

Political Dialogues. By MAURICE CRANSTON. (New York: Basic Books Inc., 1968. Pp. 192. $5.95.)

Readers of political theory whose eyes tire easily following graph lines can find relief in the comforting prose of a new book by Maurice Cranston, Senior Lecturer in Political Science at the London School of Economics. Cranston is a representative of that vanishing species of political theorists which makes philosophy its sister discipline with no apologies and barely a courtesy to sociology. But of all Cranston has a felicitous way with words and a fine sense of style. Both in ideological position and style he suggests the best of A. D. Lindsay or Ernest Barker.

Using the format of imaginary dialogues, Cranston has contrived conversations between pairs of political philosophers ranging from Savonarola and Machiavelli on the State to Maine and Arnold on Democracy. Among the eight dialogues, Locke and Shaftesbury differ mildly over Tolerance, Diderot and Rousseau divide on Progress, Burke and Paine square off on Revolution, Marx and Bakunin fume over Anarchism, and Mill and Stephen coo about Liberty. Voltaire and Hume also have a session with Morality which meanders over too many subjects and proves to be the least satisfying of the dialogues.

It is easy to understand why writers shy away from the dialogue form. First of all there is going to be that inevitable comparison with Plato. Then there is the charge of presumption at orchestrating conversations which never took place. Also there is the necessity for a near complete command of the known writings of the subjects. What if a conversational coup with dialogue can be undone by an overlooked or unknown piece of writing? And there will always be the charge that the composer of the dialogues is being outrageously selective in his use of materials.

It is to Professor Cranston's glory that he has survived these objections. He had the boldness to use the Platonic format and not merely to be

awed by it. Rather it is the reader who is awed by Cranston's impressive command of his materials. The conversations are based on the actual words of the discussants. In some instances unpublished letters and notebooks are used as sources. This puts the critic at some disadvantage, especially one who is expecting to find a string of the familiar quotations fondly remembered from the History of Political Thought or frequently found embellishing Container Corporation of America ads.

Sometimes the arguments or anecdotes used are so unfamiliar to even a well schooled eye that a reader may wonder just how much of this is being fabricated. I hasten to add that Professor Cranston has my full trust on this score. The honesty of his scholarship is not in question. However, to readers who are so used to diving down to footnoted confirmation of every fact or argument, it may come as a shock to preceed from one beautifully phrased comment to another without the usual security of citation. Perhaps this is the supreme act of trust required by the dialogue form. You have to be certain that the subject did say what is attributed to him, especially when you find your favorite character voicing the unexpected or the unfamiliar. This can make a reader extremely uneasy about the whole idea of contrived dialogues. Unless you enjoy the pastime of literary detective, it can be very disconcerting.

Once this initial act of faith in scholastic accuracy is established, the dialogue can become a very refreshing medium for airing propositions. Cranston has certainly mastered the medium. His selected materials are smoothly woven together. Even if some of the material is new, it all sounds very plausible. Within the brief confines of these dialogues and with a minimal biographical sketch, the personality of the philosophers emerges with remarkable few strokes of argument. This is a fine tribute to the pertinency and quality of the remarks chosen. So little says so much. Even when the disputants had never met historically, you are often left with the feeling that it is a pity they didn't meet because it was such a nice conversation.

There is very little card stacking. It must be a great temptation to over draw the characters for dramatic effect. Take Machiavelli and Savonarola. At the beginning of this dialogue, Savonarola seems to be pouring forth vacuous ecclesiastical rhetoric. You are certain he is being set up as Machiavelli's straight man. Soon you see this is only part of the characterization. There are good lines and a plausible case for both men. Neither is pictured one dimensionally, and if ever there could be an understandable temptation to oversimplify characters, these are two very likely candidates. Instead we get a very fine discussion of the nature of force and fraud.

Friends of Rousseau might object that Cranston has been overly severe. Rousseau is drawn as a vulnerable and intolerant romantic. In their discussion of Progress, Diderot is continually upstaging him as the voice of calm reason. Rousseau is shown as essentially a Calvinist dreaming of a Christian Sparta. This questionable analogy suggests but does not begin to resolve the complex relationship of Rousseau's individualism and his collectivism. A rather awkward last minute introduction of his idea of social contract gives one the feeling that perhaps Rousseau was not getting equal time to present his case, or at least better arguments could have been mustered in his defense.

A similar feeling of being short changed in the dialogue comes up in the discussion of Tolerance between Locke and Shaftesbury. Locke is clearly the favorite. When Shaftesbury asks how Locke can dondemn all atheists *a priori* when we do not praise all Christians similarly, but rather insist on the test of experience, Locke never answers. The dialogue proceeds to another point. Once again a point has been hit but not developed. This is a definite limitation of the medium. One has to be content with sketches of ideas. Actually where responses assume essay proportion, as in the Mill and Stephen exchange, interest flags. The discourse has to be kept short to be effective, and this can result in skimping on ideas.

In a departure from the pairing of antagonists, third disputants are introduced into the dialogues on Revolution and Democracy. It doesn't appear that Mary Wollstonecraft (Why not her husband William Godwin?) is much help to Paine in taking on Burke. Her arguments are Paine's. But Morley contributes perspective in the exchange with Maine and Arnold over Democracy. His is an independent position and adds significantly to the dialogue.

The credibility of the dialogues is a strong point of the book. One's eyebrow seldom arches, but one does wonder if Marx could ever have shown such patience and forbearance in a real conversation with Bakunin over anarchism. Yet on the whole these excellent conversations ring very true.

These dialogues were presented over BBC Radio. They are meant to be spoken. The trick is to read them this way, to revel in their justapositions, sophisticated repartee, wit, and color and not look for a set of depth essays.—EDWARD R. CAIN, *Suny, Brockport.*

The Basic Values in Law: A Study of the Ethico-legal Implications of Psychology and Anthropology. BY THOMAS DAVITT. (Philadelphia: Transactions of the American Philosophical Society. Vol. 58, part 5, 1968. Pp. 144. $4.00.)

Any venture into the breach between disciplines

risks the likely consequence of being criticized only piecemeal and appreciated by no one. Yet some ventures may be worth the risks, since even a superficial punching and poking at the boundaries of our generalizations concerning social behavior may redefine some directions of inquiry. Professor Davitt's search for "basic values in law" must bear the scrutiny not only of his fellow jurisprudents, but of psychologists, anthropologists, and political scientists as well. But, regardless of the variety of shortcomings which might thus be levied against the endeavor, he asks us once again to consider the possibility of discovering (or inferring) universal value-judgments in the regularities of social behavior—and this means reassessing the possibilities for making comparative generalizations about legal "rules" or "norms" without necessarily contradicting our preoccupation with the study of decision-making processes which issue those norms.

Davitt presents his arguments for the universality of certain value-judgments in law in two parts: one linking values with law and the other surveying anthropological data for universal patterns of verification. In the first and much shorter part, it is argued that basic values—as certain appraisals of "our actions and their objects"—are derived from certain basic drives. The "naturalistic fallacy" is swept away by an insistence that "good" is merely an arbitrary designation for "the nonarbitrary and verifiable quality which some objects have of reducing drives, of fulfilling needs, of satisfying desires" (p. 8). And human motivations are presented as felt tendencies toward or away from objects, which are fundamentally evaluated according to the demands of basic drives. While not all actions are motivated and not all motives are *directly* related to basic drives, values are implied in all motivated activity. And lawmaking, it is argued, partakes of the motives, and hence the drives and values, common to all men. The basic drives, distinguished from instincts and characterized by a wide flexibility in means of fulfillment, are classified according to the familiar sort of motivational hierarchies which have been associated with the works of Abraham Maslow, Henry A. Murray, and Raymond McCall. Within the encompassing "master drive" of self-realization, man seeks self-preservation, sexual union, knowledge and freedom of decision, possession, and association in social and political unions. Further, Davitt contends that the basis of obligation and personal rights is derived from these basic drives, and that together values, obligations, and rights are the content of law.

Law, for Davitt, is particularly related to man's basic drives because the distinctive, definitional character of law—as a set of regulations—is found in its functional concern for the values common to all members of a society. Law if defined as "a directive judgment of those with authority regarding means necessary for the common welfare of the people" (p. 23). These directive judgments are distinguished from commands on the basis of general applicability and service to objectively common needs and values. Of course, since Davitt is concerned with data from preliterate peoples, he must insist with the directing authorities of law can be the people themselves, as well as their leaders or acknowledged delegates. And he hopes to clarify the proper inclusion of unwritten regulations in the identification of law on the basis of his notion of common welfare and the occurrence of public reaction to violations. Finally, it is argued that self-evident values in law are present universally (or almost universally) and require little or no reasoning or justification—while values which are not self-evident may be "general, common, frequent, and occasional," but not universal.

The second and greater part of the book is a presentation of the author's propositions about value judgments in law as their universality is supported by data from anthropological descriptions of preliterate societies. The universality of law, itself, is reaffirmed; and a classificatory schema is laid out for the location of value judgments along a continuum ranging from universal and almost universal through general, common, frequent, and occasional occurrences in societies. Of course, it is recognized that the reliability and inclusiveness of the evidence is only proportional to the accuracy and extent of the ethnographic reports; but Davitt argues that enough evidence is available to allow a classification of basic values in law which parallels his model of the basic drives encompassed by self-realization.

A brief overview of Davitt's findings reveals a small core of universal or almost universal value judgments derived from each of the basic drives—presented together with a much larger set of less than universal values. From the basic drive of self-preservation is derived the universal approval of self defense and regulations controlling killing, and condemnation of unjustified (relatively defined) killing. The drive toward sexual union delivers up such universal judgments as the approval of regulating sexual activity, of selectivity and permanence of relations, and of marriage and family. And the disapproval of polyandry, adultry, and incest are "almost" universal.

Man's drive to know results in the universality of values concerning the regulation of education and knowledge serving common needs, the good in language and artistic creation, and the desirability of cosmic explanations. The drive for freedom of decision is apparent in the universal values calling for regulations guiding decisions, the relating of responsibility to decisions to act, and even the

"decision" to observe customary law. The essence of justice, found in the basic drive to distinguish "mine and thine," is the basis for universal value judgments regulating property relations, allowing individual title, and regulating relations of acquiring title to things—to the exclusion of theft. Finally, the basic drive to associate with others accounts for the universality of judgments which value protection and security, the necessity for cooperative effort, regulations assuring peace and security, conformity with directives, and the pursuit of relatively defined justice through the judging of cases. Almost universal is the value of distinguishing between direct and indirect offenses against the whole community.

While for each of the categories examined extensive references are made to anthropological literature, the book should not be taken for a systematic survey of "legal anthropology" or even for a treatise on the impact of anthropology upon definitions of law. Neither can it be expected to contribute much to our understanding of the judicial process as an analytically defined aspect of social interaction. For this is primarily a book about legal norms—about the inferred presence of value judgments considered as "facts"; and it speaks most forcefully in the field of jurisprudence. Davitt's straw man is the *legal* positivist, for whom values as absolute ethical judgments are excluded from law defined as command. And he argues that the *universality* of certain value judgments, which through logic and an implicit functionalism can be inferred from regularities in widely comparative contexts of social interaction, is demonstrative of the *necessary* inclusion of basic values in all law.

For social scientists, *The Basic Values in Law* will be less than satisfying in a number of respects. Davitt never seems to understand the need to clarify the methodological questions involved in the identification of the regulations or customs which in particular are law. He uses the distinguishing characteristic of service to "common welfare," but does not fully recognize his implicit dependence upon the concept of observable sanctions—as "common responses" to violations. And this failure contributes to serious difficulties in demonstrating how a number of his value judgments, even if accepted as "reported data," are necessarily part of customary law. The entire chapter on knowledge and decision, in particular, seems to be a presentation of apparently universal regularities in behavior without any attendant demonstration that these regularities are law—or "regulations for common welfare." Davitt cites no clarificatory examples of public reaction or sanction for transgressions of the norms which might distinguish them as *legal* rather than merely general, *social* postulates!

While it can be left to the anthropologists to correct or alter any questions concerning the accuracy or generality of the data collected, we might note that except perhaps through some process of negative demonstration it is difficult to understand the operational criterion for selecting the content for the universal category. And, as we have seen, the guidance of the basic drives model seems to allow an unwarranted *forcing* of certain values into the legal framework. The additional and extensive outlining of "less than universal" value judgments proceeds according to some "considered estimate" of generality—often in the face of anthropologist's recognitions of the lack of sufficient data.

In spite of the shortcomings mentioned here and in addition to the fundamental emphasis on jurisprudential concerns, Davitt's work is an interesting exploration in the area of comparative generalization which presses to a wider audience the possible uses of anthropological data and fosters renewed interest in the comparative analysis of values as legal content. The work is worth critical examination, if only because of the implications of the topic.—D KENT MCCALLUM, *York University, Toronto.*

Totalitarian Rule: Its Nature and Characteristics. BY HANS BUCHHEIM. Trans. by Ruth Hein. (Middletown: Wesleyan University Press, 1968. Pp. 112, $4.50.)

The author, an associate of the Institut für Zeitgeschichte in Munich, has both personally experienced and written widely of the Third Reich. The book under review here is a translation of a work published in Germany in 1962—the first of his studies to appear in English. With just under 100 pages of text, the book can best be characterized as a long essay containing a number of impressionistic theories about totalitarianism. There is no attempt at systematic empirical description, nor at the construction of empirical indices for various factors which are related. Concrete evidence is brought in only when needed to illustrate a point. Evidence is drawn primarily from the experience of Nazi Germany and the Communism of the Soviet Union, with brief references for the purpose of contrast to Mussolini's Italy (considered nontotalitarian) and other fascist regimes.

In the first and longest chapter, the author defines totalitarian rule ("the demand for unlimited control over the world and hence social life"), differentiates it from other non-democratic forms, and suggests its motive force: a threatened group (the Germans; the working class) can only be saved if certain theories are acted upon as historically valid laws (the good life comes with the "nordification" of the German people; or with the achievement of communist society). To create a wholly new society and type of man, every area of life must be manipulated—the family, scientific re-

search, art, the individual conscience. From this arises the terrible oppressiveness of totalitarianism, which sometimes affects adherents of the regime even more than its opponents. The author also compares National Socialism with Communism and finds them more alike than different. Subsequent chapters examine life under totalitarian rule, especially various methods by which the regime seduces the populace into supporting it; the totalitarian goal of a life without politics; the totalitarian mode of thought and its historical antecedents; the role of the state in a totalitarian system (a tool); and the limits of totalitarian power.

To the author's credit, most of his theories appear reasonable and do not fly in the face of generally accepted knowledge about totalitarianism. This might be attributed, however, to the fact that there is little that is really new for the serious student—only here and there a novel emphasis or insight or different way of phrasing a well-known point. The lack of originality could not be considered a drawback if the author had attempted a synthesis of knowledge in the field. Unfortunately, he includes no footnotes, and except for a few references in the text, the reader is left unaware of how the author's theories have been influenced by the ideas of others.

In the several instances where theories are not convincing, the trouble seems to derive at least partially from theorizing at a level too far divorced from empirical referents. For example, the author attempts to explain the varying degrees of intensity of totalitarianism among fascist regimes by the relative lack of intellectual substance (e.g., clear-cut goals) in their ideologies, as compared with communist regimes. Thus, in the face of obstructions, such as "an ideologically solidly established society or strong political opposition," a Mussolini or Franco could "remain content with relatively minor changes" in society. Hitler's totalitarianism was more intense because the German body politic offered no significant obstructions. The author sums it up: "though Fascism contains the tendency to a totalitarian claim to power, it is not—unlike Marxism-Leninism—determined on such a course. . . ." The implication is that all communist regimes will have approximately the same totalitarian intensity, and a look at the real world would demonstrate that this is not true. In any case, without some kind of rough indicators of "intensity" or "intellectual substance" or "obstructions," it is impossible to say whether this theory seems valid or not. And the same criticism can be leveled against most of the author's theory-building.

Additionally, one can question the priorities which the author assigned in choosing topics to theorize about. For example, in explaining the success of totalitarian movements in Germany and Russia, the author focuses on very general conditions, such as man's yearning for a closed intellectual system or his proclivity in modern times for a rationalistic, social-engineering mode of thought, rather than on factors which seem more immediate, like the economic chaos of the two societies. Or in dealing with totalitarian politics, the author only makes the abstract point that the ruler cannot personally supervise everyone; thus, functionaries must necessarily exercise some discretion and thereby have political power. The author, however, does not take up actual manifestations of politics, for example, the interest group politics of the Soviet Union which seem to contribute so importantly to the dynamics of that system.

Because theorizing is done at a high level of abstraction with few references to concrete experience, this is not a work for a beginning student. To follow the author requires a great deal of knowledge about the real systems involved. Moreover, the vocabulary and manner of expression are not that of a modern, empirical political science, but a more formalistic school of German political philosophy. The knowledgeable lyaman might find the book interesting and worthwhile, however, for totalitarianism is intrinsically a fascinating subject, and theories about it are always provocative. And the political scientist might find it refreshing to be forced to reexamine his knowledge from a different persepctive. It is in these respects that the work has value.—RICHARD J. WILLEY, *Vassar College.*

Perspectives in the Study of Politics. EDITED BY MALCOLM B. PARSONS. (Chicago: Rand McNally and Company, 1968. Pp. 170. $7.50.)

According to the editor, the essays in this collection "attempt to explain how present-day scholars in disciplines other than political science view political phenomena, as well as how political scientists themselves view these phenomena." The former concern is the province of the eight contributors from other disciplines: Wayne A. R. Leys (philosophy), Norman A. Graebner (history), Harry W. Jones (law), James M. Buchanan (economics), Daniel Lerner (sociology), M. G. Smith (anthropology), Hadley Cantril (psychology), and Norman J. G. Pounds (geography). The latter concern is explored almost exclusively in editor Parsons' own introductory essay, which is essentially an historical sketch of trends in political science and a synopsis of the essays that follow.

Since there is no evidence of a prescribed format beyond the amorphous formulation quoted above, it is not surprising that the remaining essays are extremely varied in approach. They can, however, be sorted into two broad categories. In the first category (comprising the essays from phi-

losophy, history, law, anthropology, and geography) the emphasis is on the relationship between political science and the approaches, concepts, and theories of the other discipline. The essays in this category are all informative, and, in many instances, they contain provocative ideas as well.

For example, Wayne Leys suggests that the unstable pluralism of contemporary political reality generates a central philosophical question: "How can a man, dealing with different issues in different kinds of games, achieve some sense of personal integrity and preserve his sanity?" On the way to an answer, we must somehow "escape from an idolatry, a worshipping of one kind of political process" and "secure and make available promptly accurate information regarding the consensus of effective participants as to the arena in which a given issue should be decided." This point of veiw seems eminently worth exploring.

In the essay from history, Norman Graebner states that, in his view, "social science cannot subject mankind to successful predictability." As a consequence of this estimate of the possibility of prediction, "the majority of practicing historians pursue their researches unmindful of the content of the new empirical studies." This state of affairs we must regret, and we hope that such barriers to disciplinary cross-pollinization are only temporary. (We might note that the use of the verb "subject" in the first quotation suggests an underlying ideological anxiety, a fear that mankind is perhaps predicatable after all, that scientific laws will be constructed "which will control human activity." We suspect that it is this unfortunate and, we believe, inaccurate equating of prediction with control that contributes to a lack of communication between history and political science.)

In the contribution from law, we have a concise and informative summary of legal research relevant to political science. The contributions from anthropology and geography both summarize and critically evaluate their disciplines' progress toward creating theoretical perspectives. Overall, this entire first category of essays provides useful introductions to recent work of relevance to political science.

In the second category of essays (comprising the contributions from economics, sociology, and psychology) the emphasis is on the personal theoretical approaches and ideas of the authors. James Buchanan, for example, fashions an argument against traditional treatment of political institutions by economists and in favor of what he calls "methodological individualism," an approach he and Gordon Tullock first championed in a major way in *The Calculus of Consent*. This sort of intentional semipolemic should help sharpen needed investigation into theoretical and methodological assumptions and approaches.

Daniel Lerner discusses the relation between sociology and policy research, not political science. How he relates the three remains somewhat unclear at the end of his contribution, over a third of which is a quotation from an earlier publication.

Hadley Cantril summarizes a number of his ideas within the framework of "transactional psychology," a term handed down from Dewey and Bentley. For political scientists, one of his more useful notions may be a classification of motivations, of "demands the human imposes on society." Among other things, this kind of effort may contribute to a more adequate way of describing and evaluating the effects of governmental decisions.

In sum, the essays in this volume, allowing for occasional lapses, are of good quality. Individually, they are worth the serious attention of the political scientist. However, granting that academics are notoriously difficult to hold to a common format, this collection inevitably suffers from discontinuity. The contributors scatter to the four winds, and, to be fair to them, there is no evidence of a serious attempt on the editor's part to keep them corralled. All this may be a function of a more general problem—the poorly defined purpose of the volume. Nowhere does the editor clearly state how interdisciplinary relationships were to be explored or what audiences he had in mind. It cannot be denied that there may be value in giving a number of intellects free rein with a given subject matter. However, in this context, with just one selection per discipline, the non-comparability from one essay to the next lessens the impact of the work as a whole.

The interrelating of political science and other disciplines is a topic of considerable importance. This volume indeed contributes to the understanding of these relationships. It seems to this reviewer, however, that the issues involved deserve far more explicit and extensive probing. To this end, a format which, for each discipline, would elicit the viewpoints of *several* of its representatives and *several* interested political scientists could be of inestimable value. (The interdisciplinary sections of Sidney Koch's edited series, *Psychology: A Study of a Science*, are a fair approximation of what we have in mind.) We need thorough discussion of such issues as the extent of substantive overlap among disciplines, the validity of borrowing theories, concepts, and methods, and the potentialities and dangers of interdisciplinary research. Hopefully, the present volume will help provide an impetus to these more ambitious undertakings.—ALLEN R. WILCOX, *University of Nevada*.

Hobbes's System of Ideas: A Study in the Political

Significance of Philosophical Theories. By J. W. N. Watkins. (New York: Barnes and Noble, Inc., 1968. Pp. 192. $1.95.)

In view of Hobbes's secure position as the most exciting and profound of all those who have written political philosophy in English, it is surprising that the amount of secondary literature about him which is both valuable for beginning students and interesting to more sophisticated readers is very small. Watkins has furnished a welcome addition to our small resources in these respects; his book is well-written, responsible, provocative, and altogether a pleasure to read. If the purpose of a review is to advise the relevant public whether a book is worth reading, the answer here is unhesitatingly in the affirmative.

There are some disappointments. Watkins' considerable abilities are apparent, but so also is a certain negligence in ordering his thoughts. The opening lines of the Preface state that the book answers the question: "How much of Hobbes's *political* theory is implied by his *philosophical* ideas?" It takes a close reading of the text to find the reply to this question and when found it is extraordinarily inconclusive. Fortunately, political theorists are less interested in such implication than in consistency, and about Hobbes's consistency there are only marginal doubts.

The organizing principle of the book is not evident; topics follow one another in no particular order. No serious attempt is made to convey an overall view of Hobbes's philosophy or political thought, to relate his thought to his period, to show what problems he attempted to solve, or what influence those solutions had. Of course, a book which attempted to do *all* these things might be intolerably long, but one or some of them might have been attempted, particularly given the author's gift for vivid and accurate summary.

The best parts of the book deal with topics in metaphysics. The author treats clearly and well of Hobbes's nominalism and materialism-motionalism and discovers what I believe is a previously unnoticed connection between Hobbes and Leibniz which increases considerably our appreciation of Hobbes's intellectual power and originality. More conventionally, but in a section which is a model of critical exposition, Watkins explains the relation between Hobbes's scientific approach and that of Galileo and earlier members of the School of Padua. More of this would have been a welcome addition.

A major topic in recent Hobbes literature has been the character of the obligation to obey the laws of nature in the state of nature and the relation of that obligation to the obligation to obey the laws of the sovereign in civil society. Watkins argues persuasively for what I suppose is now the general opinion, that such obligations are neither moral nor religious, but prudential. More precisely: "Hobbes's laws of nature are assertoric hypothetical imperatives in Kant's sense." (p. 83) In a footnote (p. 99), Watkins relates the contrary Taylor-Warrender thesis to those recent studies of Locke which tend to assimilate the latter to the austere authoritarianism usually associated with Hobbes and says:

> This situation is painful for examination candidates liable to be asked to 'compare and contrast' Hobbes and Locke. So let us all agree to the following compromise: Hobbes was a moralizing lawyer in the Hooker tradition, while Locke preached a mixture of egoism, fear, and authority; and Hobbes wrote *The Second Treatise,* while Locke wrote *Leviathan.*

The author writes from the standpoint of the contemporary English analytic school of philosophy ["My largest debt is to Karl Popper. His has been the main influence on the viewpoint from which the book is written . . ." (p. 11)] and the cramping effect of that commitment is visible throughout most of the book. On occasion, however, the commitment seems forgotten. On pp. 175–6, in connection with a discussion of Hobbes's definition of liberty as the ability to do what one wants, Watkins asks us to imagine two members of an underground organization in a totalitarian country, both of whom are captured and 'brainwashed.' A is rehabilitated and released and gets a good job with the regime; B successfully resists conversion and is sent to a labor camp:

> A is doing what he wants to do, B is not. But there is an important sense in which it is A who has become a captive of the regime, and B whose soul has remained unenslaved, although he is now doing slave-labour. Hobbes's concept of liberty makes no provision for this sort of inner defiance.

Is Watkins sending the founder of linguistic analysis back to study under Plato and Spinoza? Perhaps Watkins himself should go, for here, and elsewhere in discussing justice and morality, he transcends his adopted viewpoint only to land at the most unsophisticated level of the traditional normative position.

Watkins labels his own criticisms of Hobbes's political conclusions "pedestrian" and as only his "personal opinions." They are indeed pedestrian in the sense of being commonsensical, *i.e.*, culture-bound and ideological, but what it means to label them "personal opinions" escapes me. Is it implied that there is some transcendental ground of criticism which he admits not having reached, or some scientific approach which he has not mastered, or does it simply mean that we are not to take his thoughts here seriously? The reader will probably prefer the last alternative.

Yet, despite all these imperfections, it must be admitted on balance that Watkins has produced an excellent book. In the many matters on which he is competent, he shares with Hobbes some of

the latter's inimitable instinct for the jugular, in instinct well appreciated by our best students. Perhaps, given a prior feeling for politics, history, and humanity, no other characteristic is more important in writing or understanding political theory.—ROBERT J. MCSHEA, *Boston University.*

Social Science in Latin America. EDITED BY MANUEL DIÉGUES JÚNIOR AND BRYCE WOOD. (New York, Columbia University Press, 1967. Pp. 336. $4.50.)

One does not expect discussions of scope and purpose in the social sciences to be particularly illuminating; this collection of papers presented at the Conference on Latin American Studies in Rio de Janeiro in 1965 confirms this apprehension. The contributors, all Latin Americans, were brought together under the auspices of the Joint Committee on Latin American Studies of the American Council of Learned Societies and the Social Science Research Council "to reinforce the bonds among social scientists in this hemisphere." The Conference may well have succeeded doing that, but the published papers add little more to the effort. Two of the essays are useful bibliographical contributions, one on Anthropology by Fernando Cámara Barbachano and the other on law and legal sociology by José María Franco. The other papers are confusing and unstimulating. This result reflects not so much shortcomings of the authors themselves but rather the difficulties inherent in addressing issues of scope in the abstract.

The scope of a discipline is the result of thousands of scholars producing, criticizing, modifying and extending specific lines of enquiry. Thus, a book review is a discussion of scope. The writer argues, in effect, that a particular topic should or should not be pursued in a particular way for these or those reasons. Obviously one cannot, in a single paper, make all the specific criticisms and corrections of all the work appearing in the discipline. Consequently, a generalized discussion of scope is usually a covert book review: the works treated are often unmentioned and the specific criticisms rarely detailed. The result is shadow-boxing.

In his Introduction to the collection Manuel Diégues Júnior criticizes "foreign experts, whose models, although carefully prepared and highly refined, are rarely compatible with the social reality of our own countries" (p. 12). Which experts? Which models? Which reality? Why, specifically, are they not compatible? The author does not say.

Florestan Fernandes inveighs against North American scholars working in Latin America for diverse vices: being "more or less indifferent to what is happening in Latin American communities" (p. 53); holding "old stereotyped opinions about the significance and scope of indigenous advances [in the social sciences]" (p. 51); being, "as a rule, completely ignorant of the language and of the economic, cultural and social history of the areas selected" (p. 32). No names are given, no works cited; far worse, no concrete—let alone detailed or thoughtful—defense of such propositions is offered. It is tempting to criticize such criticisms, but to do so would plunge us into the same lack of concreteness which afflicts the criticism. We desperately need specific examples. If, for example, Fernandes knows of a neglected Latin American scholar, then for Heaven's sake, he should tell us about his work and explain its utility.

José Nun, writing on political science, argues that political philosophies—such as Marxism or liberal capitalism—are our "models" performing a function akin to that of the wave theory of light in physics. (Many of us would feel slippage between analogies.) Nun then speaks against certain philosophical positions and in favor of "critical Marxism" as a research model. Unfortunately, he does not explain what, in practical, concrete terms, the significant characteristics of each "model" are, nor how, specifically, each leads or does not lead to knowledge.

The writers of *The American Voter* are, for example, accused in a passing reference of "formalism" (p. 80). Formalism, incidentally, is "an ideology of liberal capitalism" which combines a "liberal democratic conception of politics" which is a belief in "equality as a function of liberty" and an "irreducible dualism: society and state" (see Locke, Hartz, Hobbes, Laski and Hegel for further clarification) *and* an abstract view of man which is man defined "by his essence"—if you know what I mean. Obscured by this metaphysical fog is the issue of interest to political scientists: what specific findings in *The American Voter* are undermined by the use of "formalism" (whatever it be) and how? Nun makes no attempt to show it. We are left with mere name-calling—only we do not even know what the names mean.

Melioration was one theme which appeared in most of the papers. The writers seemed agreed that research ought to be undertaken with the end of eventually making things better in Latin America. But again, the high level of abstraction prevented the concrete discussion of the real issue here, namely: which specific lines of enquiry lead most productively to useful knowledge and why? For example, what are we to make of the rhetorical question raised by Victor Flores Olea in his discussion of political science: "Are Latin American specialists only to be concerned with reinforcing the status quo and, ultimately, with defending and justifying an order that is unsatisfactory for the majority of the people, and, at times, unjust

all around?" (p. 185) Is this merely banal demagogy or an exhortation to terrorism? No answer is provided by Flores who does not say *who* is justifying *which* unsatisfactory order, *how*. Which book is he reviewing?

The results of this conference suggest, then, that formal discussions of scope and purpose ought to be avoided in favor of substantive matters where issues of scope and purpose will be explicitly—and therefore productively—addressed.—JAMES L. PAYNE, *Wesleyan University*.

Social Class and Social Policy. BY S. M. MILLER AND FRANK RIESSMAN. (New York: Basic Books, 1968. Pp. 302. $7.95.)

At a time when social science is being asked to appraise and reevaluate its relationship to social problems (e.g. the "new sociology," the "new political science"), this volume by two scholars unambiguously committed to *applied* social science, is an excellent exercise in problem-science linkages. S. M. Miller, who is Professor of Sociology and Education at New York University and Program Advisor in Social Development at the Ford Foundation, and Frank Riessman, Professor of Educational Scoiology and Director of the New Careers Development Center at the same university, make it quite clear from the outset that the book represents a bridge between their concern with social problems and social change, and their social science training and interests. They are committed to a dialectic between social science and social problems: to develop and test social science theory by applying it, while enhancing the utility of social science to "actionists" by clarifying problems, and aiding in the development of effective strategies and techniques to solve them.

Although one would have to conclude that the book is primarily devoted to action programs and change strategies, these programs and strategies are based on sound analyses of existing situations in three substantive policy areas (poverty, education, mental health). The student of public policy will find many significant and useful insights, conceptualizations and typologies in this volume. However, both the action-oriented and the theory-oriented reader will have to pour through material of primary interest to the other in order to get at the parts he is primarily interested in. Both ought to benefit from the exposure.

Substantively, *Social Class and Social Policy* is about urban poverty and the urban poor (the "new working class") in America. The eighteen chapters of the book are divided into six sections. The opening section ("Policy Objectives") outlines what the authors call the "relative view of poverty," i.e., relative to the "possibilities of society and the 'average' standard of life in a society." To be poor is to be excluded from full membership in society. They criticize the "insufficiency of income" theory of poverty as inadequate, and offer a more broadly conceived concept of "inclusion through command over resources"—economic, political, service, psychological, and social. In an affluent society, the reduction of poverty involves increasing the range of meaningful choices in relation to the choice increase of the non-poor.

The next three chapters ("Social Class: The Poor Today") characterize the current state of poverty. The utility of distinguishing among the plurality of poverty types is stressed. By combining measures of class (economic security/insecurity) and life style (familial stability/instability) Miller and Riessman develop a typology of poverty diversity (stable, strained, copers, unstable). The authors assume that there are alternative poverty alleviation or melioration strategies available (direct economic aid, direct services, indirect change through "climate" changes) and suggest that the concept of poverty diversity is useful in fitting policy program to poverty type in order to maximize the amount of positive change.

In addition to a typology of the poor themselves, Miller and Riessman analyze and critique the major alternative views of the poor—the underserving poor, the self-defeating poor, and the victimized poor—indicating how definition of the problem affects program proposals. Expectedly, they use this opportunity to criticize the "culture of poverty" thesis (a broader version of the self-defeating poor which implies "an intergenerational transmission of values and practices which inhibit constructive action") as unduly pessimistic. They insist that the positive qualities of the poor be emphasized if successful programs are to be developed.

Sections III, IV, and V, which constitute two-thirds of the book, critique three policy areas relevant to the poor—education, mental health, and community action programs. What we aren't told however, is why *these* three; no criteria of inclusion or exclusion are offered, despite the fact that mental health is not usually considered to be as critical as, say, housing or the welfare system.

The discussions of all three policy areas stress the necessity for developing a variety of "social interventions" to cope with the pluralism of poverty. In the six chapters on education, there is strong emphasis on flexibility and adaptability to get out of the "credentials trap" which represses the mobilization of what talents the poor have, to deal with the drop-out problem (education is a "discontinuous process") by providing meaningful "second chance schools," by accepting poverty and policy pluralism and avoiding the one solution syndrome in school administration, teacher training and utilization, pre-school programs, and integration (where a rigid position in opposition to seg-

regated community control seems contradictory).

The plural approach to change strategies is continued in the section on mental health, where current institutional features of psychotherapy are described as "not congenial to low-income clients, not congruent with their expectations and traditions, and poorly understood by them." Miller and Riessman suggest modifying treatment approaches (e.g. introducing role-playing, use of non-professionals, "helper" therapy, family approaches, socio-therapy, and "pills and needles"), in addition to educating the poor to create new or modified expectations about treatment.

The four chapters which comprise the section on "Poverty and Community Action" are concerned with the variety of forces acting to alleviate poverty and improve conditions of the poor. Three forces are identified: the "inside" force of the established social agencies which constitute the social service network, "outside" militant pressure groups, and mediator agencies which work with both the inside and outside groups.

In light of the new action framework created by the war on poverty and the civil rights movement, new and more innovative programs and processes that are relevant to the demands and needs of the poor are suggested for the traditional agencies: neighborhood service centers, consumer participation in conduct of services, and the widespread use of non-professionals.

The conflict-confrontation oriented outside groups (examples cited are Alinsky and the New Left) are considered too revolutionary, and concerned too much with increasing a sense of efficacy among the poor and too little with substantive gains.

The "third force strategy" of the local poverty programs (CAPS) is designed to promote change in social service activities without inevitably creating community disruption. The strategy stresses intervention between the demanders (poor) and the suppliers (established community institutions). Although the authors, consistent with their concern with pluralism, insist that all three strategies have merit depending on the circumstances, they clearly favor the community-action strategy which "offers social delivery, not a social movement or a comforting continuation of past programs."

Despite the analytic insights spotted throughout the volume, the book suffers from theoretical eclecticism, substantive exclusion and unevenness of development. This may be the result of the division of labor among the authors (including several, in addition to Miller and Riessman, who co-authored some chapters). But this should not detract from the important insights into significant urban issues nor the highly relevant treatment of the policy science question. Besides, the authors promise us a follow-up volume in which they will distill the implications of their analysis for general social science theory. Hopefully, the promise of *Social Class and Social Policy* will be fulfilled in the authors' next book.—Louis H. Masotti, *Case Western Reserve University.*

Freedom and Community. By Yves Simon. Edited by Charles P. O'Donnell. (New York: Fordham University Press, 1968. Pp. 201. $5.50.)

Yves Simon was one of the foremost Thomist theorists of democracy. All those of us committed to constitutional democracy who have recognized the accuracy and significance of Paul Tillich's observations that "the intellectual defense of Anglo-Saxon civilization against Fascist ideologies is extremely weak," and that "common-sense philosophy and pragmatism are not able to provide criteria against the dynamic irrationalism of these movements" should regard with interest and appreciation Simon's conscientious attempt to make good this theoretical deficit from his own philosophical perspective. Simon concentrated his attention especially upon the too-often neglected problem of the role of authority in democratic society. The impetus behind his work was the conviction that a proper understanding of constitutional democracy can be obtained only through an appreciation of the interplay of freedom and authority—concepts which were widely and facilely held to be unqualifiedly antagonistic under the premises of classical liberalism's ontological simplicism.

For those who are not themselves either Catholics or Aristotelian universalists, however, Simon's answers to the critical problems he examines are bound to seem *ultimately* unsatisfactory. Admittedly, democratic liberty certainly must be conceived as something other than simple license ("absence of impediment," to use Hobbes's phrase), and we are in Simon's debt for his insistence on this score. But on the other hand, surely it is an excessive contraction of the notion of freedom that leads to the belief that "any guarantee against the risk of a wrong choice is purely advantageous to freedom as such." (p. 59.)

At a critical juncture the Catholic belief in and adherence to an institutionalized principle of authority which must be accepted on faith is perhaps not fully able to assimilate the deep epistemological humility in the realm of moral knowledge that is so basic to the rationale of constitutional democracy. Nevertheless, no political order, democratic or otherwise, can survive a complete lack of authority or complete moral skepticism, for order of any kind is incompatible with genuine anarchy or total nihilsm. As political theorists try to discern the distinguishing features of democratic political order, therefore, they can profit

from the thoughtful, theoretically sophisticated formulations of Simon, a committed and insightful analyst who took these problems seriously.

The present volume is a collection of six essays centered broadly around the problems of freedom and community. The editor, Professor Charles O'Donnell, remarks that *Freedom and Community* is intended to exemplify the author's dedication to freedom, a dedication which O'Donnell suggests may have been obscured by the fact that Simon's work "has been so closely associated with the idea of authority." (p. ix.) In fact, the general belief that Simon emphasizes the centrality of authority in any society, including democracy, will probably not be substantially modified by this book. It conceivably can be argued that freedom, rather than authority, was actually the central concern of his theory of democracy; but to do so it must be admitted that Simon's distinctive concept of "freedom" jettisons some of the elements which have generally been considered essential to that word's meaning.

Three of the six chapters have appeared previously in other formats. These previously published essays, "Freedom in Daily Life," "Liberty and Authority," and "Pessimism and the Philosophy of Progress" are by literary standards the most satisfactory parts of the book. The remaining essays, "Freedom and Community," "Autonomy and Authority," and "Political Society," previously unpublished, are manuscripts that Simon originally prepared as the basis for lectures given at the University of Chicago in 1955, and which he intended ultimately to comprise part of his proposed "Philosophical Encyclopedia." Since the manuscripts were still in incomplete form at the time of the author's untimely death in 1961, the editor has utilized notes which Dr. Vukan Kuic took from the original lectures as aids in supplementing them.

Admirers of Yves Simon will certainly be grateful to Professor O'Donnell for his efforts in making these essays available, even though the logical coherence and completeness of the new chapters is not all that might be desired. The editor understandably and wisely sought to keep his interpolations to a minimum; and the relative sketchiness of the final copy is an unavoidable result of this editorial fidelity. Nevertheless, the final product is a group of essays whose quality is not quite up to the usual standard of Simon's work; they are rough-hewn wood-work rather than polished gems. The present volume, therefore, constitutes a rather marginal addition to Simon's *corpus*, as the editor himself would undoubtedly attest; but in the case of thinkers of Simon's stature margins may justifiably be deemed significant.—THOMAS A. SPRAGENS, JR., *Duke University.*

The Political Philosophy of Rousseau. By ROGER D. MASTERS. (Princeton: Princeton University Press, 1968. Pp. 464. $12.50.)

Among the many books on Rousseau's political philosophy, this one stands out because of its form. Some of Rousseau's major works are considered, in turn, as whole books: the *Emile,* the *Discourse on the Origins of Inequality,* the *Social Contract,* and the *Discourse on the Arts and Sciences.* Reading a work written in this manner is almost always instructive for the reader because he can learn about the author by matching his understanding with that of the interpreter in the face of the text as it was meant to be read. This is true of Masters' books because it does contain some extraordinarily interesting remarks about Rousseau's writings.

What can we ask of a work written in this form? Unlike a commentary on one work, it does not attempt exclusively to find out the author's question and his answer. Rather, Masters examines a number of Rousseau's books in order to find out about his political philosophy. Because of Rousseau's pervasive concern with politics, it is clear that his whole teaching can not be found in any one book. One must accumulate an understanding from several of his works in order to have a full grasp of his politics. To come to such an understanding, Masters must examine a number of Rousseau's books in some order. The sufficiency of that understanding depends upon a wise choice of books, and the adequacy of the exposition depends upon the order in which they are presented. In examining Masters' work about Rousseau's political philosophy, one needs to question both the choice and the arrangement of the books to be analyzed as well as the understanding of particular books.

In respect to the arrangement of books to be considered, Masters' book is also different from most books about Rousseau's political philosophy. It begins with the *Emile,* proceeds to the works commonly understood to be the theoretical ground and theoretical exposition of Rousseau's political thought. It does not approach those such as the *Considerations on the government of Poland* and the *Letters Written from the Mountain* in which Rousseau is commonly thought to have expressed his practical recommendations for governments. Thus, Masters could be understood to go from an introduction, to the theoretical ground of the political works in the *Discourse on the Origins of Inequality* and the *Discourse on the Arts and Sciences,* to the *Social Contract* itself. At this point he should be in a position to understand the *Social Contract* thoroughly, perhaps without reference back to the more practical works, particularly considering his discussion of the comments

on the Roman government in the *Social Contract*.

Reading the *Emile* as an introduction to Rousseau's thought needs no defense. It was certainly one of Rousseau's most popular works, and it contains many comments on government. In addition, as Masters notices, Rousseau said that it was one of his major works and recommended it in the *Dialogues* as an introduction to his thought. The question then arises of what kind of an introduction the *Emile* could be. Masters says a number of times that the *Emile* is like a novel. But a novelistic work that could introduce a political philosophy is not simply book with characters that is easier to read than a treatise. In order to read such a work, it is necessary to be aware of the places where philosophy could appear, from which one could move from a novelistic impression to philosophic questions. A novel, at least at the time of Rousseau, had characters who acted according to the principles of human action understood by the author and whose fates were determined by the author's understanding of the way things happen. In the *Emile,* this is made evident by the presence of the tutor, who shapes the environment in which Emile grows and against which he is tested. Masters, however, treats the *Emile* as if it were a treatise, rather than a novelistic work grounded in philosophy. For example, although he remarks that the "confession of the Savoyard Vicar" was an "exoteric" discourse in the midst of this novelistic work, he treats its religious and metaphysical conclusions as if they were Rousseau's most profound. The consequence is that he turns up with a "detachable metaphysics," not required for Rousseau's other arguments and positions. Thus, although the *Emile* could be a good introduction to Rousseau's philosophy, Masters does not treat it as an introduction.

This same attitude toward the form of books is illustrated by his treatment of the *Discourse on the Origins of Inequality*. Although he understands this work to be a history of man, he does not inquire into the kind of history. He seems to think that men progressed evenly from the first page to the last, history being movement, as if there were no philosophic reason for its division into two parts. But one of the most striking things about the division of the *Discourse* into two parts is that a number of topics are covered in both parts. For example, there are two discussions of the origins of language, one demonstrating that it is impossible to develop language in the state of nature and the other how it could have developed. Masters' failure to observe this leads to his conclusion that the family could be understood to exist in the state of nature. One can make the mistake of reading back into the arguments for the most isolated state of nature, those considerations required to understand man's emergence from it. There clearly must be a relation between the two arguments, but it can only be understood after their differences are identified.

Although Masters' book does have these problems, it has the virtue, in contrast to much of the literature about Rousseau, of bringing our attention back to Rousseau's works and to the problem of what they say about politics.—ANNE M. COHLER, *Harvard University*.

Mass Behavior in Battle and Captivity: The Communist Soldier in the Korean War. ED. BY SAMUEL M. MEYERS AND ALBERT D. BIDERMAN (Chicago: The University of Chicago Press, 1968, Pp. 377. $11.00.)

This book may be of more than passing interest to students of military history but defects of method, presentation and insulation from major relevant literature severely limit its utility for the broader audience to which it is addressed. The book consists of five studies drawn from interviews with 118 North Korean and Chinese prisoners of war conducted, under the direction of the late William C. Bradbury, during the latter part of the Korean War. The stated purposes of publishing this material are: "to make available a body of data and . . . interpretations both to those interested in the China field and to those concerned with political warfare . . ." and to contribute "specifically to the understanding of Communist authority patterns."

In fact, however: 1) relatively little of the open-ended interview material is presented so that the reader will find it difficult to judge the appropriateness of the interpretations; 2) the interpretations themselves indicate only that a given variable is relevant but less of how it functions and almost nothing about its relative importance; 3) the interpretations are not explicitly related to more recent related research on the consequences of captivity (indeed, virtually none of the items in the bibliography post-date the middle 1950's); and 4) the methods used to secure the data make suspect the applicability of the interpretations, even to the population of Communist prisoners of the Korean War, let alone to Chinese or Communist socio-political authority patterns.

Forty-five interviews were conducted with Chinese prisoners of war. These interviews were conducted in an atmosphere of urgency because it was known that the respondents would "momentarily be moved," or a sample chosen as "knowledgeable" by the U.N. command, the judgment of U.S. compound officers, interpreters and even prisoners. The stimulus situation varied (not only by locale within the prison compound but by relative

freedom—two interviews were conducted in a Prison restaurant, three in the U.S. a year later), the topics covered in the interview varied; the order of interview varied; recordation of response was unsystematized. No member of the research team spoke Korean or Chinese, so that all interviews were conducted through interpreters who were U.S. Army employees who had worked with the prisoners.

The editors are quite candid about these seeming shortcomings in design, but adopt Bradbury's assertion that knowledge about these phenomena is so meager that the studies are nonetheless valuable. Perhaps they will prove heuristic. Bradbury himself, however, argued that one could generalize from this data to Chinese Communist practices and their consequences. He writes: "At this stage in investigation . . . (we) are in the position of the boy who wants to find out whether the white crystals in the bowl are sugar, salt, or something else—tasting a pinch or two of crystals from the top will tell him what he wants to know." But this is true, of course, only if we assume that all the crystals are the same—that the sample of one or two crystals from the top does reflect the composition of the bowl. By the sampling procedures used here, we would expect to find systematic differences between these respondents and even the population of POW's and the data presented in Appendix A corroborates this.

If the problems in research design were necessitated by the pressures of the research situation, they might be at least somewhat alleviated by imaginative analyses of the data. Unfortunately, the coding procedures and the principles of evidence used by the authors in reaching their conclusions are not clearly presented. The analysis seems to have been largely intuitive. The editors suggest that this historical-descriptive mode of presentation was necessitated by the critical review of the government research sponsors. Perhaps, but maintaining this mode could not have been a precondition for permission to publish. It would have been considerably more useful if these pieces had been revised to more adequately present both their analytic procedures and the relationship of their findings to more recent research on prisoners of war, prison inmates, and others in captivity situations.

Although the book as a whole suffers from these defects, readers will find some idea or fact of interest in each of the individual chapters. Bradbury and Jeanne J. Kirkpatrick's "Determinants of Loyalty and Disaffection in Chinese Communist Soldiers . . ." presents a most balanced assessment of the utilities and disutilities of Communist control techniques and includes a fascinating 20-page series first-person vignettes of careers under initial post-revolutionary Communist control. Samuel M. Meyers' chapter, "The Role of Traditional Orientations Toward Social Relations in Chinese Responses to Communist Military-Political Control" not only traces the concern indicated in the title but indicates the centrality of physical coercion behind the effectiveness of Communist political-military control. Kirkpatrick and Uliassi's chapter, "Adjustment . . . to the Communist Demand for Ideological Participation" indicates how enforced verbalization of ideology functions as counter-attitudinal dissonance to achieve new loyalties for the Communist regime and shows how the Chinese Communists, in post-revolutionary consolidation, used the concomitant decrease in "real" communications to prevent institutionalization of anti-regime alienation. Finally, Meyers' and Bradbury's "The Political Behavior of Korean and Chinese Prisoners of War in the Korean Conflict: A Historical Analysis" is an interesting topical and chronological presentation of the surprising behavior and behavior shifts among the Communist POW's.—DAVID C. SCHWARTZ, *University of Pennsylvania.*

The Human Organization: Its Management and Value. By RENSIS LIKERT. (New York: McGraw-Hill, 1967. Pp. 258. $7.95.)

In this book Professor Likert describes more fully and supports by additional research the "System 4" concept of management originally outlined in his previous book, *New Patterns of Management* (1961).

Most academicians would place this volume in the human relations field of organizational studies, although its author specifically denies this categorization. The system of management proposed focuses upon the accomplishment of organizational goals by satisfying employees psychological and social needs. This focus, perhaps with the emphasis upon the individual need satisfaction, has been the primary concern of human relations theorists for some time.

Most books concerned with human relations in management have a missionary zeal about them. Professor Likert's book has this enthusiasm. It also offers some interesting research data to support his "scientific-based system of management," labelled System 4.

System 4 is composed of a number of different "principles" which should be followed by managers in administering an organization. One principle, concerned with motivational forces within the organization, calls for more effective use of non-economic motives to create cooperative attitudes and behavior among organizational members. Another principle recommends group decision making at all levels of the organization. A third principle concludes that group methods of supervision are best. Other System 4 principles are dedicated to insur-

ing that each experience of each individual will, to the extent possible, build and maintain his sense of personal worth and importance. The book contains other "principles."

Proof that System 4 is the best method of managing an organization is provided by empirical data gathered from members of many different firms. One study quoted at some length involved a business firm with approximately 100 branch sales offices. The researchers requested salesmen from the best 20 sales offices and from 20 other sales offices chosen at random to complete a questionnaire regarding their branch sales manager. The questionnaire was designed to ascertain the extent to which each branch sales manager incorporated important System 4 principles in his system of management. The results showed that the managers of the best offices were, for the most part, applying System 4 principles to a greater extent than the managers of the other twenty units.

Data from other studies is similarly examined.

Another concept presented by Professor Likert involves the division of organizational variables into three groups—causal, intervening, and end-result. Causal variables are those which determine the course of developments within an organization and which can be changed by the organization's management. These include organizational policies and behavior. System 4 principles such as group decision making and supportive relationships are causal variables.

Intervening variables reflect the internal state of the organization, e.g., the attitudes, motivations, and perceptions of its members. The condition of the intervening variables are the result, largely, of the causal variables.

The end-result variables are the achievements of the organization, e.g., productivity, costs, and earnings. These variables are influenced by the intervening variables.

Professor Likert points out that the usual method of calculating a firm's assets is in measuring the end-result variables. Since these end-results are dependent upon the other organizational variables, he recommends that causal and intervening variables be measured. This is especially important because fostering causal variables recommended by System 4 may not make an impact upon end-result variables for 2 or 3 years. Further, a manager can rapidly liquidate important causal and intervening variables—employee loyalty, quality standards, customer loyalty—and show a temporary increase in end-result variables, e.g., profit. Thus, unless causal and intervening variables are measured regularly, one may be over-rewarding managers who and supporting policies which are destroying important long-term organizational assets.

The book mentions ways causal and intervening variables can be measured, but cautions that only experts can take and evaluate these measurements.

While it claims to be applicable to both business and goverment, this publication is written with only an incidental eye for the governmental entity. The management techniques might be applicable to a broad range of organizations in our culture, but the causal/intervening/end-result variables concept is of limited value to organizations with significant problems in end-result definition and measurement.

The author suggests that his System 4 principles constitute a theory of organizations. However, his area of concern in the organization misses important elements which should be included in any organization theory. For example, the tasks which his organization members are performing are somehow given in advance. In the usual organization, decisions must be made as to what the employees of the organization will do. This important decision-making function is outside the purview of System 4 principles. Many of the tasks Philip Selznick assigns his "institutional leader," such as assessing and coping with the external demands upon the organization, are not touched upon in this book. Rather than being an analytically descriptive theory of organizations, System 4 is a prescriptive set of recommendations or principles for managers. As such, the book has value.—JAMES F. SULLIVAN, *University of California, Riverside.*

AMERICAN GOVERNMENT AND POLITICS

The Goldwater Coalition: Republican Strategies in 1964. By John H. Kessel. Indianapolis: Bobbs-Merrill Company, 1968. Pp. 371. $8.50, $4.75, paper.)

Far more than is usually the case, this is a book written on a numbler of levels—at least three, according to my reading. It is an account of Presidential politics in 1964; it is an analysis of the campaign strategies of the Johnson and Goldwater camps and an appraisal of their effectiveness; and it is an attempt to cast the events and reactions of 1964 into a broader theoretical approach to parties and politics. This review will move through these levels and in that order.

It is a brave man who undertakes an account of one year's Presidential politics now that the Theodore White has twice brought his practiced hand, considerable resources, and intensive efforts to the

same task. But while Kessel may not write with all the authority of that experienced journalist (who among us does?), the account is smoothly told. The chapters dealing with the pre-convention and convention politics add comparatively little to what we know already. Once the narrative reaches the campaign, the increment over earlier works is more considerable, a result at least in part, one would suspect, of Kessel's position with the Republican national committee in 1964. The description and analyses of the Goldwater campaign will especially be of interest to students of campaign politics, especially the material on the attempts to establish state-wide Republican vote quotas.

But these materials, even though they absorb at least half of the book, are in the nature of background or preliminaries; the book should certainly not be judged in their terms. As its title suggests, the book focuses on the "second level," the study of the Republican strategies of 1964. After lengthy explications of the two Presidential strategies, Kessel examines the impact of the strategies in a long chapter (chapter nine) which is, I think, the most useful and stimulating segment of the book. He draws almost exclusively but effectively on the 1964 data of the Survey Research Center to gauge the impact and effectiveness of the strategies. (I use the plural repeatedly, because Kessel does also evaluate the Johnson strategies, the title notwithstanding.) It enables him to assess the parties' successes with groups they had marked as "target populations," the impact of the campaign over time, the effectiveness of various themes and issues, and the effect of canvassing and other organizational contacts. Even where the conclusions merely buttress what less thorough reports and observations (including our own) have earlier suggested, the information is more refined and precise here. Occasionally, too, it surprises—as in the strong case which Kessel builds that Republican canvassing did indeed apparently increase the Goldwater vote.

Valuable and stimulating as this analysis is, I do have several concerns about it. There is first of all the danger that the "strategies" and "coalitions" we have here and in similar analyses (I think also of the application of formal models) are at least in part the result of the author's own insight and intellectual organization. Do candidates and campaign organizations actually map out such detailed strategies and follow their consequent means-end corrolaries in the heat of the campaign? The day-to-day responses to events, people, resources and contingencies may indeed appear, after the fact, to have been explicit strategy, but it is "strategy" only in a very general, almost truistic sense. My other reservation (and a difficulty which the author recognizes) is over the difficulties in assessing the impact of a campaign with general sample survey data. One can note changes in the electorate over the course of the campaign (when the sample has been interviewed more than once), and one can note distributions of perceptions, attitudes, and preferences in the electorate. But to infer that this or that activity of one candidate or his party caused them is another matter. I am therefore hesitant, without more detailed information about the respondents' perceptions of and exposures to the campaign and non-campaign events and influences, to conclude, *inter alia*, that the 'Goldwater offensive of early to mid-October was having some impact until dramatic foreign events diverted the attention of the electorate" (p. 297).

Finally, on the "third level" this study of strategies in '64 is encased in beginning and concluding chapters which propound a more general theory of parties and politics. The theory—which strikes me as more of a model or paradigm—is built on interlocking concepts of electorate, group, coalition, and institution. "The electorate is a set of persons, potential voters. A group is also a set of persons, a relatively small set of persons who are members of the electorate. A coalition is a set of groups, and an institution is a set of coalitions." (page 323). The concluding chapter is involved with an extended statement of the linkages between attitudes and behavior in each of the four and with linkages between and among them. It serves Kessel as a framework for viewing the events of 1964, although it seemed to me not ot be an essential part of the strategic analysis. Certainly he does not use it extensively as a basis for empirical hypothesis or generalization.

Just how useful the reader will find the electorate-group-coalition-institution framework (even for heuristic purposes) may to a considerable extent depend on his preferred concept of the political party. The bias of the Kessel model is strongly toward a view of the parties which is nonorganizational, and which focuses on electoral coalitions, electoral politics, and voter behavior. It may be that the Kessel approach is partly a consequence of the focus of his present researches and his need to rely heavily on survey data on voting behavior. In any event, my own preferences are for a much more organizational view of the parties, one which recognizes the discontinuity between party and electorate and which does not try to subsume the full range of activity in the electorate into a concept or theory of the political parties.

In brief, this book will be of interest to political scientists chiefly for its additions to the meager systematic literature on political campaigns. Its care and its imaginative use of data, especially the sample survey data, suggests a number of useful ways to integrate the study of political campaigns into the broader literature on American politics.

—FRANK J. SORAUF, *University of Minnesota.*

Democratic Representation: Reapportionment in Law and Politics. By Robert G. Dixon, Jr. (New York: Oxford University Press, 1968. Pp. 654, $8.00).

The Reapportionment Revolution triggered by the Baker v. Carr decision of the U.S. Supreme Court in 1962 has occupied the attention of lawyers, politicians, judges, and scholars since its inception. In the future, it inevitably will receive comparable attention. But it also has raised the left unanswered complex and fundamental questions of representation.

Professor Dixon's book is a rich source of information and ideas for those who seek an understanding of the entire reapportionment issue. It includes the background to the major U.S. Supreme Court decisions in the area, set against historic and continuing problems of representation. It traces the problems of implementing the one man-one vote apportionment standard by courts deeply immersed in Frankfurter's political thicket. It is difficult to think of what Professor Dixon has omitted from this impressive volume, for it covers exhaustively just about every significant issue raised by the Revolution. For example, he discusses the bases for judicial intervention, the problems of establishing judicial standards, and the difficulties of courts applying equal representation standards, both substantively and procedurally, to legislative bodies. He also examines the use of non-legislative apportionment agencies, policy outcome studies, and computer apportionment techniques. He briefly summarizes, in the appendix, legislative apportionment in every state with relevant statistical data for each. In short, the book is complete and thoroughly researched, yet written in a pungent, concise style with a light touch where appropriate.

This is a critical book. There is little that escapes Professor Dixon's criticism. Combining his talents as political scientist and law professor, he draws upon a large volume of research materials which include not only the standard reapportionment works and the contributions of political science to various aspects of reapportionment, but the wealth of legal materials that have accumulated in a comparatively short period of time. Professor Dixon has utilized the law reviews of course, and the opinions of federal and state courts at all levels. But, he also has used to a considerable extent the briefs submitted to various courts by all parties—plaintiffs, defendants, and *amici curiae*. With these tools, and his own research efforts, he analyzes the arguments and actions of almost everyone involved in reapportionment. Few escape his censure—the Supreme Court, other courts, the lawyers' briefs, oral arguments in the courts, political theorists, political scientists, legislatures, non-legislative reapportionment agencies, constitution writers, reformers—all come in for criticism at some point, and, in the logic of Dixon's position, most deserve it. One doesn't agree with Dixon in all respects; but his critiques are well developed and throught-provoking.

If his general argument can be extracted from a volume of immense scope, Professor Dixon agrees that population must be the preeminent standard for representation but feels strongly that key questions of representation have not been settled, nor even raised, by the helter-skelter pattern of compliance with judicial one man-one vote standards. He argues that variant simple standards of mathematical equality hardly confront genuine issues of representation; indeed they permit them to be neatly sidestepped. In his analysis, he raises very real questions of what actually constitutes adequate or effective representation for contemporary democratic society.

Professor Dixon does recognize that apportionment involves essentially political decisions, even within the guidelines of equally populated districts. He stresses that functional inequality in representation may continue to exist, even under a one man-one vote standard, occasioned by the possible impact of varying districting schemes upon particular political situations. He notes the relevance to the measurement of representativeness of gerrymandering, single- and multi-member districting patterns, the geographic concentration of political party strength, and the use of local political boundary lines in districting, among other factors. He argues effectively that reapportionment efforts must accommodate political realities and operate within state constitutional and statutory frameworks which maximize political compromise. To anyone familiar with the endless possibilities of drawing legislative districts or devising apportionment formulae, this is an eminently reasonable position. Professor Dixon properly recognizes that neutral reapportionment is unattainable.

Yet, while he concedes the political nature of reapportionment, Professor Dixon is troubled seriously by judicial treatment of reapportionment cases. He is unhappy with the conduct of the arguments, poorly-prepared briefs, unanswered sallies by the adversaries, the logic of the opinions, the process for remedies fashioned by the courts, and especially the enunciation of simplistic arithmetical standards for equality. He argues that the virtues of swift compliance have been offset by the chaos of disparate judicial and legislative responses to mandates to reapportion.

But the questions of representation which concern him are not easily resolved. Beyond fundamental restructuring of American political sys-

tems, and of American political values, it can be suggested that these issues are not near settlement. They raise basic political questions that American society assiduously has avoided to date. Also, they raise questions upon which there is still very much disagreement. Considering the extent of disagreement and prevailing American values, however mythical in application, it is at least understandable that courts would avoid basic representation issues and confine themselves to simplistic artithmetical considerations, with room to apply them as standards as flexibly as state and local political systems are diverse.

There is much in Dixon's book to argue with because he raises so many appropriate questions. Indeed, this is its merit. His unhappiness with the remaining reapportionment issues is everyone's who has had to confront them. But alas Professor Dixon doesn't resolve them either.—RALPH EISENBERG, *University of Virginia.*

Electioneering in a Democracy: Campaigns for Congress. By DAVID A. LEUTHOLD. (New York: John Wiley & Sons, 1968. Pp. 150. $6.95.)

Congressional campaigns are rather like perennial and abused step-children: largely ignored, under-financed, easily forgotten. Excluding occasional studies of individual efforts, the accumulation of systematic knowledge about political campaigning has, for obvious reasons, been mainly derived from focusing on the presidential struggle. Yet it is not obvious that the characteristic way resources—organization, money, workers, and issues—are acquired and utilized in presidential campaigns is transferable in miniature to congressional contests. Quite the contrary. Following the national conventions, both (perhaps I should say the three or four) presidential aspirants are more often concerned with properly utilizing resources, whereas for all but a handful of congressional candidates, resource acquisition still constitutes an often discouragingly large part of the post-primary campaign. So argues David Leuthold in this compact, empirically-based, cautiously-developed comparative case study of ten Northern California congressional races in the 1962 mid-term elections.

Utilizing the SRC's voter typology to suggest four of the six major resources candidates must acquire, Leuthold is primarily concerned with describing this process as it differentially affects the candidate's vote-getting power. Two of the book's ten chapters focus briefly on the use of these resources, e.g., the kind of appeals undertaken, but the emphasis is on acquisition: What are the personal characteristics that distinguish winners from losers? Incumbents from nonincumbents? How is the support of prominent party leaders gained and held? What are the major information sources candidates use to inform themselves about voters? About issues? How is the support of non-party groups obtained? What are the major sources of campaign funds? How are campaign workers recruited and motivated? How do they perceive the candidate? The campaign?

By examining a relatively small number of candidates (20) in districts clustering around the San Francisco Bay region, the author was able to employ a variety of research techniques and collect data from a variety of sources (personal interviews with the candidates and key campaign personnel, observation of candidates before meetings and at party gatherings, responses by campaign workers to a two-page questionnaire, financial reports submitted to the California Secretary of State, etc.) Leuthold, however, omits any serious discussion of the reliability of these multiple information sources and it is thus not clear, for example, how much emphasis was placed on the candidate interviews, or even what questions were asked, or what criteria were used in selecting and observing (or not observing) "campaign events." Since it was neither feasible nor essential that he cover all the races in equal detail (the book is a revised dissertation and most of his illustrative materials come from two of the ten campaigns), a brief discussion of these factors would have been helpful.

The most consistent finding, hardly surprising but patiently if not laboriously documented, it that inequalities in the distribution of campaign resources are not dispersed; not even modestly so. Incumbents of both parties are enormously advantaged by the cumulative nature of campaign resources, whether it involves comparatively incidental factors like family support, or more substantial matters like financing, gaining group support, controlling and manipulating information about issues, or developing a stable and effective campaign organization. One pathetic challenger greeted a mid-summer volunteer by saying, "Congratulations, you're the first to volunteer so you're the campaign manager." The only campaigns in which this pattern is even vaguely disturbed were in the three districts which could be classified as competitive, one of which had no incumbent running. Nevertheless, even in competitive districts the distribution is greatly skewed toward incumbents.

But the assumption cherished by most professional politicos, that by simply equalizing some resources, e.g., money, a campaign organization can convert these resources into votes, does not follow. Although one hazards to use 1968 as an example, the three million dollars reportedly raised and distributed by the Republican Congressional Campaign Committee to 81 "target" districts was not conspicuous for its success and these were districts most likely to be vulnerable. Moreover, as Leuthold notes in passing and as Nelson Polsby

has forcefully argued, there are some demonstrable advantages in the historically decreasing turnover of personnel in the House (for both liberals and conservatives), advantages which would presumably disappear if that stability were threatened by increasing competition. Since Leuthold adopts Schumpeter's criteria of "democracy"—with its excessive emphasis on the mechanics of a competitive struggle over votes—he does not seriously consider what alternative functions are performed by the structure of defeat and the resulting existence of a guaranteed opposition in the House no matter how strongly electoral tides are moving in one or the other direction.

The author does show, on a variety of dimensions, that citizen participation increases in competitive districts, and ends his study by suggesting a number of reforms which would partially, if not increase electoral competition, help to disperse resources more equitably than is not the case, a necessary but not sufficient condition for meaningful competitiveness. Most have been suggested before (direct subsidies to challengers and their staffs, permitting candidates to compete simultaneously for two offices), others are relatively new and would require a revolution in the present habits of state legislators and federal judges (substituting competitiveness for compactness as a criteria for redistricting). Leuthold recognizes the dismal prospect for most of these proposals when with admirable and, one hopes, intentional irony he comments, "Some of these suggestions, especially the suggestion that congressmen vote campaign salaries to their challengers, are not likely to be adopted immediately . . ."

The chief strength of this study, however, resides in the careful manner in which descriptive materials are marshalled around the central question of resource acquisition, independent of whatever policy recommendations might be drawn from the findings. Regrettably, the small number of campaigns, while more adequate than traditional intensive case studies, does not really permit any solidly based generalizations. The book is loaded with tendency statements ("Democrats who worked in primary elections were found to be more liberal than Democrats who worked only in the general election," (p. 37), or ". . . experienced candidates were more likely to emphasize the Burkean notion of independent trusteeship, while inexperienced candidates thought of a congressman's role as that of an instructed delegate," (p. 115) which would have profited by noting exactly how many—3 of 10? 5 of 10? 8 of 10? Some other minor problems: The author's disciplined caution occasionally lapses into dissertation-like timidity—"In the Bay area, mail appeared to be more valuable for congressmen than for nonincumbents as a source of information about voter attitudes," or, "Most people would consider democracy weakened if the House of Representatives were to be appointed by the President, as is the Supreme Court." But these are quibbles. The book constitutes a welcome addition to what one hopes will be a research renaissance on congressional campaigns, elections and recruitment.—JEFF FISHEL, *American University.*

The Shrine of Party: Congressional Voting Behavior, 1841–1852. By JOEL H. SILBEY (Pittsburgh: University of Pittsburgh Press, 1967. Pp. 292. $6.00.)

This study of voting in the House and Senate from 1841 to 1852 is of interest to political scientists on at least two grounds. Substantively, Silbey reveals a good deal about the relative strength of party and section during the 1840s. His analysis adds strong support to the thesis advanced by Richard P. McCormick in *The Second American Party System* that by 1840 two national parties had developed as highly organized institutions competing on substantially equal terms throughout all the major regions of the country. McCormick's study is a description of party formation in the various states; it ends in 1840. Silbey's study begins in 1841 and is an analysis of rolls calls in both houses of Congress.

Contrary to the views of some historians, Silbey finds that in the 1840s voting in Congress was marked by a rather high degree of cohesion within both the Whig Party and the Democratic Party, marked differences in the voting patterns of the two parties, and low cohesion among representatives from the major sections of the country. If Congress was polarized, in the 1840s the polarization was of parties, not of regions.

The situation was, of course, more complex than this. Voting along party lines that cut across sectional boundaries was more characteristic of the early than the late '40s. The Whigs regularly supported proposals for a national bank, tariff protection, and appropriations for rivers, harbors, canals and other "internal improvements." The Democrats regularly opposed these objectives. The Democrats were expansionist and agressive in foreign affairs; they sought new lands into which free farmers and slave owners could move as they exhausted the old. The Whigs were more cautious in foreign affairs and more likely to oppose expansionist schemes.

The voting patterns of the two parties seemed to reflect two conflicting and yet internally coherent ideological perspectives, each designed to appeal, though in different ways, to the major economic, social, and regional interests of the country. To the Whigs, economic and social progress required the mutual development of agriculture, commerce, finance, and industry under the benign

regulation of the federal government: the Whigs were Hamiltonians. The Democrats, more Jeffersonian, sought to reduce the rule of government to the barest minimum because laissez faire was the best guarantee of freedom and prosperity, so they thought, in a nation of farmers, particularly since the income of farmers in the South and West depended on international markets.

Even if hindsight tempts one to over-interpret the strains of sectionalism (Silbey suggests that many historians have wrongly read the controversies of the late '50s into the '40s) there were nonetheless serious stresses within the parties. Western farmers needed appropriations for improvements in transportation in order to market their produce; Southerners did not, and feared that federal appropriations for roads and harbors would have to be paid for by higher tariffs. Thus on this issue, the South and the West had irreconcilable goals. John C. Calhoun never quite succeeded in getting Southern Democrats to support the West on internal improvements as a price for support by Western Democrats on issues involving slavery.

Slavery, of course, was the major sectional issue that threatened the unity of both parties. Whenever the issue of slavery expansion came up—and in the second half of the decade it could no longer be avoided—the party coalitions tended to fall apart and on this question voting assumed a markedly regional pattern. Possibly because of this, by the end of the decade the parties ceased to have the internal cohesion they previously had shown on other issues.

However, as Silbey shows, the decline of party cohesion did not mean that Congress was polarized into sectional blocs. Instead, "Congressional voting had broken down, more than ever before, into a multiplicity of factional groups, behavioral factors, and individual decision-makers, with only occasional alignments of these patterns into large-scale partisan or sectional groupings." An unpublished study of House roll calls in the 1850s by Donald Forbes and myself indicates that this unpolarized pattern of multiple, cross-cutting cleavages persisted throughout most of the '50s, except for a brief polarization into sectional blocs, during the dispute over Kansas in the 34th Congress (1855–6). Finally, to be sure, the long heralded, fearfully sudden, and virtually complete polarization into two regional blocs dominated the Congress in 1859-60 and terminated in civil war.

Silbey's study is interesting not only for its historical findings but also because of its methodology. If Lee Benson, Samuel P. Hays, Allan Bogue and a few other historians were the first generation of pioneers in the use of modern quantitative methods in political history, Joel Silbey can be said to be of the second generation. Silbey is probably the first historian to analyse a decade of Congressional roll calls in the nineteenth century by means of scaling techniques. The results seem to me fruitful, and we may expect that Silbey's generation of historians (aided perhaps by an occasional political scientist) will ultimately provide us with similar analyses of voting in Congress for its entire history.

In the application of quantitative methods to the study of history, that discipline is about where political science was less than two decades ago. Social and political historians trained in quantitative methods are few in number, predominantly young, and confronted by colleagues whose mood seems to be shifting from hostility to resignation, healthy skepticism, and even encouragement. Although large parts of the study of history may never lend themselves to quantitative methods, historians who use such methods skillfully are bound to have a significant effect on the discipline during the next several decades.—ROBERT A. DAHL, *Yale University.*

ESEA: The Office of Education Administers a Law. BY STEPHEN K. BAILEY AND EDITH K. MOSHER. (Syracuse: The Syracuse University Press, 1968. Pp. 393. $10.00.)

"This is a study in public administration." These, the first words of Professors Bailey and Mosher in this exhaustive study of the Office of Education (USOE) and the Elementary and Secondary Education Act of 1965 (ESEA), are wholly accurate but unduly modest. The book can be more accurately categorized as a study of public administration as an integral part of the political process. Such an approach is certainly not new, for we have been graced with the expanding literature of the Inter-University Case Program for nearly two decades (Professor Bailey having recently published a short ICP study on ESEA) and several general and theoretically directed works by Hyneman and Woll along with a few recently published anthologies with bureaucratic politics as their main focus. However, the Bailey and Mosher work is distinctive due to its analysis in depth of one situation involving a single administrative agency, its balance of focus on ESEA's pre-natal and post-natal history, and its careful mixture of descriptive-causal analysis with normative-prescriptive material. The fact that no study of such dimensions has yet appeared can be explained, in part, by the fact that only in recent years has the study of administration and politics developed the requisite maturity and depth of interest in the systematic measurement of revolutionary legislation on an administrative agency to make such investigations worthwhile.

Since it is likely that most students of public policy accept federal involvement in local education matters as a fact of life, most were not quick

to recognize the adjustment crisis which ESEA caused for both USOE and for state and local school administrators. In consciously linking a sound educational system to the eradication of poverty, the law directed that most funds for elementary and secondary education be funnelled to school districts with high percentages of culturally and economically deprived children. Other sections provided funds for books and related instructional materials, for new experimental education centers, and for university and individual research projects.

Such challenges fostered much anxiety, frustration, and conflict within USOE and between USOE and external organizations affected by the law. The status quo is, due to both apathy and a natural fear of the unknown, preferred by many to the uncertainties and insecurity which could result from such sweeping changes in an agency's mission. The pre-ESEA period found USOE in an "old-line" status, characterized by hyper-specialization, a disjointed and irrational organizational structure, estrangement from its parent department (HEW), and gun-shyness due to constant bombardment by opponents of federal aid to education. ESEA necessitated more organizational integration, new blood at all levels and ranks, and a modernization of agency financial and management procedures. External conflict was met with a very intricate "give and take" between USOE and affected school officials, church spokesmen, and concerned interest groups, with USOE personnel demonstrating deplomacy and flexibility in adjusting programs to the administrative-political facts of life. Still, opposition from state and local school administrators was quite troublesome. USOE's insistence upon school desegregation as a prerequisite for program participation, its overly bureaucratic approach, and the timing for releasing funds all caused constant intergovernmental strain. While USOE officials expressed satisfaction over the first year's administrative record (to mid-June, 1966), the authors note that such self-defined success cannot be accurately assessed as yet.

While the book is strong on descriptive and causal analysis of ESEA's origin and early administration, it is equally strong on the analysis of both the adequacies and shortcomings of federal aid to education as it has evolved within the American political system. In reviewing a century of federal concern for education the authors conclude that, while today we are more able to appreciate the philosophical, political, religious, economic, and administrative problems associated with federal aid, we have failed to deal adequately with the important questions of proper public-parochial relations (basically because of the constitutional "limbo of uncertainty"), school desegregation, interagency administrative coordination, and with the relatively new challenge of tailoring educational policy to the needs of the poor. Striking a more positive note, they speak of how the case illustrates that an administrative agency can champion retionalist policies while remaining politically responsible. They cite how both advocates and critics of federal aid can take comfort from the ESEA story because the "administrative apparatus of a separation of powers, Federal, and politically diffuse polity, is too cumbersome for tyranny; too interdependent for anarchy." They note that "the bargaining processes of public administration" in the long run conform to prevailing social norms. This, they say, "makes reformers restive; but it also disturbs the satisfied from their callous slumbers." In graphically illustrating the "kaleidoscope of policy-making," ESEA was directed at mediating between opponents and proponents of federal aid "without unduly blunting the edge of true innovation . . . The real tailor was the bargaining processes of a domocratic polity shaped here and there by the unusual skills of a few cutters and seamstresses sensitive to emerging fashion."

There is, to be sure, abundant gloss to the book's style, especially when analysis is the major focus. But it is, above all, a very lucidly written work that calls for such expressive terminology to relate the honest assessment of the authors. In short, I find its style, depth, organization, and balance very appealing. Granted, it is a study of the experiences of one agency with an unusual challenge operating in an atypical political environment and, to that extent, it is not representative of the ledger of a typical agency with a routine mission. Yet the abnormalities of the situation help to underscore than amplify the intricacies of the American political system and enable the reader to appreciate the almost infinite complexities of the system, and to acquire an appreciation for the constant tug-of-war between administrative rationalism and political realism.

As a devotee of the case study as a teaching device, I must confess to a methodological bias on my part. I am also compelled to publicize my awareness of the tendency for some case study authors to become captivated by and overly appreciative of the system under study. While acknowledging the possibility that there may be some of this here, my feeling is that it is minimal if it exists at all. The book's major thesis is unquestionably valid and its contents true to its thesis. The unavoidable interdependence of politics and administration is portrayed with all its complexity, intrigue, confusion, and frustration. Students of public administration, legislation, public policy, and politics will undoubtedly argue as to whether the implications of this study give cause for optimism or pessimism. But let no one finish this book and argue against the thesis that the study

of public administration, politics, and political theory are logically separate fields of inquiry. This book will serve many purposes for many political scientists, but above all it illustrates the inevitable interconnection of these areas, and it illustrates it very well.—WILLIAM L. MORROW, *DePauw University.*

Constitutional Politics in the Progressive Era: Child Labor and the Law. BY STEPHEN B. WOOD. (Chicago: The University of Chicago Press, 1968. Pp. 320. $10.00.)

Stephen Wood's *Constitutional Politics in the Progressive Era* is an extended, but incomplete, historical account of two episodes in the movement to regulate child labor by national legislation, the first culminating in *Hammer v. Dagenhart* (1918), barring the use of the commerce power for such purposes, the second ending in *Bailey v. Drexel* (1922) when the Supreme Court blocked a parallel effort based on the tax power. Wood presents a fuller study of some of the interest groups and individuals involved in these episodes than we have yet received. His work is nevertheless incomplete in at least the following respects: For reasons not made clear in the book, the research effort terminates with the Supreme Court's *Bailey* decision holding the child labor tax of 1919 unconstitutional. While reference is made to the *Darby* decision of 1941 upholding the child labor provisions of the Fair Labor Standards Act, no effort is made to trace the legislative and judicial developments which led to this result. In addition, the volume leaves unexplained and unexplored the remarkable ineptitude, mismanagement or opposition to both child labor laws glimpsed in the account of the Justice Department's direction of the child labor cases through the courts. There are sharp studies of the counsel for the opponents of the legislation and of the egregious district court judge before whom both laws began their ill-fated adventures in the federal courts. There is near silence on the roles played by Solicitor General John W. Davis in the Dagenhart case and by Solicitor General James M. Beck in the Bailey case. Surely these apostles of limited government, the latter of whom could speak of the "perversion of federal powers" likely to result if the judiciary refused to inquire into the motives behind Congressional enactments, deserve as much attention as is given to James M. Emery, the brilliant and skillful general counsel of the National Association of Manufacturers, who supplied both direction and content to the first campaign of opposition to federal regulation of child labor.

It may well be that Wood conceives of his work as an intensive examination of one theme in the Progressive movement. If this is so, the work must be taxed for its complete neglect of the historiography of progressivism. Wood uses a simplistic dichotomy between "liberals" and "conservatives" as his fundamental classification of participants in the events he describes, going so far as to write without qualification of Justice Holmes as a "liberal," despite all the critical attention this old, if not hoary, chestnut has been given. He confounds, by unquestioning inattention, the designation of a political era (Progressivism) with a specific goal-orientation in politics (progressivism), thereby lightening his scholarly burden to the point where he can dismiss President Wilson's nomination of James C. McReynolds to the Supreme Court as a "mistake," without inquiring whether this phenomenon of Progressivism, like the presence of John W. Davis as Solicitor General in 1917, does not suggest a disjunction if not indeed an opposition between the Progressive Era and that policy of support for the needs of the depressed and disadvantaged designated as progressivism.

These reservations about the acuity of Wood's scholarly perceptions are unhappily reinforced by one of his more explicit methodological assumptions. He states that in using biographical data as sources of insight into the doctrinal positions of the justices he discusses, he will make inferences "only where what we know about factors influencing selection or explanatory of behavior appear to justify conclusions that transcend speculation" (118). What factors these are which are explanatory of behavior and yet transcend speculation does not appear from the impressionistic and fragmentary sketches Wood presents. The characterization of Justices Holmes, Brandeis, and Clarke as liberals is one result of this unspeculative method: Holmes and Brandeis voted, without separate opinions, in the eight-man majority that struck down the child labor tax; Clarke dissented in the same case, also without a written opinion. In this circumstance the characterization is a model of speculation unsupported by what "we know" of the rules of evidence and analytical reasoning. Wood writes as if he believes his documentary sources are so rich and so unambiguous in meaning that careful evaluation is unnecessary, a belief which would be folly in historians and a complete abdication of scholarly responsibility in political scientists.

One chapter of Wood's book is of a different order and demands special notice. "Judge and Company: Raising the Test Case," is an excellent study of the strategy and tactics of constitutional adjudication. Wood uses his sources—expecially the files of the National Child Labor Committee, and of the Department of Justice—to reconstruct the selection of the test case and the arrangement of an essentially friendly suit to test the constitutionality of the Keating-Owen Act. Effective re-

sort to biographical data results in a detailed portrait of Judge James E. Boyd, the federal judge who presided in the case. The resulting trial and decision is at once a tribute to the skill with which opponents of the legislation sought to determine the outcome by control of "the issue, the judge, the particular circumstances and even the parties," (93) and a searching illumination of the extraordinary powers exercised by individual district court judges before the reforms of 1937 restricted them by requiring three-judge courts to sit in suits seeking injunctionive relief against allegedly unconstitutional Congressional acts. In the best sense of the phrase, this is "writing for the anthologies."

The contrast between this chapter and the remainder of the book is striking. The role of relevant groups, like the women's rights organizations, is minimized or ignored. Case analysis is pedestrian, painfully so when a footnote reference to T. R. Powell reminds us of five pages in which there is a sharper examination of the issues than in an equivalent number of chapters by Wood. Finally, an infelicity of expression is regrettably a fair indicator of the book's content. We read of "the Supreme Court's power of invalidity," (300) and find as a last paragraph the following lines, fully appropriate to support a negative assessment, but out of place as the last words in a scholarly endeavor:

> It is a commonplace of constitutional commentary to assert the controlling place of popularly determined policies in American democracy. However, the child labor history instructs—if nothing else—the vulnerability of this principle when conflict exists, as it almost always does, over "the extent of powers actually granted" Congress and over the purposes that government can legitimately pursue, whether the achievement of social welfare or the effectuation of equality.

[sic].—Leo Weinstein, *Smith College.*

Negroes, Ballots and Judges: National Voting Rights Legislation in the Federal Courts. By Donald S. Strong. (Alabama: University of Alabama Press, 1968. Pp. 100. $5.00.)

If the primary purpose of the Civil Rights Acts of 1957 and 1960 was to enfranchise Negro citizens in the South, the efforts were futile. Donald Strong traces the attempts at implementing these laws through federal court action, and arrives at this conclusion.

The book is excellent as a reference source, both for the previously uncollected data and for the behavioral insights into legal processes in the area of Negro voting in the South. It includes a short summary of the 1957 and 1960 Acts, and that portion of the 1964 Bill relating to voting. Then comes an account of various instances of *official* discrimination which the federal government tried to resolve by suits. Firing of teachers involved in registration attempts, arrest, threats, actual beatings by sheriffs and deputies, and public harassment were among coercive devices employed. Registrars discriminated blantantly in literacy tests, and purges were conducted. Sheriffs refused to allow Negroes to pay their poll taxes. The Justice Department sought, and sometimes obtained, relief through cases on a county-by-county basis. Later, suits were filed against the entire states of Mississippi, Alabama, and Louisiana, claiming that the state laws were discriminatory *per se.*

There are few errors of commission in Strong's work; the author's shortcomings lie in the restrictive approach employed, and in the limited nature of his sources. He relies heavily on *Race Relations Law Reporter,* and largely ignores hearings and reports of the U.S. Commission on Civil Rights, and numerous theses and dissertations on the subject. This causes him to omit reference to the coercion and intimidation which was heavily documented in these sources.

The other major section of the book, by far the best and most useful portion, deals with the problem of enforcement. Sometimes state legislatures, chiefly those of Alabama and Mississippi, tried to circumvent the federal acts by various methods—a sort of gamesmanship.

The other hindrance to enforcement was in the behavior of federal judges who openly professed to believe in segregation, and saw themselves as protectors of the White South. This superior section reveals the enormous role that these judges had in retarding or accelerating Negro enfranchisement. The author places on these men a major portion of the burden for failure of the 1957 and 1960 Acts, and the transfer of the responsibility from the judiciary to the executive branch in the resultant 1965 Voting Rights Act.

In this section, Strong's limited sources cause considerable error. He is hampered, as are all researchers in the field of Federal law, by the highly selective process by which Federal district court cases are included in the *Federal Supplement.* Less than twenty per cent of these opinions handed down are printed here. The rest are mimeographed or typed, and filed with clerks in various offices in each judge's district. Thus accurate quantitative research is extremely difficult.

For example, the author states that "Claude F. Clayton, the judge of the Northern District of Mississippi, appears to have handled only three cases in this area. Judge Clayton has a perfect record of ruling against the (Federal) Government—three to nothing." I was able to locate eleven cases against Mississippi officials; Clayton ruled for the federal government in eight of the eleven cases, and for state officials in only three cases—those listed by the author. Further, Clayton's language in these eight opinions was so strong that registrars were quite certain that harsh penalties

would follow if they persisted in discriminatory practices. (The one registrar who tried was cited for contempt.) Similar errors concerning other judges are highly probable.

The author also ignores the role of the Court of Appeals of the Fifth Circuit in implementing the Civil Rights Acts. With a large part, possibly a majority, of these nine appellate judges being staunch segregationists, Chief Judge Elbert Tuttle had to carefully manipulate assignments to the three-judge courts on crucial cases. When his judgment failed, so did the efforts of the Federal Government.

The epilogue, a four-page comparison of the results of the 1965 Voting Rights Act with the previous efforts, is emphatic, but somewhat lacking. The author's premises that the older Acts were failures, and the 1965 Bill a success, are easily amenable to empirical testing. Negro registration *has* grown, largely as a result of the bill. This should have been documented more fully, and would have been a more credible put-down for those who contended Negroes did not register and vote because "they just don't care."—STEVE WHITAKER, *Temple University.*

The Jurisprudence of John Marshall. BY ROBERT K. FAULKNER. (Princeton: Princeton University Press, 1968. Pp. 307. $10.00.)

The story (apocryphal?) is told that when John Marshall was Chief Justice (1801-1835) he would, on many a judicial occasion, turn to Story (Mr. Justice) and say: "This is the law, now find the precedents for it." A century later, in participating in a celebration of Marshall's accession to the Chief Justiceship, Oliver Wendell Holmes (then on the bench of the Supreme Judicial Court of Massachusetts) noted that ". . . If American law were to be represented by a single figure, sceptic and worshipper alike would agree without dispute that the figure would be one alone, and that one, John Marshall. The former statement obviously implies that if Marshall was indeed a great Chief Justice it was merely because he happened to preside over a Court that was writing large segments of the law of the land. The latter statement just as obviously implies that Marshall's individual contributions to American jurisprudence, as revealed in his opinions, justify the accolade expressed.

The beauty of Robert K. Faulkner's *The Jurisprudence of John Marshall* is that he does more than develop these two possible interpretations. He does even more than merely set his goal toward some kind of reconciliation of Marshall the statesman and Marshall the jurist. What he has done is to create a study in depth "of one comprehensive political, economic, and legal persuasion by which the Constitution is to be interpreted" and through that study to explore and set forth Marshall's jurisprudence. On one level that "jurisprudence revolved about the Constitution of the United States as understood at its framing, varied somewhat with great events and changing conditions in America, and encompassed practical skills in application as well as knowledge of the broad principles of American law." As such it might be said to be an early form of American pragmatism so vitally necessary in building a new nation and hence not a system of philosophic jurisprudence in the sense that "detailed prescriptions for the problems of our times" (today) can be extrapolated. But it is Professor Faulkner's worthy contribution to add to this generally accepted denominator a new value: and that is that Marshall developed "a Lockean interpretation of liberty and law which is separated from neither moral considerations nor a certain kind of political wisdom, and takes into account, moreover, America's peculiar conditions." Thus on a much higher and worthwhile level Marshall seemingly founded a legal philosophy that makes American law, American political thought, and American history an entity in its own right.

To achieve this end, and to achieve it with the distinction that Professor Faulkner gives to it, would seemingly be an insurmountable task. For one, it was necessary to avoid producing just another treatise on American Constitutional Law. For another, the facts about Marshall have already produced such a vast literature that one might instinctively say what more can be added. For still another, Marshall's own writings, aside from his judicial decisions, are either missing, scattered, or dull—and the mere fact, for example, that Professor Faulkner has mined the volumes of Marshall's *Life of Washington* merits high praise in and of itself.

Despite these obstacles Professor Faulkner has produced a most stimulating, thought provoking, and readable book which at one and the same time uses all that is known to add a new dimension about Marshall himself as well as about the American legal system and the society it is meant to serve. With real ingenuity Professor Faulkner has in reality written six essays (four in the text itself and two as appendixes)—each an important study in its own right—bound together by an era and a man. Instead of using the Constitution as text and analyzing Marshall's concept of say the commerce clause or the contract clause or whatever, we have here a bolder and more difficult undertaking: how did Marshall (and his era) conceive of such things as "Liberty, Interest, and Empire"; of Sovereign Government"; of "The Republic"; of "The Court and The Constitution"; and how did he instill them into the marrow of America while performing his task as Chief Justice participating in the task of expounding the Con-

stitution as a legal and political document for a new nation. In the final analysis the values inherent in making these concepts meaningful are the enduring ones. They are the ones by which a people must live and be known. For any text, even a Constitution, can seemingly shift its emphasis as time dictates, as witnessed, for example, by the diminution of the contract clause in the wake of due process and equal protection. And it is through his development of these values in these areas that Marshall created his jurisprudence.

Yet Professor Faulkner is obviously too competent a scholar to present Marshall's jurisprudence in historical perspective alone as based on Locke and "natural rights". He recognizes that Marshall is now conceived to be the "older jurisprudence" and as a leitmotif (Appendix) examines its replacement: ". . . the assertion of social expediency as the standard of judicial decision, the doctrine of judicial deference, the change from the primacy of property rights to that of free speech, the rise of moral skepticism, sociological jurisprudence, legal realism, and many other things now widely taken for granted"—a "Holmesian victory" as he calls it. His analytical study of both Holmes and Marshall transcends a mere plaintiff appeal not to downgrade John Marshall. It is rather a plea for a greater understanding and comprehension that "the most fundamental concerns of the American order" must by founded, if they are to be dealt with in a manner that will endure, on a "moral, political, and legal philosophy," for it is only by a deep rooted philosophy that America's "purposes, its character, even its institutions" can be saved from "aimlessness and degradation."

And no better place can be found for such a reexamination and reappraisal than in Robert Faulkner's *The Jurisprudence of John Marshall.*—BENJAMIN MUNN ZIEGLER, *Amherst College.*

The Politics of Mental Health: Organizing Community Mental Health in Metropolitan Areas. BY ROBERT H. CONNERY *et al.* (New York: Columbia University Press, 1968. Pp. 595. $10.00.)

The Politics of Mental Health is a study of problems concerning the implementation of the Mental Retardation Facilities and Community Mental Health Centers Construction Act of 1963. Supported by a grant from the National Institute of Mental Health, the study developed from discussions in NIMH's Committee on Social and Environmental Variables as Determinants of Mental Health. It reflects a growing interest among mental health specialists in social and community conditions of mental health. (For one student's satirical treatment of community mental health see *Transaction,* July-August, 1968.) The book includes separately authored case studies by local academic observers in Houston, Seattle, New Orleans, Syracuse, Boston, and Minneapolis-St. Paul. Analysis is based on data from published reports, records, and interviews with local observers and participants. Most of the data were collected in 1963-1964.

The Politics of Mental Health is partly a guide to community mental health action and partly an analysis of such action. As a guide to community mental health action, and to the implementation of the 1963 act, the book is an interesting discussion of obstacles and problems, and ways to overcome obstacles and problems. The Houston, Seattle, and Minneapolis-St. Paul studies seemed especially interesting to this reviewer. One of the main obstacles considered in the book is governmental fragmentation; but others are also considered. For example, difficulties in defining "mental health" and "community" are described. Also described are (1) problems stemming from the scattering of mental health functions among federal, state, and local governments (2) the splintering of mental health functions among several agencies at the same governmental level (3) problems of community financing and staffing and (4) the impact of community power structures. As a discussion of obstacles and ways to overcome them, *The Politics of Mental Health* should be a source of helpful ideas for mental health activists in other cities.

As an analysis of community action, however, the book has several shortcomings. The persons interviewed are not specified, and the nature of the data collected in the interviews is often unclear. With their data, and other evidence available to them, the researchers are forced to make logical guesses, plausible assumptions, and "stopgap assessments" about the impact of various factors, combined or separately, on the implementation of the 1963 act. No quantitative analysis is presented, and the book is not put in a broad theoretical context. No common research design is used in the case studies, although similar questions are pursued. None of the many models of community action analysis, or policy analysis—such as those used by Paul A. Miller, Delbert C. Miller, Kaufman, Banfield, Green and Mayo, or Dye—are used, or even mentioned. Mention is made, however, of professional, community, and federal "inputs" and of the impact of community environments on mental health. But variables affecting local mental health action are not systematically classified as political system and environmental variables, mental health system and non-mental health system variables, or community and extra-community variables. The findings are not systematically compared with other findings on community action, urban policy, or governmental fragmentation. Few recent studies by political scientists are discussed or even cited. In the six sum-

mary chapters only seven studies published later than 1964 are cited. None of the seven studies are by political scientists, and none were published later than 1966.

The senior author's preface claims only that *The Politics of Mental Health* should be valuable to mental health administrators and political leaders who must deal with mental health problems in their communities. I agree with this statement of its value. It should sensitize mental health activists to practical obstacles to the achievement of their goals. If it does *The Politics of Mental Health* will make an important contribution to community mental health action. Activists in other fields have often proceeded on the assumption that the only thing needed to achieve their goals was to publish a meritorious, above-politics plan and sit back while the plan sold itself.—BRETT W. HAWKINS, *University of Georgia.*

The Warren Court & Its Critics. By CLIFFORD M. LYTLE. (Tucson: University of Arizona Press, 1968. Pp. 233. $5.00.)

Clifford Lytle's goal is "to bring a degree of analytical order" to the criticisms which have been levied against the Supreme Court and "to project the attacks . . . into a manageable framework so that they may be better understood." (p. xi) His study spans a period beginning with the School Segregation Cases in 1954 and ending with the Miranda Case in 1966. Attention is given to the attacks on the Court engendered by its decisions in the areas of race relations, church-state separation, legislative apportionment, national security and rights of defendants.

Lytle's framework distinguishes two aspects of the attacks on the Court: the sources of criticism and the nature or rationale of the criticism. To begin with the latter, Lytle recognizes four bases for attacks on the Court. These are discordant social philosophy, intra-governmental conflict, decisional obstruction, and the operation of the judicial process. The first three categories are dubbed "result-oriented" criticisms and the last is viewed as "process-oriented" criticism.

One is hard pressed to find in Lytle's discussion the criteria which distinguish clearly the three categories of result-oriented criticism. The category of "discordant social philosophy" is, for instance, defined as embracing "ideological dissent which involves economic, political sociological and psychological differences." (p. 3) But it seems obvious that the "intra-governmental conflict" category involves ideological differences on a political matter, the proper distribution of authority between the branches of the national government and between the national government and the states. Similarly, the distinction between the "intra-governmental conflict" and "decisional obstruction" categories is unclear. The latter focuses "not so much on a positive struggle for power as on elimination of a 'functional obstruction.' This type of attack generally stems from those governmental agencies which have been hindered in the full and free exercise of their functions as a result of certain Supreme Court decisions." (p. 4) It is unclear whether the Court's 14th Amendment defendant's rights cases fall into this category or into the "intra-governmental conflict" category inasmuch as they impose restrictions on the "full and free exercise" of state executive and judicial functions while also pitting federal judicial power against state authority.

Lytle categorizes the sources of criticism as the Congress, interest groups, state government agencies, state and federal law enforcement agencies, and professional groups and their members. In the latter category he includes judges and academicians as well as bar associations. An important omission in his list of critics is the public. There are not available a substantial body of survey data and a number of excellent scholarly writings on the public's attitude toward the Supreme Court. These suggest some discrimination by at least the attentive public between the defendant's rights cases and School Prayer Cases, which tend to be disapproved, and the race relations and reapportionment cases, which tend to have at least majority support. The public's role as a critic of the Supreme Court ought not to be overlooked in a volume of this kind, both because of the implications for legislative efforts to curb the Court and the implications for the question raised by Dahl and other political scientists about the Court's proper role in a democratic society.

Even readers, such as this reviewer, who strongly favor the recent directions in Supreme Court policy-making will be disturbed by Lytle's treatment of the Court's critics. Lytle time and again dismisses criticism of the Court as "irrational" or "politically motivated" or "emotional" or "shallow" or "not judicially presented." In introductory and concluding sections of the book, he suggests that criticism may play an important role in the political process. (pp. 5, 111) But nowhere does he articulate the criteria which distinguish such valuable criticism from that which is to be viewed as unworthy, thus leaving the reader confused about how the author arrived at his judgements.

At some points the author runs afoul of his own rhetoric. He severely rebukes the Southern critics of the Court for "guilt-by-association" tactics because they attempt to link the Court to left-wing groups and viewpoints, particularly in the race relations cases. (pp. 16–17, 95) Elsewhere, however, admittedly reputable critics of the Court are chastized by associating them with the most reckless

of the Court's opponents. Thus the action of the ABA's House of Delegates in adopting in 1959 resolutions critical of the Court's decisions in the national security area was "entirely proper" when "taken out of context" but must nonetheless be condemned because "the timing . . . all but nullified the good intentions" and "contributed an air of legitimacy not only to the well-intended critics of the Court, but also to those who had made reckless attacks." (p. 99)

Despite the disagreements between this reviewer and Lytle at a number of points cited above, it must be noted that Lytle's volume is well written and very well researched. It provides an excellent commentary on the development of controversial lines of cases and the responses of some of the critics. This will be helpful to teachers of civil liberties, including this one. Such a use is, however, limited by the weaknesses in the conceptual framework, the organization and the objectivity of the presentation.—DAVID ADAMANY, *Wesleyan University.*

Double Jeopardy and the Federal System. BY LEONARD G. MILLER. (Chicago: University of Chicago Press, 1968, Pp. 152. $9.75.)

In United States v. Lanza, Chief Justice Taft, speaking for a unanimous Supreme Court, upheld the federal prosecution of defendants who had already been convicted by a state court for the same acts charged in the federal indictment. Although no justice had dissented in the case, J. A. C. Grant launched a vigorous attack upon the decision and the dual sovereignty rule which underlay it. Thirty-seven years later, in 1959, the Supreme Court reexamined the dual sovereignty rule in Abbate v. United States and Bartkus v. Illinois. The Court, through Justice Frankfurter in Bartkus and Justice Brennan in Abbate, affirmed the Lanza doctrine but this time Justice Black, joined by the Chief Justice and Justice Douglas, dissented. Unconvinced by the majority, Walter T. Fisher, counsel for Bartkus, continued his arguments in the law reviews. Now Leonard G. Miller has joined the debate.

Double Jeopardy and the Federal System is a defense of the dual sovereignty rule and in a sense it is also a detailed rebuttal to J. A. C. Grant's attack, the Black dissents, and some of Fisher's arguments. Since the author focusses upon the dual sovereignty rule, he does not explore the intricacies of double jeopardy *per se* but rather only those special possibilities of successive prosecutions which arise from the federal system. Professor Miller agrees with the results in Abbate and Bartkus because they rest upon a "110 year old formulation." Indeed the decisions were an obvious response to considerations of *"stare decisis,* judicial self-restraint, effective law enforcement and the pre-requisites of federalism." In support of these conclusions the author devotes the major part of his book to tracing the origins and history of the dual sovereignty rule in judicial adjudication and to analyzing the Bartkus and Abbate cases.

The dual sovereignty rule, he concedes, does not by any means ensure just results in every case yet, assuming the value of punishing the guilty to be as great as that of freeing the innocent, less injustice will arise from retention of the rule than from its rejection. In earlier cases courts have exercised their education function to mention the possibilities of injustice and to suggest that other branches of the government guard against them. Steps have been taken. Through the doctrine of preemption and through flexibility in sentencing the courts themselves have mitigated some of the potential harshness in the rule. Congress and some twenty states have responded by erecting statutory bars to successive federal-state prosecutions under certain circumstances. Prosecutors may also avoid such successive prosecutions by adopting standard operating procedures or by exercising individual discretion to avoid those successive prosecutions which will not effectuate justice. Exercise of such discretion is particularly feasible within the framework of the traditional cooperation between federal and state law enforcement officials.

Since the success of these attempts to mitigate the potential harshness of successive prosecutions seems important to the author's conclusions, more discussion of such attempts and empirical evidence regarding them would have been welcome to this reviewer. However, the interesting subject, "Prosecutor's Discretion" is disposed of in five pages which are devoted mainly to the directive, issued by Attorney General Rogers a few days after the Supreme Court, handed down its Bartkus and Abbate decisions, instructing federal attorneys to continue avoiding successive prosecutions except under compelling circumstances.

This book, a revised version of the author's doctoral dissertation, is a careful and exhaustive study within its selected limits. The materials of research consist almost entirely of the facts and reasoning in cases, arguments of counsel, legislation and law review articles. As approached, the subject does not lend itself to the newer methods employed in much research regarding the courts nor are such methods used. *Double Jeopardy and the Federal System* illuminates one segment of the constitutional law dealing with persons accused of crime. This segment has resisted the revisionism which has affected other parts of the general area and if Leonard G. Miller's arguments are heeded that revisionism will not be the wave of the future for the dual sovereignty rule.—THORNTON C. SINCLAIR, *University of Houston.*

The Australian Ballot: The Story of an American Reform. By L. E. Fredman. (East Lansing: Michigan State University Press, 1968. Pp. 150. $5.75.)

In the post-civil War era, *laissez-faire* ideas prevailed in politics as well as in economics, and the notion that government should become involved in the printing and distribution of ballots was novel, and indeed radical. But in the late 1880's the hour for this reform suddenly arrived. In 1888 the states of Kentucky (for the city of Louisville only) and Massachusetts adopted legislation providing for an official ballot listing all offices and candidates, available only at the polling place, to be marked there by the voter in secret. Within four years, 36 other states enacted similar laws, and by 1910 the Australian ballot was being used in all but two states.

L. E. Fredman, who teaches history at the University of Newcastle in New South Wales, has written a brief account of the origin of this kind of ballot in Australia, its subsequent adoption in Great Britain, and its rapid acceptance in the United States. He finds that credit for first adoption should go to the state of Victoria, which preceded South Australia by one month (in 1856) though the idea had been advocated in both states during the preceding five years.

The Australian ballot was espoused by Americans from many social and political groups who shared an antipathy for the party organizations of their day. Henry George published one of the first articles advocating it while editing a California newspaper. Richard Henry Dana III, Boston aristocrat, was the prime mover in securing adoption in Massachusetts. Grover Cleveland gave his unqualified endorsement in a major address between his first and second terms. In many states, labor unions provided the main organized support. The midwestern Populists put the Australian ballot in their platform.

In opposition were most of the party bosses. Cleveland's arch-foe, Governor David B. Hill, eloquently denounced the system when he vetoed the first bill to pass the legislature. But popular support appears to have grown rapidly, and one party or the other was likely to endorse the reform in a close election. The only Republican to cast a negative vote in New York's lower house was boss Thomas Platt.

Of course the reformers soon found that the machines were not to be overturned by revision of election-day procedures. They appear to have assumed that the venal voters had so little honor that the secret ballot would make vote-buying impractical, and that the parties would stop assessing the candidates if government relieved them of the cost of printing ballots. But there were plenty of other party expenses for the candidates to defray; and, while some improvements in election-day behavior occurred, it is hard to tell whether these were due to the reform or to more basic social changes. In fact, Fredman's account is spiced with references to another import from Australia—the "Tasmanian dodge" or endless chain, which enabled a precinct captain to provide each paid or intimidated voter with a completely marked ballot, once a single official ballot had been spirited out of the polling place.

Fredman's approach is that of a historian, and he has made good use of manuscripts and contemporary periodicals in describing the adoption of the reform in Australia and several American states, with special attention to the leading personalities in each case. A few dates in some of the standard texts could well be revised on the basis of his work. But much of the book is devoted to a very sketchy treatment of two large subjects—the social characteristics and political ideas of the upper-class, intellectual reformers who provided much of the leadership and publicity for the Australian ballot; and the assortment of electoral reforms that accompanied or succeeded it (direct primary, control of campaign expenditures, initiative, referendum, etc.). Historians will find little new here, and political scientists will find that they must still turn to older works, such as that of Specer D. Albright, for information on the kinds of voting that preceded the Australian ballot, the historical process by which the new system finally became universal in this country, and the widely varying responses that the states made to the many questions of ballot design and administration.

Fredman suggests that two further reforms—the short ballot and the nonpartisan appointment of election officials—are needed to realize the hopes of those who first advanced the Australian ballot. But perhaps a more pertinent and useful extension of their philosophy, in the light of current conditions, would be governmental assumption of some responsibility for providing voters with information about candidates. Public financing of campaign publicity, suggested long ago by Theodore Roosevelt, appears to be as much needed today as was public provision of ballots in 1888.—Henry Bain, *Washington Center for Metropolitan Studying.*

Voting and Nonvoting: Implications of Broadcasting Returns Before Polls Are Closed. By Kurt Lang and Gladys Engel Lang. (Waltham, Mass.: Blaisdell Publishing Co., 1968. Pp. 172. $2.50.)

Election night broadcasts which feature computerized projections of voting results have raised a rather serious policy question concerning the possible impact of these broadcasts on potential vot-

ers on the West Coast. In an election year, such as 1964, when the landslide dimensions of the eastern vote can become known to westerners before they go to the polls, it might be assumed that many on the west coast would simply not bother to vote. Thus, with modern communications methods, the results of an election could be influenced by those very results—or at least the projections of those results.

In order to test the effect of these election night broadcasts on voting behavior in 1964, Land and Lang studied late voters selected from sample precincts in the East Bay area of California. These respondents were interviewed during the week immediately following the election (while memories of election day were still fresh). As a control, the Langs also surveyed a sample of late voters in similar precincts in Cuyahoga County, Ohio, where polls closed before network election coverage began. Ohio was selected since it was also a pivotal state in the presidential contest and also had a highly publicized senatorial contest similar to the Salinger-Murphy race in California.

In general, this is a rather well-conceived research design. Some might argue that a panel design should have been used, rather than just an after-election study. Lang and Lang deal with that argument, citing especially the spurious turnover effects inherent in panel studies.

The major findings of the study should not be surprising to those who, like the Langs, are familiar with the short-term effects of mass media: the election night coverage by the networks was inconsequential in altering voting behavior. Lang and Lang found only one demoralized Goldwater supporter who lost all interest in voting after hearing about the lopsided Johnson margin. In an interesting chapter on "Nonvoting and the Nonvoter," the Langs go on to document the null effects of the broadcasts on turnout. Since the study employed registration lists as sampling frames, it was possible to perform a very careful check on reported voting and non-voting of respondents. The study also explored the possibility of bandwagon effects and/or sympathy votes. Neither type of voting change was discovered.

The authors explain these null findings by what they call the "law of minimal consequences." In essence, the potential voter, if he listens to these broadcasts at all, perceives them within a nexus of other factors. His political attitudes such as involvement and intensity of candidate choice, his previous expectations about the election outcome—perhaps informed by polls and other news—all affect perceptions of election returns. Other factors, such as a close senatoral race, can mitigate the effects of presidential returns.

Lang and Lang explore in some depth the social-psychological factors associated with early knowledge of election returns. Although behavioral change was not manifest, attitudinal changes did take place. In particular, some respondents reported that, upon hearing news of the returns, they were less eager to vote. This was especially true of those who felt certain that the network projections were correct, and also of those whose previous expectations about the outcome were invalidated. The authors continually warn of the implications of their findings on future elections. As the technology of election night projections becomes more reliable and thus more credible to the public, the potential for non-voting becomes greater.

For those interested in utility models of voter choice, Lang and Lang offer a chapter which should prove of some interest. What happens to voters who believe that the utility of their vote is approaching zero? This utilitarian factor is contrasted with other explanations of voting choice based on social-psychological dimensions (partisanship and civic-mindedness). Unfortunately, the measures used are quite weak. "Whether it makes a great deal of difference who won" is used for both utility and partisanship measures; there is no measure of party identification, etc. But more importantly, the data do not shed light on the major proposition we would like to test—that utility changes with certainty of outcome.

In all, this is an excellent, informative study for those interested in the policy questions regarding the delaying of election night broadcasts—indeed that was the purpose of the study. In addition, it offers some useful new information for students of voting behavior.—David E. RePass, *University of Minnesota.*

Government Contracting and Technological Change. By Clarence H. Danhoff. (Washington, D.C.: Brookings Institution, 1968. Pp. 472, $8.75.)

In 1954 Don K. Price wrote a perceptive essay on "Federalism by Contract," (*Government and Science,* Ch. III), bringing to our attention the fact that the federal government's research and development programs were creating a new dimension in public administration—the use of nonfederal contractors (business firms and universities) to perform government work. When Price wrote, about half of R&D was contracted out; by the 1960's the proportion had grown to 80 per cent, where it remains today.

With federal R&D expenditures running at a $17 billion annual level, accounting for two-thirds of all R&D in the United States, and drawing the presumably autonomous spheres of industry and higher education into novel relationships of interdependence with government, one might have thought that political scientists would have

ploughed this ground thoroughly as an area of both practical importance and theoretical interest because of its implications for the conception of pluralism as a system of diverse, autonomous sets of institutions. Such, however, has not been the case. With the exception of scattered articles and a recent book on atomic energy contracting, we have not followed up Price's lead.

Danhoff's book is therefore to be welcomed as the first sustained treatment of the phenomenon of contracting-out for research and development. More descriptive than analytical, it comprises an excellent background reference for the student of government on both the substance of R&D programs and the processes of contracting-out.

Before World War II, legislation and regulations circumscribed governmental procurement contracts rather tightly. The fixed-price, competitive bid system was the norm and almost the exclusive instrument. This was satisfactory for the purchase of Army shoes and civilian typewriters, but broke down during World War II when the need was for the development of new weapons for which no specifications existed. Danhoff details well the gradual shift over to Cost Plus Fixed Fee (CPFF) negotiated contracts and the accompanying rise in the proportion of extra-to intra-murally performed R&D works (Chs. II-IV). After a summary of the roles played by the President and Congress in R&D policy formation (Ch. V), he describes in considerable depth the range of relationships between government agencies and the four categories of contractors: business firms, universities, not-for-profits, and government-owned contract research centers (Chs. VI-IX). These chapters are valuable for rheir detail (and for their bibliographic footnotes) and also constitute a useful introduction to the complexities of the subject for those not already familiar with it.

Given the author's evident intention—to produce a factual guide to the processes, policies and problems of R&D contracting—the book is quite successful. One can still wish, however, that one who had looked at the field in depth would have speculated at greater length on the larger impact on governmental processes and the political system. The material for extrapolation is there. For example, Danhoff notes (pp. 183–184) that by calling in private institutions, government "expands the technical abilities directed to the problem, increases the alternatives to be considered, and may generate more participation by private groups and individuals in the debate" over program development. So far, so good. But he also calls to our attention that (p. 4)

> government has relied upon private institutions, not only to carry out projects already judged necessary, but also to suggest how its objectives might best be achieved, and even what the objectives might or should be.

This is the larger significance of contracting-out, as it is extended from military hardware development to strategic advice, to the operation of domestic soical programs (e.g., Job Corps centers contracted to business firms). The processes of government policy formation are being altered, perhaps radically, and the distinction between private and public in our polity is rapidly becoming meaningless. What do these developments portend? While Danhoff (an economist) does not grapple with this type of question, his book provides much information relevant to a needed examination of an important political system development.—MICHAEL D. REAGAN, *University of California, Riverside.*

Poverty and Politics: The Rise and Decline of the Farm Security Administration. BY SIDNEY BALDWIN. (Chapel Hill: The University of North Carolina Press, 1968. Pp. 438. $10.00.)

Sidney Baldwin has written a major book. In a clear and forceful style, and armed with 18 years of exhaustive research in the primary materials, including 43 interviews, he has analyzed the best part of the agricultural New Deal, the Farm Security Administration (FSA). Formed in 1937 out of antecedent agencies, crippled in 1943, the FSA was replaced in 1946 by the Farmers Home Administration which emerged chastened and "remained . . . docile . . . , attracting none of the enmities of the FSA, but also evoking much less of the excitement and hope that characterized its embattled predecessor."

In 1937–1943, the FSA dealt rather successfully with poverty in agriculture. Its "conservative" Tenant Purchase program had started some 40,000 tenants toward farm ownership by 1943, and its controversial Resettlement Program had tried to place some 10,000 farm families—small numbers in view of 6 million census farmers.

Meanwhile the Rural Rehabilitation (RR) program of FSA made loans and/or grants to approximately one million farm families in dire poverty, outside the ordinary public-private credit systems, often ridden with disease, always poorly educated and frequently semi-literate or less. In 1943, nevertheless, 75 per cent of them had paid back their loans or were fully current. The new social invention of supervised credit, including the joint bank account and the farm and home plan printed on reverse sides of a single sheet, and technical assistance had been successful—and were to be useful in some less developed countries. FSA reinforced them with ingenious programs of debt adjustment, group medical care, and producer and consumer cooperatives. Thus FSA proved that even society's outcasts could be reached and helped. Paternalistic? Maybe. John D. Black put it better: "You have to lead them by the hand." To their ever-

lasting glory, Rex Tugwell, Will Alexander, Paul Appleby, Beanie Baldwin, Pete Hudgens, Jim Maddox, and their patriotic colleagues reached out a hand to those who were sore in need.

"Success" did not mean that the problem was solved. In 1967 President Johnson's Commission on Rural Poverty found millions still living in circumstances that were a "national disgrace" and asserted that urban riots "had their roots, in considerable part, in rural poverty." Had the reactionaries in Congress, in the American Farm Bureau Federation (AFBF), and in the USDA been unable to beat the FSA to its knees, our urban travail might be much less. To be sure, FSA would have had to devise different methods of dealing with the poor as the situation changed; but the imagination shown in the 1930's might well have sufficed! Future attacks on poverty problems will be informed by Baldwin's impressive study, especially by his political and administrative analysis of the triumphs and mistakes (part of the mistakes may have been what Thaddeus Snow called FSA's "bad manners"; but I doubt it. FSA had to be aggressive to get anywhere, in my judgment; and the greatest error would be to confuse FSA's executioners with "gentlemen," Southern or otherwise.)

The FSA story fully justifies even more than the qualified encomiums of the book's last three paragraphs. We need more of Tugwell's "defiant and blasphemous faith" and less concern about "administrative minutiae [and] . . . institutional equilibrium, maintenance, and survival."

Chapters II through VI superbly describe farm poverty and tell the story through the creation of FSA. In chapter VII on "The FSA in Action," generally excellent, I missed an examination of regional differences in administration which might have compared Ernest A. Morgan's more vigorous attack on rural poverty in Alabama and the South East with the more cautious approaches of regions administered from Raleigh and Little Rock. Morgan dug more deeply into "the lower third of the lower third", I believe, and tried harder to reach Negro families. But then Morgan had the staunch support of the Bankheads and Richard Russell, whereas the Raleigh region was saddled with Senators McKellar and Byrd. In chapters VIII and X, despite the careful discussion of FSA's comparative disadvantages, I missed an examination of FSA's workload which ran between 100 and 125 RR families—an unwieldy number, considering the amount of individual counseling needed.

I am puzzled by the statement (p. 237) that Extension, the Forest Service, the AAA, and the SCS made up 30 per cent of the USDA's budget in 1937. Rather, I am sure that the price-support, production-control programs of the USDA have consistently accounted for some two-thirds of USDA's annual expenditures—a fact that has helped make the AAA and successors central in agricultural politics. Again, I should moderate the criticism of FSA for not fully exploiting the potential political support of local committees. AAA's committees became the priesthood of parity; SCS developed an organization of district supervisors with political muscle. These and other agencies, however, could directly organize the more influential farmers. But FSA's clients were as out of it politically as they were economically and socially. FSA advisory committeemen I met in 1941 were much more interested in their Production Credit Associations!

All these points are dwarfed by my admiration for Baldwin's book. FSA's administrative aspects as well discussed, including the uses of governmental information programs (p. 117–118). Congressional relationships are fully treated, especially in regard to Congressional group alignments (p. 321), and the superior strength of the House of Representatives in finally crippling FSA (p. 381 and 386). The book deals comprehensively not only with FSA's relationships to blacks but also to Japanese-Americans after Pearl Harbor. The politically crucial migratory farm labor program is examined. The shortrun advantage and the long-run disaster in the lack of a sound base in legislation are shown. Perhaps above all, the significant role of strategic individuals (for good or ill) is persistently underlined.

Finally, I turn to Baldwin's use of social science theory, beginning in chapter I which, however, concludes that "the scholar's concern with political and administrative means and methods . . . [contains] danger of neglecting substantive ends; and without some attention to such ends, the study of politics and administration may lose its substantive soul . . . Politics and government are inconceivable and meaningless on an uninhabited desert island" (page 16). Subsequently, Baldwin's faithful references to theory are relevant enough but usually add little to his analysis. Indeed, the power and precision of his analytical narrative is often underscored by the frail and diffuse models he adduces. Exceptions are chiefly references to Paul Appleby which always illuminate Baldwin's interpretation.—CHARLES M. HARDIN, *University of California, Davis.*

California's Prodigal Sons: Hiram Johnson and the Progressives, 1911–1917. By SPENCER C. OLIN, JR. (Berkeley: University of California Press, 1968. Pp. 253. $6.95.)

For California, Hiram Johnson is the home politicus par excellence. No governor, before or since, has had such a profound impact on the political process and style of the State. In the preface to

what is a short, clearly written monograph, Olin says he will focus on "the impressive gubernatorial administration of Hiram Johnson, and Johnson's role as a popular and extremely effective governor." In a lively narrative, Olin links together the major political events associated with Johnson's tenure in California (1911–1917): emergence and election as governor, important executive and legislative reforms, "Bull Moose" campaign, and in most detail, the circumstances of the 1916 Hughes defeat. More than six years in preparation, the monograph—a revised history dissertation—is exhaustively documented, with interpretations closely related to available scholarship. To the general reader, Olin presents an interesting and reliable historical account of the conspicuous political events of the Hiram Johnson era in California.

Beyond these noted achievements, however, the monograph proves disappointing. Written to update George Mowry's excellent *California Progressives* (1951), Olin does little more than add footnotes from new manuscript collections and recent dissertations; otherwise, nowhere does Olin attempt major departures or reinterpretations of Mowry. By design moreover, Olin eschews explanation in favor of description—"primary concern is with practice rather than ideology." The result is a facile exercise, however accurate, of historical journalism. At best, the monograph should be viewed as a modestly revised supplement rather than a serious rival to Mowry's work.

To a political scientist, the monograph reads much like an early article in *APSR* or *PSQ*. The book is resolutely descriptive, stressing specific events and avoiding concerns of concept or method. The framework for inquiry appears to be a chronological interest in Johnson's political adventures and reforms. The result is that Olin's selective tour through history lacks apparent direction, raising and abandoning ad hoc themes. Perhaps false hopes are created by the few historians most political scientists commonly read, for example, Benson and Hofstadter. But particularly when the subjects are electoral, legislative, and executive politics, the absence of even the most general findings or observations of political science becomes disturbing. Certainly Olin's statements about election results or "the means by which Johnson sought and won through politics" could benefit from more analytical attention.

Perhaps the most important disappointment was Olin's decision to identify and order events without devoting close attention to his stated focus on the Johnson administration. For in six years, Hiram Johnson led the State to adopt what was then a "radical" list of reforms. The State House record included direct primaries, nonpartisan elections for cities and counties, cross-filing, initiative, referendum, and recall, controls of public utilities, workmen's compensation, conversation of natural resources, comprehensive civil service law, establishment of a legal minimum wage for women and children, and many others. These measures profoundly changed the character and content of the politics of California. How and why Johnson was so signally successful are questions that merit the closest attention, but these unfortunately are not the concerns of Olin—Johnson's political tactics, strategies, goals, and style are, for the most part, left to the inferences of the reader.

Such criticism should be kept in perspective, for Olin's credentials and approach are not those of a political scientist, and perhaps more important, the literature we can offer on California politics is quite limited and with few exceptions, highly descriptive. Except in introductory textbooks for the lucrative junior and state college market, few political scientists have devoted any sustained attention to *California*. None have attempted anything like Seymour Lipset's *First New Nation* or even Duane Lockard's *New England State Politics*. Protests aside, most of the literature is singularly ahistorical. Thus, Olin's monograph is a useful reminder of the importance of the historical context on the practice and ideology of a state's politics. For Olin accurately scores the Johnson deeds and major political events between 1911–1917; and this description should provoke the serious reader to ask the more critical questions about political change and the "authoritative allocation of values" in California. Yet this reviewer hopes that in the future, Olin will use his demonstrated skills in research and writing to clarify and search for explanations rather than report on the news stories of political history.—RONALD O. LOVERIDGE, *U. of Cal. at Riverside.*

Moment in the Sun: A Report on the Deteriorating Quality of The American Environment. BY ROBERT AND LEONA TRAIN RIENOW. (New York: Dial Press, 1967. Pp. 286. $6.00.)

This book is reviewed here particularly because of the demand made at the APSA conference, 1968, for greater policy relevance in our journals. In terms of the concern, underlying that demand, the Rienows' book may well be more important than anything else published by professional political scientists in this generation; the only book which I recollect having reacted to similarly in my lifetime is Myrdal's *American Dilemma*. Yet, *Moment In the Sun* has not, to my knowledge, been reviewed in any social science journal; as it stands, there are understandable reasons why this should be so; and, by one of the characteristic iro-

nies of political ideology, it is likely that the proponents of a "more relevant" political science will be less receptive to its emphases than their less relevance-conscious colleagues!

For, essentially, it makes an extremely comprehensive indictment of the whole notion of economic growth and technological progress as this is generally admired in the United States today. In making their indictment, they have placed in perspective many discrete items of which we are in general only aware as specifics. They have shown how the deterioration of the environment and the reduction in many ways of the quality of life is a consequence of *ungoverned* technology.

The approach is, of course, ecological. The environment is becoming progressively less livable because of such matters as: (a) the unpleasantness of breathing and the contamination of oxygen (with such effects as increased incidence of emphysema and in extreme smog even suffocation) (b) the excruciating increase in noise levels (with consequences not only in irritability but in such physical defects as deafness and possibly even ulcers) (c) the greater severity of flooding in much of the country, and resultant greater danger from hurricanes, etc. (because of the removal of the forest and grass cover) (d) the stench and ugliness of what used to be beautiful lakes and beaches (e) the reckless killing-off of many beautiful and useful species of animals and birds, and the consequent multiplication of destructive insects and "real" vermin, that is to say the upsetting of the balance of nature (f) and, partly following on the last, spraying with and ingestion of pesticides, the dangers of which are unknown and potentially great—which are not regulated, to any significant degree, as yet. Instead "when a proscribed poison becomes too prevalent in a product, (the F.D.A., Department of Agriculture, etc.) simply legalize it." (p. 162). Not content with all these costs of technology, progressive men—including some who advocate conservation in other areas—see a great future in similar exploitation of the ocean depths, because they do not "conceive that the powerful force which slams our coasts . . . is (also) a delicately balanced creation. (Already) in a ceaseless stream . . . man pours his wastes and poisons to transform this primeval cradle of abundant life into a graveyard. This is the 'untrammelled ocean' in which mankind sets so much store (as a source of future foodstuffs!) (p. 197)."

The Rienows have, in actuality, brought Malthus up to date for the waste-producing affluent society. "Science, lassoed by the lariat of commerce, . . . is likely to be a destructive force . . . Desperate to remedy environmental deficiencies, we shall without careful social consideration deface our surroundings even more . . . (for example) engineers are eager to control water shortages by building "hydraulic empires, but "we have seen how all these 'hydraulic empires' now long buried under Mediterranean sands went into a frenzy of . . . building, only to find themselves every year buried deeper in upland silt from the stripped watersheds. Unlike the ancient civilizations, we can not plead ignorance of ecological principles . . ." (p. 207)

But, unlike the ancient civilizations, we must contend with continual population increase, due to the control of epidemic diseases, *and* we have the democratic insistence that everybody is entitled to be affluent—which means to pollute the atmosphere, poison the soil, and leave wastes which destroy the quality of life. If we focus on the problem of poverty, we get campaigns against poverty, emphasis upon economic growth, etc., but if we focus on the problems of the quality of the environment, we have to rethink our emphasis on technological "betterment." If we think about the two together, we are confronted with considerable cognitive strain (until we get accustomed to the juxtaposition). What, for instance, will be the effect of raising economic standards, according to present proposals, in Appalchia, the Ozarks, rural Maine, and the rural South? The people who have been pushing for a policy-conscious political science, by and large, are the people who are committed to, e.g., campaigns against poverty; because of this commitment, they are likely to find the emphasis which the Rienows make unacceptable.

The greatest weakness of the Rienows' book, the other way around, is that it, too, is not written in terms of balance; obviously, for instance, once a control for malaria has been discovered, it will be used; obviously, in human terms, such a control is, on the whole, desirable; as Frank Graham, Jr., says in a review of their book: sometimes "they pile horror upon horror in the manner of an early Elizabethan tragedy" . . . in such a way as to cause some readers to "succumb to shell shock." The weakness is comparable to their predecessor, Malthus; the atmosphere of gloom overwhelms. How, one might want to know, do you strike a balance? Optimally? Or granted a world in which a presumptive Secretary of the Interior expresses grave doubts about "conservation for conservation's sake" incrementally?

At this point, we come to the kind of policy relevance which concerned people everywhere have a right to ask from political scientists. For policy problems, taken seriously, as analytic conceptions, rather than as the bases for doctrines, involve weighing and balancing, in the words of former Congressman and Philosophy Professor T. V. Smith, "the turning of principles into interests."

But, since no other writer, either on conservation or on civil rights, poverty, et cetera, has to my knowledge tried to balance the seemingly incompatible values here against each other or tried rigorously to work out ways to escape between the horns of the apparent dilemma, *Moment In the Sun* must be enthusiastically recommended.—LEWIS A. DEXTER, *Dalhousie University (visiting)*.

COMPARATIVE GOVERNMENT AND POLITICS

Political Development in Latin America: Instability, Violence, and Evolutionary Change. BY MARTIN C. NEEDLER. (New York: Random House, 1968. Pp. 210. $2.45.)

Latin Americanists in our discipline have long been criticized for their failure to incorporate modern methods of political analysis into their research. Past studies have tended to rely on recognizably outmoded, traditional approaches or have failed to take note of significant work emanating from related fields. Consequently, the study of Latin American politics has acquired an altogether too well-deserved reputation for archaism, loose methodology, and, in a few cases, poor scholarship. Notable exceptions exist to be sure; Kalman Silvert and Charles Anderson are only two of a growing number that have urged the application of more stringent techniques and have attempted to set examples through their publications. On the whole, however, Latin America has taken a back seat regarding modern political science. Specialists trained in other geographic areas have produced some of our most valuable studies, although unfamiliarity with historical, cultural, and anthropological patterns of the continent has caused certain additional problems.

Happily, Professor Needler has combined the talents of the methodologist with those of the professional Latin Americanist. *Political Development in Latin America* is a short but firm step to remedy the lamentable situation described above and is exactly what the author intends it to be, ". . . no more than an early stage of succession of increasingly exact or comprehensive approximations." (p. 7) A concise and conceptually clear model is presented, hypotheses are drawn and tested, and the reader feels that loose ends are finally being pulled together. At the same time, Needler is plagued by the same old bugaboo that has discouraged other Latin Americanists from attempting quantitative studies; sufficiently accurate "hard data" suitable for comparative purposes are not available. This dilemma is readily acknowledged, and the reader is cautioned to accept all conclusions as "suggestions" rather than demonstrable proofs. The suggestions are condensed into a set of twenty-three theorems that will hopefully become hypothetical assumptions to be tested by future analysts as data become available.

One might not always agree with either the definitions or operationalizations of Needler's concepts, yet they seem tenable given the dearth of data and previous systematic studies. The reader is assured that the major dependent variable, political development, should not necessarily be confused with "democracy" or "democracy as currently practiced in the United States." Development is defined as the transition from one stable state of equilibrium to another so long as that transition is in the general direction of the societal goal; the societal goal, in turn, is defined in terms of the ideological norms a polity posits for itself. In the case of Latin America, Needler argues, the ideological norms do, in fact, approximate current North American and Western European political practices since the area has been so heavily influenced by the western powers. Written constitutions are often little more than Spanish translations of the Philadelphia document, the mass media has been fed by western news services, and generations of Latin political leaders have received their educations in the *patrias* of Jefferson, Locke, and Montesquieu. Hence, although developmental goals are not synonymous with democracy in a theoretical sense, the peculiarities of the Latin experience make them alike in practice. In other words, the Latin Americans have posited democracy as the ideological goal of their polities. Democracy itself is conceptualized as: 1) the maintenance of constitutional integrity (assuming that written constitutions are democratic in content) plus 2) extensive popular political participation.

Certain semantic problems occur regarding the words "stability," "development," "change," and "democracy." Stability, we are told in the introductory chapter, is to be regarded as a type of homeostatic equilibrium; development is the transition from state to state in the direction of the ultimate goal. One, then, must regard "change" as any type of movement, whether in the general direction of the goal or not. Unfortunately, "stability" is often used in subsequent chapters as a synonym for "constitutional stability" or "constitutionality" (which is, by definition, one aspect of the societal goal, democracy); at other times "stability" appears to mean nothing more than the absence of open violence. Likewise, change and development tend to be used interchangeably, as do development (the transitory process) and democracy (the final goal). Consequently, the reader

becomes somewhat confused. Normally, these substitutions could be excused as stylistic attempts to avoid cumbersome repetition, but they cannot be overlooked in a book whose *raison d'etre* lies in the purity of its methodology and the "tightness" of its concepts. These problems should be regarded more as inconvenient nuisances than major faults, however.

This reader found the chapter concerning interrelationships between economic and political development the most intriguing. Past studies have attempted to correlate either constitutionality or, to a lesser extent, political participation with economic growth, but the simultaneous comparison of both aspects is a genuine contribution. Through very crude statistical tests Needler proves (suggests) that political development toward democracy is indeed positively related to economic development, although not necessarily in the "intermediate ranges." In simplest terms, both aspects of political transition cannot occur simultaneously unless economic growth is parallel. For example, if participation increases faster than the economy, depressed participants will overload the system with "other-than-peaceful" demands, often provoking military intervention and consequent loss of constitutionality. Alternately, if constitutionality outdistances economic growth, mass participation cannot be prevalent. The deference displayed by the British masses is cited as an example of the latter alternative. Numerous other hypothetical implications could be derived from this thesis—hopefully they will be analyzed in the near future.

In conclusion, the serious scholar seeking to incorporate modern theory and techniques into his research can gain much from this modest though significant study. The conceptual framework is sound, operationalizations and tests are crude but reasonably exact, and a host of new insights are suggested. Every bit as noteworthy, important contributions of Almond, Coleman, Easton, LaPalombara, Powell, Pye, Verba, Weiner and other innovators are finally being applied to the study of Latin American politics. This book definitely merits its adoption as a supplementary text in graduate and undergraduate courses concerning politics of the Latin American republics or political development in general.—BURT H. ENGLISH, *The University of Kansas.*

Egypt: Military Society. ANWAR ABDEL-MALEK. Trans. C. L. Markmann. (New York: Random House, 1968. Pp. 458. $8.95.)

Egypt: Military Society: The Army Regime, the Left and Social Change Under Nasser (a translation and revised updating of *Egypte: Societe Militaire* first published in 1962) clearly qualifies as the most important socio-political analysis to appear dealing with post-1952 Egyptian politics. Ahe combined product of reflective intellectual insight and exhaustive scholarly research (based almost entirely on Arabic sources but also accompanied by a command of European and American secondary scholarship) it is simultaneously thematic and exhaustive. Its theme derives from the Egyptian-Coptic birthright and Marxist ideological commitment of the author. Thus a portrayal of Egypt's essential national identity (rather than Arab identity) emerges while the substance of the book details the vacillating fortunes of the Egyptian Communist Party under President Nasser. The conceptual framework of the book is also Marxist in its concern with the economic basis of social and political factors. The utilization of this framework, as one might anticipate, has its difficulties but Abdel-Malek more than any other present scholar elaborates for us in a sympathetic manner the social basis of the regime and its policy shortcomings.

The book consists of four sections entitled: (I) "Egyptian Society Before the Coup d'Etat," (II) "The Social Character of the Military Regime," (III) "In Search of a National Ideology" and (IV) "The Value of the Egyptian Experiment."

In Part One "Egyptian Society Before the Coup d'Etat" there is found a compact treatment of largely nineteenth century and pre-1952 history which is instructive in its discussion of the relation of the growth of individual land ownership in nineteenth century Egypt to Egyptian social structure and the manner in which this relationship is related to the politics of particularly the post-1924 parliamentary period. What emerges is a strongly sympathetic interpretation which sees the Wafd during the entire period and the Left after 1945 as the true registers of Egyptian political sentiment while the large landowning class and industrial class are seen (correctly) as having dominated the formal processes of government in collaboration with the Palace. As he accurately notes, the Wafd as the truest political expression of the Egyptian people only ruled a total of seven out of twenty-eight years and, he might have noted, never for more than twenty-four months and this only in 1950–52 when perhaps partly as a result of persistent exclusion from the political process it was flaccid, energyless and without authority.

In Part II, "The Social Character of the Military Regime" he presents an acute sociological analysis of the shifting class alliances of the lower middle class military regime. Thus the 1952 Agrarian Reform Laws are seen primarily as political and economic in intent and not drafted in terms of the aims of social justice. They were successful in destroying the economic base of the large landowning class but they were unsuccessful in diverting this class's economic resources from the

agricultural to the industrial sector. In the late 1950's his economic failure became apparent and the alliance of the regime came to be with the industrial middle class. The failure of the union with Syria was due at least partly to the manner in which this class sought to take economic advantage of the Syrians and the July, 1961 reform laws and arrests in late 1961 and early 1962 were intended to remove this class. Since that date the military has ruled in alliance with the technocrats of what the author terms the American-German school of administration.

Part Three "In Search of a National Ideology" documents the ideological groping of an essentially pragmatic regime. The regime is characterized as being persistently and forceably opposed to a Marxist ideology and incapable of generating one of its own. In the course of his discussion he elaborates at length upon what can be said to be one of the two greatest failings of the regime, that of its inability to attract to it the abundant imagination and creative talents of Egypt's intellectuals (the other failing is that of not having enlisted in spontaneous fashion the support of Egypt's rural masses).

Finally in Part Four, "The Value of the Egyptian Experiment" he concludes that contrary to the regime's claims it is not socialist because the profit motive operates in the milieu of state capitalism with all decisions being made at the top and with no provision for the expression of popular demands. He concludes with an appeal for a return to pluralistic democratic politics in Egypt while at the same time noting that the present regime's fifteen years in power and the June, 1967 defeat have created the potentialities for genuine social reform.

As already noted, Abdel Malek analyzes in detail the nature of one major failing of the regime namely its alienation of Egyptian intellectuals. His explication of the failure of the regime to mobilize the rural masses and transform (contrary to the author's assertion, p. 62) their historical political passiveness is perhaps the least convincing portion of his analysis. His analysis rests largely on the distracting qualities of the regime's agrarian reform programs and its unsysmpathetic bureaucracy rather than upon the actual sociology of the countryside. As long as the maximum land holding (as of July, 1961) continues to be 100 feddans (roughly acres) we can speculate that the peasant's traditional subservient relationship to his umda (village headman) and the latter's subservience in turn to the resident landowning class (50–100 feddans is usually considered a large holding) has not changed. It is this relatively untouched character of the countryside which perhaps at least partially explains both the relative stability and the persistency of libertarian or from the point of view of the regime "rightest" sentiments in Egypt after fifteen years of revolution. The dilemna of the regime is real, for to reduce the size of the maximum land holding would probably result in an amount of agricultural economic dislocation that Egypt's tightly drawn economy can not tolerate.

A further minor comment is in order in reference to the development in the last chapter of how the army in modern Egyptian history (i.e. from 1882) has grown with Egyptian nationalism (p. 360). This is not true and is lost sight of by many analysts of the army and the Egyptian regime. After the disbandment of the army in the wake of the Arabi rebellion Egyptians had little military and no political role in their army until the date of Nasser's entry into the military academy in 1937. Once alerted to the potential bias involved in a Marxist analysis, this book can be read with equal profit on the two levels of general background and minute attention to its detailed documentation.—Louis J. Cantori, *University of California, Los Angeles.*

Managerial Power and Soviet Politics. By Jeremy R. Azrael. (Cambridge: Harvard University Press, 1966. Pp. 258. $4.95.)

The political potential of Soviet managers and economic administrators to influence the development of the Soviet regime has been a subject of speculation ever since the Bolsheviks came to power. Even before the Revolution, as Azrael shows, anarchist writers had argued that the result of Marxist revolution would be to replace the capitalist exploiters with a new ruling stratum of engineers and technicians. Since the Revolution its enemies have relied on Soviet administrators and technicans to divert the regime from revolutionary aims, while such functionaries have appeared to friends of the Revolution as a chief source of the danger of its subversion.

The worst fears of some, including men who dwell in Peking, have been realized—or so they say, but they obviously exaggerate. Even if, as may plausibly be argued, the regime's revolutionary dynamism has been attenuated and economic technicians have contributed to this result, fundamentally the regime remains what it was. It has not been transformed into the administrative-technocratic state predicted by some, or into the liberal-democratic state hoped for by others. Were such expectations warranted in the past? Are they warranted today?

In his effort to answer these questions, Professor Azrael of the University of Chicago has reviewed the place of managers and specialists in the Soviet scheme of things. His definition of the managerial

elite—"those executives who have borne primary responsibility for the administration of Soviet heavy industry" (p. 7)—properly includes economic administrators as well as factory managers. (By limiting their sphere to heavy industry, however, Azrael creates some unnecessary difficulties.) He was not satisfied to look only at recent developments, but carried his research back to the beginnings of the regime and even to the doctrinal controversies that preceded its founding. *Managerial Power and Soviet Politics* is at once *scholarly*—being based on a close reading of Soviet writings on the subject, as well as on relevant speeches of Soviet leaders; *journalistic*—since Azrael reports on views he encountered among managers during an extended stay in the USSR; and *polemical,* since he has strong opinions which he sets against those who have written differently on the subject. The result is a closely argued analysis which, though somewhat involuted at times, is written with a lively and forceful style and is both rewarding and often extremely interesting. One of the book's chief virtues is that it makes accessible a great deal of information on the problem of the managers, that lay buried in old Soviet books, and has directed Soviet scholars to the sources where more can be found, and where Azrael's own findings—some of them disputable—may be further tested.

The book is not without flaws. At times, after making a valid and original point, Azrael carries his argument further than the evidence warrants. He is able to show, for example, that contrary to a common view, the patristic writers (Marx, Engels and Lenin) recognized the need for authority and discipline in socialist industry. He goes on to assert, however, that according to the patristic doctrine "the old management forms . . . were to remain essentially what they were" under capitalism (p. 15). The evidence he cites does not prove this; moreover, we are told that many party theorists believed the patristic doctrine to be the opposite of what Azrael says it was (p. 41), and that Lenin himself engaged in "purposeful obfuscation" of that doctrine. Occasionally Azrael mis-stated the evidence, as when he says that Khrushchev at the Twentieth Party Congress "proposed that the central ministries should be stripped of most of their operational functions and perhaps even completely abolished" (p. 132). Actually, Khrushchev never mentioned the possibility of abolishing the central ministries before he suffered a defeat at the hands of the economic bureaucracy at a meeting of the Central Committee in December, 1956.

On the central theme of the book, Azrael succeeds in exposing the serious difficulties in the determinist view that industrialization and economic development inevitably lead to the administrative-technocratic state, or, as some would argue, to the democratic state. This is a useful service. Azrael's target is not limited, however, to such economic determinists. He is also critical of the view that the managers have a large political potential which, if realized and used in accordance with their inclinations, could lead to major changes in the character of the Soviet political system. Unlike some critics of this view, Azrael does not deny that the managers constitute a distinctive political group in the Soviet system, or that it has characteristic political views that have sometimes brought it into conflict with the party apparatus. However, he is inclined to emphasize the docility of Soviet managers and their willingness to submit to the will of the power-oriented leaders of the party apparatus. He also stresses their political incapacity once they do enter the political arena. The managers have failed to take power or win great political influence in the past and, in Azrael's view, the prospects are that they will not do so in the future.

What of the widely held view among Sovietologists that economic administrators played an important political role in the Stalin succession crisis? Azrael will concede only that "managerial support was not a completely negligible factor in the political arena" (p. 131). This assessment—oddly, it is perhaps Azrael's strongest acknowledgement of the political significance of the managers—indicates how far his zeal to refute the economic determinists has carried him in downgrading managerial power. Actually, the economic bureaucracy played a major role in recent Soviet political history, and its political potential remains high. How far it is realized will depend on such circumstances as the political capacity of its leaders and partisans and the depth of factional divisions among leaders of the party apparatus, especially during crises of succession.

By providing the first systematic examination of managerial power and Soviet politics Azrael has contributed importantly to Soviet studies. Political scientists who are interested in the role of managers in other regimes, as well as in the broader question of the relation of economic progress to political development, will find here much that is relevant and useful for their purposes.—MYRON RUSH, *Cornell University.*

Les Familles Politiques Aujourd 'hui en France. By EMERIC DEUTSCH, DENIS LINDON, PIERRE WEILL. (Paris: Minuit, 1966. Pp. 126.)

The book presents the political opinions of the six "political families"—extreme left, moderate left, center, moderate right, extreme right, and the *marais* (nonparticipants)—into which the French electorate is divided, the party identifications of

the families, and their voting patterns in the 1965 presidential elections. Classification was by self-assignment on a left-right scale.

Responses to questions on political issues reveal the absence of sharp ideological division between left and right. Except for the extreme left, considerable heterogeneity of opinion exists on most issues within each political family. On no question do as many as three fourths of those in the moderate and extreme left oppose three fourths of those in the moderate and extreme right. In some cases, a large segment of the left takes a "right" position, e.g., against nationalization; in others, the right takes a "left" position. Despite the lack of sharp ideological cleavage, the authors assert that a "temperamental" difference continues to divide left from right, with the two groups having broadly different opinions on most issues.

The survey data on which the study relies suggests an asymmetry between left and right, stemming from greater ideological homogeneity on the right than the left and a greater affinity of the center for the right. Analysis of the political families' attitudes toward political parties also reveals a cleavage between the moderate and the extreme left. Whereas the right is relatively united politically, the moderate left displays great hostility toward the PCF. (And the surprisingly lukewarm approval of the moderate left for its "natural" home, the FGDS, helps explain the FGDS' collapse in 1968.) The survey data also underline the fundamental conservatism of French politics. Political and ideological fragmentation on the left constrasts with homogeneity on the right. The center and the *marais* lean toward the right. (The *marais* hardly participates politically but, when it does, expresses rightist opinions and behaves in a rightist manner.) The center, moderate right, extreme right and *marais* total 65 per cent of the sample, the left, 35 per cent.

As one of the early efforts in French survey research, it is to be expected that the book contains weaknesses. The assertion that French political opinion is divided into six families is an artifact of the scale; if respondents had been asked to place themselves on a scale with a larger or smaller number of segments, the number of political families would have varied accordingly. Futhermore, while the authors are free to choose which groups to analyze, one might question the emphasis placed on the six families. Especially given their lack of ideological and political homogeneity, more profitable results might have been obtained from a focus on the different groups of party identifiers—who have a political reality that, as the authors themselves argue, is lacking for the political families.

More serious, if major attention is to be centered on the political families, they should be defined with care. Yet the *marais* is constructed—and distinguished from the center—in a most questionable manner. Two groups of respondents are combined to form the *marais*. One, the non-participant center, consists of respondents who classified themselves in the center of the political continuum but reported little or no political interest. Whereas respondents placing themselves anywhere but the center who reported little or no political interest were considered part of their self-assigned group, the non-participant centrists were assigned to the *marais*. The other constituent of the *marais* consists of respondents who refused to classify themselves on the left-right continuum. The two steps requiring explanation are thus the separation of the participant and non-participant centers (unlike the two segments of the other political families), and the grouping of the non-participant center with the politically unclassified.

The authors explain that, for all political families but the center, "There weren't sufficiently large differences to justify separating participants and non-participants." For these families, "there weren't appreciable differences in opinions and behavior between participants and non-participants. On the other hand, the participant center distinguished itself sharply from the non-participant center, which closely resembled the unclassified." (p. 20) However, inspection of appendix 10, where responses to questions on issues are tabulated for participants and non-participants in each family, suggests that the data do not support the hypotheses. Within the extreme left, participants and non-participants diverge more than the two center groups as frequently as the reverse. The two wings of the extreme right are also nearly as divergent as the two wings of the center. The divergence between the two groups of the center is thus not necessarily greater than the divergence between the two components of other political families.

Moreover, the additional step of combining the non-participant center with the unclassified is also unwarranted: the non-participant center resembles the participant center and the non-participant right about as often as it does the unclassified. While the authors might be perfectly justified in assigning the non-participant center to the *marais*, the reason would be their lack of interest, not their opinions. Either way, however, the non-participant center should not have been treated differently than non-participants from other political families.

The consequence of the methodological flaw is that the proportion of the nonactivists, the *marais*—32 per cent of the sample, according to the authors—is either inflated (by erroneously assign-

ing to it the non-participant center, which constitutes 22 per cent of the sample), or depressed (by not including the 31 per cent of the sample constituted by non-participants located elsewhere on the political spectrum). Whereas the authors express surprise at the large dimensions of the *marais*, American political scientists might assert that, on the basis of the data presented in *Les Familles Politiques*, the proportion of nonactivists in France is double the size of the *marais*. Subsequent survey research in France should benefit from the book's strengths—and weaknesses.—MARK KESSELMAN, *Columbia University*.

Il comportamento elettorale in Italia. By VITTORIO CAPECCHI et al. (Bologna: Il Mulino, 1968. Pp. 474. L. 10,000.)

This book is the first product (there are to be three additional volumes) of a major research undertaking conducted by the *Istituto Carlo Cattaneo* in Bologna and sponsored by the Twentieth Century Fund. The overall theme, or object of inquiry, is political participation in Italy. Capechi's work, as befits an introductory volume, is concerned with that broadest of all modes of political participation, national voting behavior.

The method of voting analysis employed might best be described as self-consciously ecological; it is ecological in the sense that the study reports associations between particular geographic voting patterns and census-derived demographic variables, while it is self-conscious in that the authors are aware of the predictive limitations imposed by this approach. There is, in fact, a methodological essay in the Appendix in which the warnings of Alker and Ranney along these lines are catalogued and discussed. As to the inevitable question about why no use was made of the available survey data (the *Istituto DOXA* has been sampling Italian opinion since 1948), the authors respond by noting that such surveys have tended to underestimate the extent of popular support for Left parties, particularly the Communists. They explain this latter phenomenon in terms of the wariness with which Italians, especially those hostile to the regime, confront the interview situation.

More specifically, the book is organized so as to portray continuities and shifts in party strength throughout the country over the 1946–1963 period. After a short and useful chapter on Italian electoral history since unification, concerned with the extension of the franchise and the rise of mass parties in the pre-Fascist epoch, the authors begin their analysis making use throughout of two basic sets of data: electoral returns at the communal level and the censuses of 1951 and 1961. The version of the party system employed is a simplified one with only the Communists and Christian Democrats treated as discrete entities. The remaining parties are reducted to two *ragruppamenti:* the Non-Communist Left and the Right.

Although Italy's seven thousand some-odd *communi* are taken as the basic units of analysis, the writers cluster them in ways which permit the examination of broader regional voting configurations. To this end the analysis is conducted not only at the communal and provincial levels, but also at the "zonal." The country is divided into six "zones;" the Iron Triangle of Piedmont, Lombardy and Liguria, a "White" zone which is defined as having a Catholic sub-culture and includes the Veneto plus surrounding provinces, a "Red" zone in the center of Italy—Emilia, Tuscany, Umbria and the Marches—to which a left or socialist sub-culture is ascribed, and finally the South plus Sicily and Sardinia are accorded separate zonal statuses. Having defined the geographic entities to be used, the authors then go about their task, making imaginative use of transition and correlation matrices and linear causal circuits along the way, of relating party voting patterns to an array of independent variables.

Perhaps the most general and striking of the authors' substantive observations is the growing stability and/or nationalization of Italian electoral behavior over the post-war period. First, with respect to the Left, it is reported that the percentage of the vote received by all Left parties, including the Communist, has remained relatively constant, but within this gross pattern the Communists have been receiving a growing share of the other Left parties in the "Red" zone of central by the party's ability to attract voters away from the other Left parties in the "Red" zone of central Italy and its increased attractiveness in the just-now modernizing South—thus off-setting Left losses in the North. In other words, the Communist gains may, in large measure, be explained by the fact the South is becoming more like the North.

Second, there appears to be a rather similar pattern occurring at the Center and Right of the party spectrum as well. Namely, the Christian Democracy has been gradually increasing, at a slower rate and more unevenly than the Communists to be sure, its proportion of the total Right vote. Here again, it is in the South where most of the Christian Democratic gains have been registered. The Liberal party's role in the North, as a repository for disaffected Christian Democratic voters, has seemingly limited the latter's absorptive capacity.

What impresses the authors most about the electorate's growing homogenization is that this secular trend has taken place against a socio-economic background of unparalleled urbanization

and industrialization. But what strikes this reviewer as being of significant interest is the incompatibility between the work's empirical findings and Sartori's recent analytical theorizing about the Italian party system's growing fractionalization and centrifugal drives.

It is also relevant to note that Communist strength has grown in areas undergoing rapid socio-economic change i.e., increasing urbanization, literacy and industrialization, while an expanded Christian Democratic vote, for the most part, shows either no correlation or a negative correlation with the introduction of these processes. A point at issue here is whether the Communists' vote benefits from the presence of these characteristics per se or if the party's growth derives more from the process of change itself. On the basis of a considerable amount of data, the authors opt for the second alternative. They point out, for example, that the Non-Communist Left's vote is strongest, or has declined least, in areas like the Iron Triangle which are already heavily industrialized. This observation, in turn, may be interpreted as providing additional evidence in support of the Benjamin-Kautsky hypothesis about the curvilinear relationship between Communism and modernization.

The book's findings are marred by the strategy of analysis in at least two ways. First, the authors' decision to regard the Italian Right—Liberals, Monarchists and Neo-Fascists—as a single unit rather than as modern, business-oriented Right and traditional, clientele Right, leads them into drawing some rather bizarre conclusions about the relationship between "Right" voting and economic growth. Second, with respect to the construction of "White" and "Red" zones—areas reputedly possessing Catholic and socialist sub-cultures—the writers ascribe the Christian Democratic and Communist successes in these areas to some ill-defined form of community pressure. Yet they offer no data which might serve to demonstrate the existence of these sub-cultures other than the tautological proof of habitual voting patterns.

On balance though, the volume represents a considerable advance, particularly in terms of the rigor of the statistical measures used, over the earlier work of such scholars as Mattei Doggan and Giovanni Schepis.—LEONARD WEINBERG, *University of Nevada.*

Local-Level Politics: Social and Cultural Perspectives. ED. BY MARC J. SWARTZ (Chicago: Aldine, 1968. Pp. 437. $9.75.)

This book is the revised publication of papers presented at a conference on political anthropology held in Austria in 1966. A theoretical introduction by the editor is followed by seventeen empirical case studies and one methodological piece on the study and measurement of social networks (Barnes). Seven of the cases are African, including one that compares African and Melanesian meterials; five are from India, and one each from Burma, Norway, Mexico, circumpolar regions—particularly Siberia and the Aleutians, and Taiwan. In all of the cases, the emphasis is on the political life of local communities, or ethnic groups as polities in and of themselves and as parts of larger wholes. There is as well a refreshing sense of time and change introduced. In each of the cases, we are able to see the local political system, not as some timeless, assumedly equilibrated system operating in a fictionalized ethnographic present, but as living political structures whose actors adapt and change (or fail to) in a changing world.

The theoretical introduction by Swartz carries forward the ideas he presented with Turner and Tuden in his pervious book, Political Anthropology. Swartz eschews what he calls the structural-functional approach and uses instead the analysis of events which are sorted basically into political "fields" and "supports." A field is defined as the "interest and involvement of the participants in the process being studied . . ." (p. 9). Supports are seen as "anything that contributes to the formulation or implementation of political ends" (p. 19). The editor feels that by analyzing the political goals of actors in the "field" as these go through a set of phases, and by isolating the various supports given to actors or groups, a processual and "dynamics" analysis of political activity can be obtained from data collected at the local non-Western level. This level is often characterized by a non-differentiation of political activity from other action and thus a mode of analysis of *the* political is a difficult methodological problem in anthropology.

Swartz admits that he has not presented a full-blown theory and more importantly he also admits that most of the material in the book is "chiefly concerned with different aspects of the structure of the political arena" (p. 49). In other words the editor has adopted a number of conceptual categories, "field," "arena," "support," "regime," "phase" which he feels can be used more adequately to describe (e.g. "field") and analyze (e.g. "support") political phenomena. However the concepts are not related to one another in a systemic manner such as to create a general, explanatory picture of a local-level polity, nor are hypotheses generated for wider testing on larger, or other samples of cases.

On the other hand, this is the strength of the theoretical approach that Swartz wishes to run away from (but which re-appears in the case descriptions). Using political structure as a variable to define boundaries around a political system as

well as the skeleton of the system itself creates the basis of a general theory from which explanation and hypotheses can flow. By contrast the use of a "field" of political activities creates serious boundary problems (as Swartz admits) which make it a difficult unit of analysis. The criticism that structural approaches are not dynamic is refuted by the case studies—almost all of which deal with change and focus on the political structure in order to carry out the analysis. The most ambitious essay in the book (Bailey) attempts to create a theoretical environment in which various types of conflict are operating. In creating the model Bailey states categorically that he is using political structure as a dependent variable (p. 292). Another example of this denial, but affirmation by operation, is the chapter by Nicholas who sharply criticizes structuralism (p. 297–98) then proceeds to develop a flexible approach to the categorization of rules (the content of structure) which he then applies to Indian case material.

The only real exception to a structural analysis is the chapter by Hughes who writes a fascinating psychological analysis of Eskimo leadership. In it he develops further his transactional approach and introduces the concept of competence to explain various aspects of role behavior. Since the editor's use of Lewinian field theory depends by definition on "involvement" and "interest" i.e. on motivations, it would have been useful to have a more full-blown integration of Hughes' psychological theories fitted into the more general ideas in the introduction. In this vein it is interesting that Hughes, unlike the editor, does not criticize structural approaches as being wrong-headed. Instead, he performs the task of adding psychological depth to a structural feature, that of leadership.

What Professor Swartz and some of his colleagues wish to criticize and change can be labelled naive structural-functional analysis. In this approach the researcher describes the set of rules which are the traditional constitutional basis of political action for the actors, as if such rules were the sum total of all political behavior of the actors in these small societies. Then, as Radcliffe-Brown claimed, social process is assumed to be the functioning of structure. This produces a timeless, static, functionally integrated system common enough in the ethnographic literature, and probably non-existent in empirical reality. The criticism is well-taken, but as the essays themselves prove, naive structuralism is something of a dead issue. The question then becomes—should we tinker and adopt the structural approach to include the actual processes of political behavior, of which structure is one element, or should we look for an entirely new paradigm? As the editor notes (p. 2), I lean to the former view, while he spouses the latter, although as I have already noted, espousal and practice seem to differ—at least in this multi-authored volume.

In terms of the information presented, this collection is rich and varied. Only rarely if at all does the formally trained political scientist invest the time and energy in such far away places to obtain the kind of data presented in this volume. And yet this is the day-to-day stuff out of which politics is made in the new nations. Elites come and go; some have greater impact than others. But what that impact is and how the life of the nation is affected by the modern world is to be found in materials such as the excellent case studies in this book.—Ronald Cohen, *Northwestern University.*

The Japanese Socialist Party and Neutralism: A Study of a Political Party and Its Foreign Policy. By J. A. A. Stockwin. (New York: Cambridge University Press, 1968. Pp. 197. $6.50.)

Recent scholarship which has just begun to "explain" postwar Japanese politics rather than to merely outline its effects, has been well served by Dr. Stockwin's new study of Japanese Socialist foreign policy. It concentrates on one of the most important and divisive issues which has effected the Japanese Socialist party since the end of the Second World War. Neutralism in the sense of keeping Japan out of the cold war has had strong adherents among all the opposition parties ever since the question of independence from the occupation came up. It has taken the form of the desire to avoid military alliances or foreign bases in Japan, and to make peace treaties with both West and East including Communist China and the Soviet Union. Since such neutralism means greater independence from the United States, it also means a certain degree of anti-Americanism—at least in basic foreign policy orientations. Nor, is this feeling confined only to the opposition. Even among some major leaders in the dominant Liberal Democratic party today a desire for a more independent foreign policy permitting Japan to have better relations with its Communist neighbors is quite influential.

Although the book reviewed here is confined to the Japan Socialist party in general, it does not neglect to carefully consider the effects of the external environment of the relations of the major powers upon Japan as well as important changes in domestic politics. This has the effect of making the study of considerable value to anyone wishing to understand the most divisive issue which has beset Japan since World War II. It is the key issue influencing relations between it and the United States today.

To explain the genesis and changes in the neutralist policy, the book delves into the most basic processes at work within what is the leading oppo-

sition party in Japan—a party which looks increasingly as though it can not govern or replace the dominant Liberal Democrats for whom it supplies the principal competition and criticism in politics. It is especially interesting to note, as does the former Prime Minister Yoshida in his memoirs, that it was a coalition cabinet under the Socialist Prime Minister Katayama in 1947 which first conceived the notion of relying upon the United States for Japan's security. Yet, since 1949, when the NATO allies first seriously determined to obtain a peace treaty with Japan, the Socialist party has consistently opposed dependence upon the Western bloc for its security. This apparent *volte-face* is explained by the weakening through loss of Diet seats of the early dominant rightwing faction to which Mr. Katayama belonged and the subsequent ascendency of the more radical Marxist factions. The latters' doctrinaire anti-capitalism makes them anti-American and prefer non-aligned neutralist policies when these do not take a form which favors "socialist" countries like Communist China or the Soviet Union.

The Japanese Socialist party factions have long been noted for the extraordinary doctrinal consistency with which their leaders went back to early socialist parties organized in the 1920's for answers to questions about the nature of Japanese capitalism and the best means of overthrowing it. This consistency of doctrine was found in several streams identified with different leaders who survived the militarism and nationalism of the 1930's and 1940's. Some of these same men have led their factions in the parliamentary party till rather recently. Their rivalry for the major posts—particularly the secretary general or chairman—usually determined the direction of party policy. Since this struggle has taken the form of a rather rigid argument over points of socialist theory, the party has had a reputation for impracticality. This has not been tempered by the need to be ready to take office quickly because of the long continued disparity of votes and seats with those of the dominant parties.

Dr. Stockwin demonstrates, however, that beneath this intra-party maneuvering there is considerable awareness of conditions abroad and in Japan by the Socialist leaders who, after all, show considerable political sophistication in working within the peculiar framework of doctrine and group expectations which make up the party's own internal political process. He also believes the party shows increasing similarity to the less ideological conservative Liberal Democratic party whose factions are also mainly parliamentary party ones supported by personal ties to a few prominent local supporters but without any really significant mass organization. This presents an interesting contrast with the much smaller parliamentary groups of Communists and the Buddhist Soka Gakkai parties which have active local organizations vastly outnumbering the Socialists. The last named here, nevertheless, still managed to garner the major share of the opposition vote in Japan, although the ability to do so is shrinking perceptibly.

The Japanese party also contrasts with the Italian Socialist party which is compared in the last chapter. The European party shows an interesting similarity to the Japanese in foreign policy positions and in its factional alliances. The ability of Nenni to revitalize his party through appeal to the rank and file over the heads of its entrenched officials contrasts, however, with the unsuccessful Japanese party attempt under a leader like Eda to do the same in Japan where such local organized membership and voter loyalty is lacking.

It is striking in a wider context how the influence of more pragmatic Japanese Socialist leaders like Eda seems to rise with the tendency toward detente between the major powers in the Far East. The recent signs of a real Far Eastern "thaw" since the book was written has already given further evidence of this influence in the shaky but renewed appearance of Eda in the office of secretary general. The account of the often close but internally vexing relations of the party with the leaders of Communist China in recent years provides a particularly informative and valuable kind of reference material on the relations between external foreign contacts and internal intra-party competition.—FRANK LANGDON, *University of British Columbia.*

Angels in Marble: Working Class Conservatives in Urban England. BY ROBERT MCKENZIE AND ALLAN SILVER. (Chicago: The University of Chicago Press, 1968. Pp. 295. $11.00.)

At the outset of the era of mass electorates, Disraeli became the prophet of working class conservatism. As Prime Minister and leader of the Conservative Party he mobilized a reluctant parliamentary majority to support the Second Reform Bill (1867) which granted the franchise to the British working class. Disraeli successfully overcame grave Tory misgivings by arguing that the Conservative Party surely would benefit politically if it played the major role in transferring potential control of the machinery of government to the industrial masses. For its efforts in this "betrayal of the status quo" John Stuart Mill branded the Conservative Party—a party whose members consistently had been drawn from the upper strata of society and whose concerns had been to resist any basic transformation of the political power structure—as "the stupid party."

Time has demonstrated the accuracy of Disraeli's prediction. Since 1885 there have been fifteen elections in which one party won a clear majority, with the Conservatives winning eleven and the Liberal and Labour parties splitting the remaining four. In most of these cases, even in recent decades, "about one-third of working class voters for the two major parties have supported Conservative candidates." Thus, for nearly a century a sizeable portion of the British working class has been the guardian if not the guiding angel of the Conservative Party.

To discover why the urban working class has contributed so heavily to Conservative Party electoral strength, McKenzie and Silver combine three kinds of interpretation: historical, qualitative and quantitative. The historical perspective is provided in Chapter 2—"Conservatism and Its Appeal to the Working Class." Here the authors describe the Conservative Party's efforts to appeal to the working class on the basis of the party's record of initiating and/or improving laws affecting the working class's own self-interest. The central theme of this political communication has been the special ability of the Tories to govern the country and their competence in managing the economy for the benefit of the working class. These aspects of Conservative ideology have been more widely and successfully diffused than is suggested by the fact that only a third of the working class typically support Conservative candidates. While Labour voters readily endorse the Labour Party as the agent of the class interests, they also concede that the Conservatives are generally more qualified and competent to deal with most domestic and international policy domains. Working class Conservatives rarely accord such favorable judgments to the Labour Party. The singular attraction of Labour is "its reputation for concern for the common man . . ." Thus, the Conservative's historical claim of competence in governing appears to be widely acknowledged by the entire working class regardless of partisan preference.

The successful appeal by Conservative elites to the working class is the underlying theme of the remaining four chapters which constitute the core of the study. The analysis is based on data acquired in a four-phase, modified panel survey of urban workers employed in manual occupations. The interviews were conducted between 1958 and 1963 in what the authors term " 'non-electoral' situations." By minimizing short-term stimuli associated with political campaigns, McKenzie and Silver sought "to explore the enduring matrix of attitudes, values, and social characteristics which predispose voters to support one or another party"—a strategy which American studies might well adopt. They interpret their data with both skill and clarity. In terms of social characteristics, for example, working class Conservatives do not differ markedly from those who support the Labour Party (Chapter 3). Both groups also share many of the same social values and political perspectives (Chapter 4). But when Conservative voters are separated into "deferentials" (about a fourth of the Conservatives see the leaders of that party as the "natural rulers of Britain" and as the "sole source of whatever benefits are possible to attain") and "seculars" (better than a third of the Conservative voters evaluate their party on the more pragmatic basis of present performance and the prospects of future benefit), the groups do differ in their social characteristics (Chapter 5) and in their attitudes on social and political questions (Chapter 6). A final chapter summarizes the major findings in terms of the context, nature, and prospects of working class Conservatism in England.

In analyzing their data McKenzie and Silver detect changes in the social and ideological nature of working class conservatism. The social deference which has traditionally sustained the political preference of many working class conservative voters apparently is being displaced as the younger and better paid working class members gradually adopt a more secular political outlook. But how and why these changes are occurring is not fully explained. McKenzie and Silver are not sure how stable this change will be or how it will affect the partisan balance in the immediate future. They readily admit that the accuracy of their speculations are contingent on future political events. Another Labour victory, for example, "may greatly diminish the disposition of a considerable section of the working class to attribute unique government skills to the Conservatives." Likewise, a Labour defeat could reinforce working class deference toward the Conservative Party. In short, McKenzie and Silver conclude that British political alignments are in a state of flux and wisely conclude that future electoral outcomes will have consequences that are more far-reaching than simply determining who shall govern in the short-run.—EDWARD C. DREYER, *University of Missouri, St. Louis.*

Chinese Communism in Crisis: Maoism and the Cultural Revolution. By JACK GRAY AND PATRICK CAVENDISH. (New York: Frederick A. Praeger, Publishers, 1968. Pp. 279. $6.00.)

Both the general reader, for whom the book is primarily written, and the specialist are indebted to the authors for this excellent study of the Cultural Revolution. The specialist will find little new information, but this book will provide him with

new insights into the complexities of Chinese politics; moreover, the work will serve as a convenient reference for the leading events in this perplexing period in Chinese history.

For the reader with little background in modern Chinese history, the first chapter is indispensable for a proper understanding of the problems confronting the Chinese Communists upon their advent to power. In the following chapter the authors analyze in chronological order the policies adopted by the leaders of the CCP to cope with those problems. In retrospect, it is now clear that the conflicts at the top echelon of policy-making were far more intense than Western observers had suspected. Certainly a broad consensus did not exist regarding the adoption of the Commune system. If one can believe the allegations made by the Maoists against the "anit-Maoists," such party stalwarts as Liu Shao ch'i led the opposition forces. Since Gray and Cavendish contend that the fate of the Commune is the essential issue in the Cultural Revolution, they analyze in depth the "great debate" preceding the adoption of the decision to undertake the total mobilization of the manpower and womanpower of China.

The authors' treatment of the "Thought of Mao Tse-tung" is not entirely satisfactory. Their analysis of the concept of contradictions fails to convey to the general reader the actual meaning and the centrality of this concept in the Maoist world view. It is doubtful that such a reader could sense the virtual adulation of the "laws" of the dialectic demanded of the "95 per cent" who allegedly follow faithfully the teachings of Chairman Mao. The writers' contention that the idea of contradiction "compels to a habit of analysis" (p. 59) is also the dubious validity. Would it not be more accurate to say that the Maoists urge that the dialectic be used as a tool of analysis by those investigating problems in all realms of existence? In short, Communists *should* habitually analyze situations in light of the theory of contradictions. Nor can I accpet the authors' characterization of the concept as a "mnemonic formula" that was "useful to a political leadership composed of a mass of village cadres and former querrillas who are politically unsophisticated and mostly subliterate," but who may not, as they grow more literate, need this tool of analysis. How can Mao and his associates lay aside the theory without doing violence to the myth that the dialectic is the most dependable "weapon" of the Marxist confronted with any problem?

The assertion of the authors that Mao, unlike Stalin, has taken a consistent position regarding material incentives is subject to question. Was his stand in the early stage of the Commune period vis-a-vis the relative weight to be given "material" and "moral" incentives identical to that taken by him before the shift in economic policy? The addition of the free supply system to the system of material incentives was based on the assumption that the CCP was building socialism so rapidly that it must prepare immediately for the transition to communism. Mao, in the early stage of the Commune period, placed less stress on material incentives and more emphasis on moral incentives. Similarly, Mao, in his struggle with Cultural Revolution foes, is waging a relentless attack on those who place too much weight on material incentives.

The last two chapters dealing respectively with the cultural and political aspects of the crisis confronting the Chinese Communist leaders are doubtless those that will be of greatest interest to the general reader. The authors demonstrate most effectively that the Cultural Revolution as commonly understood was a continuation of campaigns begun in the early and mid 60's (the Socialist Education Campaign, for example). They are to be commended for their delineation of the issues and objects of attacks in these campaigns. Their explanation of the reasons underlying the attacks on certain plays, operas, and films is especially valuable. Equally impressive is the dispassionate manner in which they evaluate such acts of the Maoists as the formation of the Red Guard Movement as the principal instrument for smashing the "main" enemy, the bureaucracy. Those reading this well researched book will gain a clearer picture of the principal figures, trends, and issues involved in this most confusing period in Chinese history because the authors place them in the proper historical perspective.

Enhancing the value of the book is a documentary appendix consisting of representative literary texts and doctrinal and national policy texts. The collection would have been strengthened by the inclusion of the "three constantly read articles" ["Serve the People," "In Memory of Norman Bethune," and "The Follish Old Man Who Removed the Mountains"], all "required" reading in Mainland China.—CECIL JOHNSON, *Southern Methodist University.*

Black Intellectuals Come to Power. By IVAR OXAAL. (Cambridge, Massachusetts: Schenkman Publishing Company, Inc., 1968. Pp. 194. $7.95.)

Social Change and Images of the Future. By JAMES A. MAU. (Cambridge, Massachusetts: Schenkman Publishing Company, Inc., 1968. Pp. 145. $5.95.)

The Sociology of Political Independence. By CHARLES C. MOSKOS, JR. (Cambridge, Massachusetts: Schenkman Publishing Company, Inc. 1967. Pp. 120. $4.95.)

The publication of these studies of the British West Indies, based on field research completed during the early 1960's, essentially completes the major efforts of the U.C.L.A. West Indies Study Program. These books extend geographical coverage of the study of social and political development to the Caribbean. While the populations and territories discussed are small compared with other newly independent nations in Africa and Asia, the setting in no way diminishes the theoretical utility for social science of the substantive findings. Furthermore, these studies help fill a geographical void in our learnings about the "developing nations."

The respective modes of analysis and exposition differ. Oxaal, reflecting his journalistic background, provides an extremely well-written, entertaining (an adjective which could be profitably attached to more studies of politics), lucid description of the events leading to independence in the multiracial setting of Trinidad. The development of party politics—Dr. Eric Williams' Negro supported Peoples National Movement and the East Indian supported Democratic Labor Party now headed by Dr. Rudranath Capildeo—is a dominant theme. A lesson of ethnic politics is that numbers are supreme. The D. L. P. was defeated because the Indian population comprises only 36 per cent of the island's population.

Oxaal contributes to our understanding of the phenomena of social and political integration in multi-racial, developing societies. On page 178 he observes that "color tended to diminish in actual importance in determining mobility as educational opportunity and achievement increased over the years." Although color has declined as a social and occupational barrier in recent years, ethnic voting has increased. Therefore, Trinidad can be described as a society exhibiting politically disintegrative tendencies, even though social mobility and social integration has risen from much lower levels, particularly for dark-skinned Trinidadians who previously lived under a color bar.

In contrast with Oxaal's historical and descriptive style, both Mau and Moskos attempt systematic social science, basing their analyses mainly on the responses obtained from elite interviews. Mau's study is limited to Jamaican leaders and one mass group in Kingston, whereas Moskos interviewed leaders in Dominica, Barbados, Grenada, Guyana, Jamaica, and Trinidad. Both selected their elites using a combination of positional and reputational criteria. Persons in high positions were asked to identify influentials by reputation. Those named comprised the units of study.

Mau sought to analyze attitudes held by elites toward the various social, economic, and political changes which characterized Jamaica during the transition from colony to independent nation. Leaders who professed belief in egalitarian principles, exercised decision-making power and who were knowledgeable of the complaints and discontents of lower class West Kingston residents were generally optimistic about the prospects for progress. Furthermore, these leaders identified the masses as hostile in much smaller proportions than leaders less egalitarian, less powerful and less knowledgeable. Thus Mau concludes favorably for the future of Jamaica. More than half the leaders interviewed expressed belief in Jamaica's future potential for progressive change.

Moskos investigated the development of West Indian nationalism during the pre-independence period in the British Caribbean. His format first describes the elite structure in the West Indies followed by the results of a series of interviews held with West Indian leaders. A typology of nationalist leaders was constructed (colonialist, acquiescing nationalists, and true nationalists) and correlated with (1) standard background characteristics (sex, age, education, color and ethnicity, personal wealth, life-style, and institutional sector; and (2) the economic, political and social views of these leaders.

Moskos presents several substantive findings: (1) West Indian leaders are better educated, wealthier and possess life-styles markedly different from that of the general population; (2) nationalism is a function of territorial size, i.e., leaders in smaller islands are less nationalistic; (3) colonialist types are in economically dominant positions and often white or near-white, while true nationalists are younger and more provincial West Indian middle class; (4) true nationalists express "left" economic ideologies, colonials profess "right" economic ideologies, while the acquiescing nationalists admit to intermediate economic ideologies; and (5) nationalist movements in the West Indies exhibit properties comparable with movements which have occurred in Western Europe.

Methodologically, Mau and Moskos suffer from inadequate samples. Mau's analysis of Jamaica is based on 54 interviews and Moskos relies on 112 total interviews from six different territories. The small samples restrict the statistical analysis to an almost uninteresting simple distribution of percentages, which often lack meaningful theoretical motivation. Controls are not applied to the data since the *n's* in the cells would quickly diminish to zero. For instance, Table 22 in Mau's book gives a percentage breakdown of knowledgeable leaders by type of elite position even though the number of cases on which the per cent is based ranges from 8 to 13. A change in the responses of only a few leaders could reverse the findings. Thus, the reliability and utility of most of the tables is sharply diminished. Furthermore, tests of statisti-

cal significance are not reported, nor is an explanation provided to account for their absence. Similar criticisms apply to Moskos.

One may question whether it is fair for Moskos to combine his 112 respondents for any analytic purposes. His findings that leaders living in larger territories tended to be more nationalistic than those in smaller territories may be inconsistent with assessing the correlates of nationalism for all leaders taken together without a control for territorial size.

Methodological shortcomings may prejudice the acceptability of the substantive findings reported in the latter two elite studies. However, the truth and generality of the findings are in principle testable in other locations and, properly verified, may aid our theorizing about nationalism and attitudes held by elites in newly independent nations.

It should be noted that these books in the Schenkman series are illustrated with splendid photographs and are reasonably priced. This series of reports provides an important beginning for the study of Caribbean political development.—ALVIN RABUSHKA, *University of Rochester.*

Kassenärzte und Krankenversicherungsreform: Zu einer Theorie der Statuspolitik. By FRIEDER NASCHOLD. (Freiburg i/B: Rombach, 1967. Pp. 288.)

A German student of interest group politics is confronted with a greater variety of problems than his American colleague: the prevalent idea that pressure politics is dirty, illegal or even subversive; the relative scarcity of empirical studies; the legalistic tradition that informs much of West German political science; and the need to import "Anglo-American" group theories as models for German case studies.

In light of these problems, the present book on medical politics is a considerable scholarly achievement. It deals with one segment of organized medicine: the health insurance physician and his main organization, the KBV; his attitudes toward statutory provisions; and his perception of the physician's predicament in a pluralistic welfare state. It discusses the relationship between the KBV and other medical associations; the KBV's decision-making processes and oligarchical leadership; and its access to the political authorities.

Organized medicine cannot be treated in the same way as workers' or employers' groups: the latter operate more or less autonomously, engaging in "political" activities to promote essentially economic goals; while the former is basically professional in its aims, and hence apolitical. Such a view of the medical profession, which the author supports with (not entirely convincing) statistics on comparative professional attitudes, precludes an analysis of medical politics in terms of marxist theory or bread-and-butter aspirations, and explains why he concentrates on status politics. The author admits that status politics is elusive, incoherent and often ineffectual, owing to unrealistic and conflicting ideologies and status perceptions, including the "estate consciousness" of the profession; the glorification of the traditional GP; the view of the physician as scientist, public servant or entrepreneur; and, finally, the flight into "the ideal of the essence of being a physician" (*das eigentliche Arztsein*)—whatever that may mean. The progressive nationalization of medical care has led to a transformation of the status and role expectations of the doctor. The fee system and the doctor-patient relationship have become dependent on the dynamics of statutory health care programs and on the general economic situation. The physician in "socialized medicine" is no longer free, nor is his status based on professional mystique. This dependence has, of course, not been entirely unwelcome: physicians want to be institutionalized not only because rendering a public service appeals to their sense of professionalism, but also because they gain a secure clientele and a guaranteed income.

For a long time, the medical profession remained politically passive, because its conflicting ideologies and organizational fragmentation weakened its cohesion and because the professional self-image required an apolitical stance. All this changed dramatically in the late 1950's: the health insurance reform bill, which envisaged an administratively determined fee schedule, threatened to transform the physician into a minor bureaucrat. The threat to medical apoliticism resulted in "status polarization" which transformed the profession into a cohesive force and enabled it to employ "political" means. But the physician can have his cake and eat it: he maintains his apolitical pose but transfers political responsibility to the organizational physicians "who are less socialized on the basis of medical norms." Ultimately all doctors must become political, in order to counter any acute threats to their apolitical status.

Unfortunately, the author tends to think of "political means" in terms of parliaments, electorates and parties. If politics is construed more broadly, the medical profession has always been political, despite its argument that its intervention with the bureaucracy and similar activities prior to "status polarization" merely constituted an exchange of objective information.

A more serious problem is the author's rejection of the financial in favor of the "status" hypothesis. While he admits that the privileged status and the entrepreneurialism of many doctors have been "dysfunctional with respect to the health care re-

quirements of society as a whole," he does not conclude that doctors are as interested in exerting pressure for monetary gain as other groups; he does not adequately analyze the medical associations' quest for the retention of bargaining powers, or their interest in keeping the health insurance funds fragmented to avoid being confronted with a united opponent; or the demand of specialists for better fees than those received by GP's. The author does admit that there has been "a decline of the spiritual outlook of the doctors," but he attributes this not to the profession's cupidity but rahter to "the dominance of materialistic attitudes in man." Had money been an important consideration—so the author argues—the physicians would have favored, in the early postwar period when medical pay was relatively low, a *Verbeamtung,* i.e. civil service status. But then civil servants were paid badly, too. Moreover, if the preference of status to income were so compelling, the doctors should have welcomed an incorporation into the bureaucracy, since status and psychic income have often been more important to bureaucrats than real economic advantages. In fact, the health insurance physicians have sometimes found "governmentalization" to their advantage, because public-law status has conferred the label of legitimacy and objectivity upon their demands.

One may also question the author's thesis that the threat to the doctor's image produced an effective status polarization and cohesion. The internecine fights between medical associations continued at the height of the medical reform struggle; one medical group brought suit against another, and new medical associations fought each other over fee distribution. Furthermore, one may question the author's contention that the status politics of the physicians represents a deviant case. The white collar sector has long striven to preserve its status and organizational identity despite the economic disadvantages it has thereby encountered.

There are a few minor shortcomings: no adequate discussion of the linkage between the KBV and the federal medical chamber, of the importance of medical journals in politicizing the doctor, or of the dynamics of medical pressure on the political decision-makers. The many complex details about the development of medical statutes, organization, and attitudes are impressive, but despite the marshalling of group and status theories—which precedes the factual analysis without necessarily clarifying it—medical politics is treated as a case *sui generis,* and no general principles emerge with respect to West German group politics. What makes it difficult for the reader to follow the book is its organization: it is not divided into chapters but rather scores of numbered, often brief, paragraphs—as in a legal text; the author switches back and forth in his analysis, which remains indicative rather than fully developed. Finally, there is no index. Hopefully, however, this book will contribute useful raw material for future attempts at generalization about interest groups.
—WILLIAM SAFRAN, *University of Colorado.*

Constructive Change in Latin America. ED. BY COLE BLASIER. (Pittsburgh: University of Pittsburgh, 1968. Pp. 243. $7.50.)

The French have an appropriate term for describing this sort of non-book: *macédoine,* "a work composed of diverse pieces forming a disparate whole." Beyond their common regional focus and the fact that they were all presented to the same faculty seminar at the University of Pittsburgh, there is little the editor can use to justify the joint publication of these original essays. Prudently, he leaves the provocative title concept, "constructive change," undefined. Unconvincingly, he argues that all the authors share a common preoccupation with the lot of the lower classes and with the need for consensus to accompany social and economic transformations.

Multi- or Interdisciplinary relevance is the usual justification for such a compilation. The seven authors do represent a variety of approaches and disciplines: anthropology, economics, sociology, political science and literature—but each stays strictly within the confines of his professional specialization. Merely juxtagposing their isolated efforts, however competent, does little to meet the stated objective of overcoming "the excessive compartmentalization in teaching and research that has handicapped the development of Latin American studies and knowledge in general." Rather, it underscores its persistence.

One of the more unfortunate consequences of a *macé-doine* is that, amid the disparity in quality and diffuseness of scope, essays of major significance get overlooked. In this format they are initially less widely distributed than a well-placed journal article and subsequently less likely to be reprinted. The essay by Fernando Henrique Cardoso and Josá Luis Reyna, "Industrialization, Occupational Structure and Social Stratification in Latin America" is, I believe, of such stature. The two authors, one a Brazilian, the other a Mexican sociologist, but both formerly associated with the Latin American Institute of Economic and Social Planning (ILPES) in Santiago de Chile, take on a major and hiterto unexamined issue: Is the development process in Latin America repeating the same basic patterns of structural change which characterized the evolution of today's older industrialized societies? Their answer is a qualified negative. By extensive resort to aggregate data on the

major countries of the area, Cardoso and Reyna document a number of significant deviations from the Western Europe-North American pattern: (1) Urban growth precedes and far surpasses that of industrial growth; (2) While movement out of agricultural pursuits parallels that of the "early take-off countries", proportional employment in industry is much lower and shows marked signs of stagnation; (3) The great absorber of newly mobilized urban manpower is the tertiary sector—an omnibus category which disguises the existence of a growing proportion of economic marginals. The resulting configuration is paradoxical: on the one hand: the gradual formation, increase and consolidation of a proletariat and other modern status groups based on relatively open, performance-oriented criteria and values; on the other hand, the more accelerated growth of undifferentiated, underemployed and unassimilated urban masses, "recruited" by geographic mobility and rapid demographic growth and only latently "available" for participation in the values and institutions of a modern industrial society. This configuration differs markedly from that of industrializing Europe and while its bloated tertiary resembles that of contemporary post-industrial societies, it lacks the latter's high level of productivity. From this stratification model, the authors launch into some speculative propositions about elite responsiveness and persistence, social mobility, class conflict, fragmentation within lower strata and center-periphery relations. While the authors' imagination clearly outruns their data—itself of questionable reliability and comparability—their essay represents a major effort at understanding the Latin American development process in a critical, but nonpolemic Marxist perspective.

The remaining six essays are of lesser originality and general relevance. John Gillin discusses the values of the lower classes in a rather vague and quite undocumented sequel to his earlier and much reprinted essay on the middle class ethos. John Powelson takes a long time to make a simple *Hirschmanesque* point about the ECLA doctrine: that ideologically deformed, empirically unfounded (in his opinion) economic analysis may result indirectly in more appropriate and effective policy sequences than objective, correct (i.e., North American) economic analysis. Both place great emphasis on values and the need for consensus over them, but in their compartmentalized way seem unaware of the survey research work of scholars such as Joseph Kahl on Brazil and Gabriel Almond and Sidney Verba on Mexico.

Two essays deal with the perennial issues of private foreign capital and the Alliance for Progress. Neither contributes anything new to the discussion, conceptually or substantively. Richard Thorn's summary of the fortunes of the Alliance for Progress, for example, starts like a State Department press release, switches to the hard-hitting, disjointedly factual style of the Annual Reports of the Social Progress Trust Fund and concludes with the usual lament over the lack of a political will to implement reforms at the national level, a plea for more effective salesmanship on the part of intellectuals and a call for the establishment of a Great Power Executive Committee with United States participation to guide regional integration!

Germán Arciniegas provides an erudite—if controversial—reminder that students of Latin American politics stand to learn as much from the area's literature, "a humanized form of polemics," as from "Marxist babblers (who) have picked up the phrase, 'structural change' and are applying it with rigor as an easy answer to any solution."

In the final essay, James Malloy draws a concise and objective political history of the Bolivian revolutionary experience, but cast in a conceptual framework clearly intended to provoke comparison. He orients his account around a number of interesting working propositions about both the pre-revolutionary dilemma and the post-revolutionary process of reconsolidating central authoritative control. Unfortunately, in this short format, the exposition is much longer on inference than systematically compiled evidence. It does, however, set a promising standard for the fully-documented study of which it is a résumé.—PHILIPPE SCHMITTER, *University of Chicago.*

Communism in Malaysia and Singapore: A Contemporary Survey. By JUSTUS M. VAN DER KROEF. (The Hague: Martinus Nijhoff, 1967. Pp. 268. $11.30.)

This is a highly informative and insightful survey of Communist organizations and activities in Malaysia and Singapore with primary emphasis on the 1963-67 period. After a succinct discussion of various stages of Communist activity in this area, the author analyzes specific problems and prospects in each territorial unit—Singapore, Malaya, Sarawak, Sabah and Brunei. Special attention is paid to the pattern of Communist tactics applied in the processes of formation and disintegration of the Malaysian Federation. And the author evaluates the consequences of Indonesia's erstwhile "confrontation" campaign against Malaysia and of Communist China's policies in Southeast Asia.

The fact that Communism in the Singapore-Malaysian area is almost entirely a Chinese phenomenon is deeply rooted in the persistent ethnic and cultural identity of Chinese groups. Although they enjoy commanding economic positions and high educational development, a variety of their

communal grievances, coupled with their chauvinistic outlooks, become a fertile soil for Communist appeals and operations. In Malaya the Chinese, as "second class" citizens, suffered most from discriminatory measures in such fields as citizenship regulations, business licenses, governmental recruitment, language requirements and public assistance. In recent years Malaysia and Thailand concluded a frontier defense agreement and conducted the joint patrols to suppress the growing Communist guerilla activities along the border, given an impetus by a combination of Chinese communalism and ultra-orthodox Islamic currents. The Malayan Communist Party (MCP) fully exploited the Chinese grievances to maximize its revolutionary objectives and to further radicalize political dynamics along communal lines. Yet the heightened sense of communal differences tended to militate against ideological ties. Among leftwing Malays and Chinese, therefore, communal suspicions and tensions undermined common ideological commitments and destroyed a number of MPC's united front organizations and movements. In this sense, the author concludes, communalism is not only Malaysian Communism's greatest asset but also a major barrier to its success.

The fusion of Communism and Chinese communal interests is particularly effective in Sarawak. The so-called Clandestine Communist Organization (CCO), most significantly identified with Mao-tung's political doctrines, recruited a considerable number of young Chinese radicals and made a substantial penetration into Chinese communities. While the CCO and its front complex advocated complete national independence for Sarawak and instigated subversive activities, the Goverernment arbitrarily arrested suspects and resettled some Chinese into a more controllable area. This intensified atmosphere increased the possibility of a Communist insurgency of major proportions. In Sabah where Chinese radicalism was less evident than elsewhere, Communist activities were largely an appendix of CCO operations. But Chinese status ambiguities and grievances in Sabah contained a seedbed for political radicalism. In Brunei, however, A. M. Azahari, in apparent cooperation with the MP, opposed the Malaysian Federation, advanced the notion of a unified North Borneo state comprising Brunei, Sabah and Sarawak, and led an unsuccessful armed revolt.

The People's Action Party (PAP) in Singapore was once bipolarized between the moderate non-Communist faction led by Lee Kuan Yew and the radical Communist-oriented and communally minded faction influenced by the MCP. One reason for Tunku Abdul Rahman's acceptance of Singapore into the Malaysian Federation was to make the split of PAP complete and to contain MCP's activities in the island state. The PAP's leftwing faction established the *Barisan Sosialis* (Socialist Front) Party and appealed to labor unions, militant student groups at Nanyang University and chauvinistic Chinese communities. In the face of the Malaysian Government's suppressive measures, Singapore Communists adopted a militant strategy along the line of Peking's "revolutionary hard-line" and in conjunction with Djakarta's "saboteurs and guerillas." Although the author alludes to the MCP's control over the PAP's leftwing and later over the Barisan, he fails to clarify the extent of the MCP's control, either organizational or financial. Moreover, such crucial aspects of the MCP and the Barisan as internal organizations, ideological assertions and leadership techniques are left unclear. The term "front organization" is not defined in this book and thus vulnerable to abuse.

While the author often makes references to Peking's "militancy" and "expansionism" in the Singapore-Malaysian area, nowhere does he attempt a satisfactory examination of Communist China's strategies and tactics regarding this area. Unless such an explanation is given, it is difficult to accept that overt and organized support for the anti-Malaysian movement shifted from Djakarta to Peking in the wake of the abortive PKI-sponsored coup of September 1965 or that opposition to Malaysia was for Peking a kind of Asian test case of its ideological and tactical differences with the Soviet Union. And Professor Justus M. van der Kroef implies that a profound communal divisiveness, especially the basic clash of Malay and Chinese political cultures, continues to characterize Singapore-Malaysian political life and to promote the interest of Communist movement. But it remains to be seen whether most overseas Chinese will perpetually preserve a parochial and chauvinistic loyalty toward their ancestral homeland or will gradually develop a significant sence of attachment to their adopted countries.—CHAE-JIN LEE, *University of Kansas.*

Agrarian Reform in Colombia. By ERNEST A. DUFF. (New York: Frederick A. Praeger, 1968. Pp. 240. $14.00.)

The test of any case study is not only its validity but its relevance to an issue of general significance. Ernest Duff's study of Colombia's agrarian reform fairly well succeeds according to these criteria. In relating land reform to political processes, this book may serve as an admirable basis for other ones concerned with the politics of reform.

Chronologically and topically, Duff correctly distinguishes the legislation of land reform from its implementation. Although a number of prece-

dents nourished them, land reform measures enacted in Colombia in 1961 were facilitated by a unique constitutional arrangement (the *Frente Nacional,* which alternates executive power between Colombia's major parties), international pressures (most especially, the examples of other Latin countries, and growing American awareness of the issue), and "reformmongering" by able and principled politicians earlier described in Hirschman's book, *Journeys Toward Progress.* Once enacted, however, new issues arose—funding, administrating, staffing, and developing support for the enterprise. Duff praises the country's current president, Carlos Lleras Restrepo, for skillfully maneuvering the bill through Congress. He is also properly impressed by the imaginative activism of the Agrarian Reform Institute's first General Manager, Dr. Enrique Penalosa. But, by terminating his study in 1967, Duff unfortunately relates only an incomplete story. Has the Land Reform Institute overcome financial, technical, and political obstacles to become a viable institution within the framework of public and private agencies active in Colombian agricultural policy making? More time is needed for a definitive answer.

Perhaps this book's principal value to those concerned with political development is its simple but careful elaboration of alternatives within the rubric, "land reform." Many who earlier wrote about land reform were obsessed by the general theme and explained it as merely an "either/or" confrontation between large landholders and peasants. Duff devotes considerable detail to the political significance of expropriation and distribution procedures, colonization, marketing arrangements, credit programs, infrastructure projects, extension work, etc. The reader will gain considerably from Duff's first-hand knowledge of many earlier projects. He will, in sum, come to appreciate the complexity of the issue in Colombia and elsewhere.

Yet, the reader will not come away fully satisfied. Despite careful documentation, Duff relies too much on published material which often over-states particular views. Unfortunately, Duff does not construct a political basis (one which might relate demands and supports within the system) to consider the activities of various affected agencies. Repetitions of party positions contrast with the scarce information on interest groups—the army, landholders' associations, regional development corporations, peasant groups, etc. There is, for example, no discussion of how the Society of Colombian Agriculturalists has attempted to broaden its clientele in order to defend key interests. Reliable information on interest articulators in the context of any reform movement is, of course, as difficult to find as it is valuable to uncover.

Duff notes how neighboring Venezuela's reform has surpassed Colombia's, partly at least because of the way that country's political parties mobilized peasant support. Why haven't reform-minded Colombian politicians profited more from this experience? Could it be because of the "oligarchy" which some believe rules without competition in Colombia? Or might this failure be attributed to the lack of active political cadres within the reformist parties, the peculiar apathy of the Colombian peasant, or the legacy of *la violencia?* These questions are vital ones since their answers will suggest much about the hopes for reform not only in Colombia but in other countries as well. From the experience he has gained in writing this book, one can reasonably expect that Ernest Duff will ably explore them in the future.—WILLIAM J. BRISK, *University of New Mexico.*

The Emergence of Indian Nationalism: Competition and Collaboration in the Later Nineteenth Century. By ANIL SEAL. (New York: Cambridge University Press, 1968. Pp. 416. $11.50.)

At first glance, it may seem inappropriate that an historical work like Seal's should be reviewed in this journal and presumtuous of a political scientist to undertake the task. However, the author himself foreswears "spiritless historiography which clings to the particular and eschews the general" and sees in Indian history broader patterns of the encounter between European imperialism and colonial nationalism. For instance, he asserts that "all over the world, nationalist leaders have been formed out of government subordinates, men who put their working hours to good use in studying the mechanics of power, and devoted their leisure to studying the obstacles barring their own promotion." Furthermore, he employs some of the current jargon of political development such as "political mobilization" and displays the redundancy of statistical tables now expected in behavioral science. His chapter titles such as "The Political Arithmetic of the Presidencies," "The Rewards of Education" and "The Politics of the Associations" are also topical rather than chronological. Therefore it is fair to appraise his work as social science.

The general problem Seal sets for himself is how and why modern politics began in India when they did. "Modern" is never defined except tautologically in terms of the associations formed by the western-educated. His thesis is that the new elite created by the British to man the lower rungs of the imperial bureaucracy entered into a contingent collaboration with the foreing rulers for the sake of caste or communal advantage over other Indians, but then turned into critics when the demand for government jobs outran the supply. The uneven timing of development (and in this he sees the essence of historical analysis) be-

tween the coastal and interior regions of India provided the British with a series of collaborators as each new wave of horizontal and vertical mobilization intensified competition. Once alienated, the western-educated elite was more dangerous to the rulers than traditional foes because it could at least claim to represent all of India and could imitate more effective British modes of political debate and organization. They even invaded British domestic politics.

Seal's analysis is a kind of elitist-materialist determinism wihtout economic social classes. He bases this position on a controversial denial that British rule revitalized the Indian economy. What economic growth there was "was not sufficient to give Indian social classes based on economic categories." Instead, bureaucratic institutional change created the new elite drawn from the high status, but low income castes and therefore unlike the modernizing bourgeoisie of Europe.

The author's theses are not altogether novel except as applied to the communal split between Hindus and Muslims which ultimately led to Partition and the creation of Pakistan. He argues that the Muslim community despite its formal adherence to the ideal of Islamic brotherhood was no more homogeneous than the Hindus. Far from being backward and oppressed throughout India as those of its leaders claimed who collaborated with the British in order to obtain "reservations" in the civil service, the Muslims of Upper India were relatively better off educationally and in proportion of jobs held than the Hindu majority. Where the Muslim minority was disadvantaged, as in Bengal, this was due to poverty rather than religious prejudice as often alleged. Misrepresentation of the true Muslim condition he blames on an influential work, *The Indian Musalmans* by Sir William Hunter which, though culled from the context of Bengal, was the basis for many of the polemics on behalf of the two-nation theory (one Hindu, one Muslim). Seal disagrees with both the Indian Nationalists and the proponents of Pakistan by concluding that there was no nation at all in the subcontinent in the nineteenth century. While the tone of his presentation is determinist, implying the inevitability of the outcome (Independence and Partition), in a few places he hints that the communal split could have been avoided at this turning point of Indian history (1885) if Hindus and Muslims in the backward regions had recognized that they had more interests in common than with their co-religionists on the coasts. What he fails to stress is that Muslims in general were less inclined to be adaptable to western ways in the crucial decades because it was they who had lost most of India to the British.

The author of this volume displays immense scholarship of a kind many of our political science experts on political development avoid in favor of baseless and sweeping generalization. Seal asks the right questions, but doesn't quite push his conclusions to the level where they would lend themselves to comparisons with other cases of political mobilization in colonial areas. This suggests the need for a more fruitful partnership between the two disciplines informulating and executing such projects.—THEODORE P. WRIGHT, JR., *State University of New York at Albany.*

Urban Government for Zagreb, Yugoslavia. BY EUGEN PUSIĆ AND ANNMARIE HAUCK WALSH. (New York: Frederick A. Praeger, 1968. Pp. 151. $10.00.)

Yugoslavia—A Multinational State. BY JACK C. FISHER. (San Francisco: Chandler Publishing Company, 1966. Pp. 244. $8.00.)

The two books under consideration here, although they both concern themselves with Yugoslavia, are quite different both in approach and in effectiveness of analysis. The one (Pusic and Walsh, *Urban Government for Zagreb, Yugoslavia*) comes off as a quite routine, if not pedestrian, descriptive case-study of municipal-commune government in Yugoslavia. The other book (Fisher, *Yugoslavia—A Multinational State*) represents a solid attempt at a new and useful application of factor analysis to the well-documented phenomenon of regional variation in Yugoslav society.

The book by Pusic and Walsh is one in a series of case-studies of urban government under the auspices of the Institute of Public Administration in New York that includes municipalities of various countries. The authors open with an introductory chapter outlining the basic environmental contours of Zagreb; this is followed by a discussion of the institutions of government in their formal and structural aspect; then we have a chapter on inter-governmental relations that analyzes the different levels of administration; there is a chapter dealing with the process of social and economic planning in Zagreb; and, finally, a chapter is appended that describes assorted public service functions performed in the city. On the whole, the authors' style strikes me as plodding, quite unimaginative, with a high redundancy of terminology, a style not untypical of much writing in the field of municipal administration. One has to be either very interested specifically in Zagreb or in the details of municipal government to be stimulated, moved intellectually, or absorbed by the material that the reader is expected to digest. Despite these qualifications as to presentation and style, the Pusic and Walsh volume affords a very adequate, and indeed convenient, manual-type description of the functioning of municipal government in Yugoslavia. The book also affords insights into the spe-

cial nature of Yugoslav political institutions that derive from the Yugoslav innovations of decentralization and social self-administration (e.g., one conclusion points to the wide range of citizen participation in Zagreb as compared to the cities studied in other countries). On balance, I would recommend this book to anyone seeking a focused, straight-forward, and concise descriptive summary of local governmental institutions in Yugoslav society. In other words, this case-study treatment of Zagreb affords us a convenient handbook outlining basic aspects of local government (i.e., the commune, the municipality, the council system, etc.) that characterize the Yugoslav system of decentralized authority. For this reason, if not for its depth of analysis, the book represents a quite worthwhile and respectable piece of scholarly work.

The second book under review here by Fisher represents an impressive attempt to examine regional variation (and, more specifically, urbanization) by the application of contemporary quantitative methods. The author begins with two chapters that define the landscape for the reader in terms of broad societal-ecological features and historical aspects of Yugoslav society; then a chapter follows that presents some general indicators of economic growth and a factor analysis based upon aggregate variables for fifty-five Yugoslav cities to allow for comparisons of levels of development across the country; the next chapter focuses upon housing policy and housing conditions that exist in Yugoslavia, a chapter that fits a bit uncomfortably within the broader concerns of regional variation and economic growth; and, finally, the author devotes a chapter to the commune system which contains both descriptive materials and a factor analysis used to compare varying development-levels across Yugoslav communes. A word might be added here as regards the development of the volume—namely, somehow the assorted parts fail to mesh with one another as well as they might. In part this reflects the fact that Prof. Fisher has drawn materials for his book from several research tasks that he had completed earlier. Accordingly, he resurrects some of his descriptive work on Yugoslav communes that appeared a number of years ago in *World Politics,* and he also includes materials from a more specialized study of housing problems in Yugoslavia that has occupied him more recently. Prof. Fisher combines these two research efforts with his innovative application of factor analysis to aspects of Yugoslav regional development, and then rounds out the volume with two impressive introductory chapters to put more narrowly focused aspects of the book into perspective. The piecing together of a volume in this way (although commonly done) always leaves its effects upon the flow of the argument and the organization of the finished product. As a result, the Fisher volume comes off as being a bit disjointed, although none the less worthwhile and effective in its many other aspects.

In a more positive vein, the attempt by Prof. Fisher to apply factor analysis as a means of getting at regional variation by using aggregate indicators (e.g., age-groups, income-patterns, occupational structure, etc.) is the most interesting and most academically profitable aspect of the book. This use of factor analysis is not only appropriate but also imaginatively conceived, while being suggestive as regards a number of theoretical issues of development that go beyond the specific application of Yugoslavia. There are in fact two separate applications of factor analysis in the book—the one to the sample of fifty-five Yugoslav cities, and the other to the full array of communes in the country. Both are instructive and analytically informative standing alone and could easily be so justified; however, the relationship between the *different* factors that are derived from the two distinct operations is not made clear by the author. In addition, Prof. Fisher fails to indicate why different (although related) sets of variables are used in two separate instances of factor analysis, and what relevance this might have for his more general study of Yugoslav developmental and regional variation. It would have been useful if Prof. Fisher had defined in more explicit terms the exact nature of both the methodological and the theoretical relationship between these two applications of the factor analytic technique. Despite these reservations, I found that the use of factor analysis by Prof. Fisher as a means of ranking and defining communes along a broadly developmental (and, more narrowly, urbanization) spectrum to be both a fascinating and a fruitful application of the method. On balance, the merit of Prof. Fisher's work is reflected in the attempt to employ a new method of analysis to an essentially old and well-known problem that plagues the Yugoslav scene—namely, its severe and dramatic regional variation. And, it seems to me, that the author has indeed been quite successful in this application to the Yugoslav setting.

In general, Prof. Fisher concludes that a basically north-to-south regional variation exists in Yugoslavia and that, for example, one finds more urbanization in Slovenia, Croatia, and north Serbia than in other parts of the country. Although this hardly represents a startling finding as regards Yugoslavia, the fact that Prof. Fisher has given this a precise quantitative expression (i.e., he provides a concrete, operational measure of regional variation) does suggest that now "old" information is made available in more acceptable and

perhaps more usable form given current trends in soical science. As a result, in my view, this would represent an important step in the direction of improving our fund of knowledge about both Yugoslavia and the problems of economic growth more generally. I might add as a footnote that the large number of maps that Prof. Fisher has included in the book are also very useful and contribute importantly to the value of his work. In concluding, let me say that Prof. Fisher, in providing us with new data and expanded knowledge about regional variation in Yugoslavia, has made a significant contribution to the existing body of literature on Yugoslav society.—M. GEORGE ZANINOVICH, *University of Oregon.*

Russian Peasants and Soviet Power: A Study of Collectivization. By M. LEWIN. Trans. by Irene Nove. (Evanston: Northwestern University Press, 1968. Pp. 539. $11.50)

One of the key periods of Soviet history in general and the evolution of the Soviet political system in particular—up to now very much neglected by Western scholarship—is the period of acute crisis which followed the initial successes of Lenin's New Economic Policy. It was during this period that the Soviet leadership found itself at an important crossroads in the development of the "new Russia" and, being in disagreement over basic policy questions, could not agree on the directions.

Lenin had envisaged NEP as a phase in Russia's development toward communism which would last a long time. When in the years after 1927 the Bolshevik leadership saw itself confronted with the "accursed" problem of the relationship between town and country (or more pointedly: between the Soviet regime and the peasantry!), Bukharin advocated the continuation of more or less pure NEP measures; the exiled Left, while wanting to retain a market economy, favored rapid industrialization; Stalin, who by this time was in control, decided to solve the crisis—epitomized in the grain procurement problem—by ending NEP.

Lewin's study, spanning the years 1925–1930, was originally published in French as *La Paysannerie et Pouvoir Soviétique* (Mouton, 1966). It is the first full-length treatment in a Western language of this crucial period in Soviet history and a major contribution to our understanding of the events which led to the decision to transform the Russian *muzhik* (peasant) into a Soviet collective farmer, and to achieve this transformation, as well as rapid industrialization, at breakneck speed.

Dr. Lewin brings to his task not only meticulous scholarship and a rare mastery of sources, but also valuable personal insights and a unique feeling for his subject. The value of his study is enhanced by his own personal experiences in the USSR during 1940–1946, i.e., only 10–12 years after the events which he describes and analyzes in his book.

In the first part of his book, primarily devoted to the years 1925–1927, the author deals with the factors which led to the breakdown of NEP. He presents a lucid analysis of class stratification within the peasantry and a detailed account of the enormous difficulties encountered by the Soviet regime in its attempt to extend "Soviet power" to the countryside. Lewin argues that the heterogeneous character of the peasantry presented both theoretical and practical problems for the Soviet regime, whose initial policies were predicated on the assumption that the peasants could be readily classified into three categories, *viz.*, poor (*bedniaks*), middle (*seredniaks*), and well-to-do peasants (*kulaks*). The present volume leads to the conclusion that the ideological distinction made by the Soviet regime (and by Soviet historians) between the various segments of the peasantry concealed the real issue: the fact that the major obstacles confronting the Soviet regime in rural Russia could not be reduced to the existence of the *kulak* class, but involved the middle peasants, i.e., the vast majority. Lewin's subsequent analysis shows how Stalin turned the very vagueness of the concepts of *kulak* and *seredniak* into a political advantage. In fact, the author argues that for Stalin the terms *kulak* and *muzhik* came to be identical (p. 252).

In part II of the book the author interprets the years 1928–1929 as a kind of interlude during which the regime failed to develop a coherent long-range economic policy and attempted instead to deal with the procurement crisis of 1928 and subsequent problems through "measures of a partial and provisional nature." (p. 267). This is the heart of the book: a thorough and meticulously researched account of the events which "drove the Communist Party leadership to embark upon mass collectivization."

Part III deals with the "great turning point" of 1929—the "spinal year" (Krzizhanovsky's phrase!) of the first Five Year Plan—and with the Party's attempt to achieve collectivization on a mass scale through "dekulakization" in order to ensure the first "Bolshevik spring," as Molotov had put it in November of 1929.

Lewin suggests that the attack on the *kulaks,* which resulted in an outbreak of mass violence and brought the country to the brink of civil war, was motivated by the desire of the central leadership to coerce the middle peasant into believing that joining the collective farm was the only remaining alternative. Any opposition to collectivization and Soviet policy in the countryside in general came to be regarded as *kulak* activity" and was dealt with accordingly.

In the author's view, Stalin's decision to embark upon forced collectivization, coupled with a policy of continued rapid industrialization, took shape during 1928 and was dictated by an ever-growing economic crisis and by his maneuvers in the factional struggle, as well as by his personality. (The latter point is not well developed.) While Lewin may be right in arguing that collectivization "was not the outcome of long premeditation in the doctrinal sense" (p. 163), Stalin's political maneuvers during 1928 suggest that he probably had decided on his subsequent course of action as early as 1927—before the 15th Party Congress in December of that year. In fact, an examination of Stalin's views of the peasantry before 1927 reveals an extremely negative attitude (cf., e.g., Stalin, *Sochineniia*, vol. VII, pp. 28, 213; vol. VIII, p. 93), which hardly suggests a continuation of the regime's policy toward the peasants on the model of Lein's NEP. And while the author may be right in noting that the term *kolkhoz* (collective farm) does not occur in Stalin's writings before the 15th Congress, as early as June 1925 Stalin spoke of the necessity of organizing the millions of the peasant masses into cooperatives (*ibid.*, vol. VII, p. 157) and emphasized that the proletariat would have to wage a struggle not only against the *kulaks* but aginst the *peasantry as a whole*. (p. 173).

Perhaps Dr. Lewin has defined the explanatory parameters of his investigation somewhat too narrowly. His study would have benefited from a critical examination of the problematic nature of Lenin's concept of the alliance (*smychka*) between the proletariat and the peasantry, as well as by a thorough analysis of the character of the agrarian revolution of 1917. In general, Lenin's position on the peasant question does not emerge clearly in the book—in spite of the author's implicit recognition that Lenin *was* a participant in the "great debate." And the exposition of Bukharin's position is not without serious shortcomings. At times the author does not make clear whether Bukharin is expressing his own ideas or quoting Lenin.

Unfortunately, the translation of Dr. Lewin's work leaves much to be desired, especially in regard to grammatical construction. The general reader may object to the often excessive and unnecessary use of Russian terms, some of which are not explained, either in the text or in the glossary.—ROLF H. W. THEEN, *Iowa State University.*

Ethnic Minorities in the Soviet Union. EDITED BY ERICH GOLDHAGEN. (New York: Frederick A. Praeger, 1968. Published for the Institute of East European Jewish Studies of the Philip W. Lown School of Near Eastern and Judaic Studies, Brandeis University. Pp. 351. $8.75.)

Until now the nationality problem in the Soviet Union has not been considered sufficiently important to attract wider attention. Even books of recent vintage that survey the half century of Soviet development frequently brush off the consideration of this question. This neglect can be explained and understood, but can not be justified. The problem deserves close attention because the nationality policy is an integral part of Soviet development. It has considerably influenced the shaping of the Soviet system and has continuously affected the making of the Kremlin's policies. The Bolshevik solution of the national question decisively affected the establishment of the Soviet federal state. Stalin's decision that favored the development of a national Russian state as against a Soviet commonwealth of nations has added to the remaking of the Soviet Union from an originally intended revolutionary ideological power into an intensely ambitious imperialist nation-state. The resurgent non Russian nationalism now threatens this concept and, taken together with the general dissent demanding liberalization, presents grevious problems to the Kremlin rulers. Professor Brzezinski's recent suggestion that the national question in the Soviet Union represents one of the most explosive issues indeed is a good reminder of this empirically demonstrable yet unjustifiably disregarded fact.

The volume edited by Mr. Goldhagen, therefore, is the more welcome, because it signifies the growing recognition of the problem. It is not as all-inclusive and systematic as the editor suggests in the introduction (for example, Georgia should not be missing, the new developments of Siberia and autonomous republics are of utmost importance) and indeed the principle of the organization of the book is not very clear. Generally, it focuses on the phenomenon of non Russian self-assertiveness. The book contains five excellent, very well researched and documented articles that deal with individual nationalities. These cover the Ukraine (Yaroslav Bilinsky), the Baltic Republics of Estonia, Latvia and Lithuania (Jaan Pennar), the Turkic peoples (Garip Sultan), the legal situation of the Jews (William Korey) and the Yiddish *Sovetish Heimland* whose contents are analyzed with gusto by Joseph and Abraham Brumbergs. The other nationality studies, unfortunately, are not as instructive though they too offer some insights into the ambiguity of Belorussian nationhood (Nicholas Vakar), social change in Armenia (Mary Matossian) or the complexity of nationality consciousness in Central Asia (Edward Allworth). Generally, the authors maintain that with the exception of Belorussia, national assertiveness is strong among the non Russians, even among the Jews, whose cultural and religious life has recently

been, as Korey well explains, the most suppressed of all.

In addition to individual case studies, Mr. Goldhagen's book is valuable for discussions of broader issues. One of those is Jacob Ornstein's informative review of Soviet language policy. Another is John Armstrong's excellent analysis of Soviet nationalities in the Soviet system of power. There are of course alternative methods of dealing with the problem and one can legitimately question whether the functional model, even in the adjusted form that is used by Armstrong, is adequate for capturing the very dynamic, rapidly changing and complex process of Soviet nation building and indeed whether social communications indexes alone provide a sufficient basis for reconstructing a theoretical framework within which the Kremlin deals with its nationalities. This remark is not meant to detract from Professor Armstrong's accomplishment. His study is one of the very first in scholarly literature that examines the nationality development as an integral part of the Soviet system. The author thus elevates the study of Soviet "minorities" to the plane of an empirical analysis of the political system itself and his article is a "must" for any serious student of Soviet nationalities.

Finally, in a long but rewarding article Vsevolod Holubnychy tackles one of the most controversial questions in the field, namely, whether the Russians economically exploit the non Russian republics. In this very learned and well documented study the author uses a purely economic model to prove his thesis that economic exploitation constitutes the essence of Soviet nationality policy. The statistical evidence presented, however, is mostly limited to the Ukraine and is not as convincing as the wealth of material used suggests it would be. Holubnychy, however, has raised important questions, presented cogent arguments and thus has triggered off some overdue but very useful and badly needed discussion that might and should lead toward a better clarification of the role of economics in Soviet nationality policy. Too frequently this role has been either too narrowly understood or denied altogether.

Ethnic Minorities in the Soviet Union is a result of a conference that the Institute of East European Jewish Studies of Brandeis University had organized in 1965. Both the Institute and the editor of the book should be commended for its efforts and encouraged to continue such research.—V. Stanley Vardys, *University of Oklahoma.*

Communism and the Yugoslav National Question. By Paul Shoup. (New York: Columbia University Press, 1968. Pp. 308. $9.50.)

Among the countries in Eastern Europe, Yugoslavia appears to have the greatest mania for reorganization which currently embraces the political system, economy, and cultural life. Its communist party allegedly no longer will govern in the future but merely represent a guiding force. At least, such a change had been envisaged as far back as the July 1966 plenum of the Central Committee which met on the island of Brioni. The author of the scholarly work under review ends his coverage with this event.

Professor Shoup has had the great advantage of studying at the law faculty of Belgrade University, and his book is based on original source materials in the Serbo-Croatian language. In the thesis, he contends that "the Yugoslav national question today is very much part of the contemporary drama of nation-building and economic development now being played out in many parts of the world." (12)

Returning to intra-communist relations, for the moment, it is interesting to note the parallels between Comintern policy *vis-a-vis* the Yugoslav party and other parties in Eastern Europe. The author describes the former relationship in the background to his excellent study. Thus, after 1928 the Comintern called for peasant uprisings and in 1934 even ordered a policy of secession for Croatia, Slovenia, and Macedonia by communists in these provinces. Professor Shoup also concludes that already in May 1941 the "Yugoslav Communist Party was beginning to act independently of the Soviet Union and to conceal its actions from the Comintern, if only in matters of local importance." (p. 62) He also suggests that peasant nationalism during World War Two was based on hostility toward alien [i.e., German] rule. How many of the 3,000 communist secret police killed after that war can be blamed on the peasantry, which may have considered the victims "alien," is probably classified information at Belgrade.

Moving on to foreign affairs, the author discusses a proposed Balkan Federation, with Bulgaria envisaged as a seventh republic within Yugoslavia. This never materialized. Had the plan been implemented, then perhaps the current Macedonian dispute might never have arisen. Professor Shoup contends that solution of the nationality question for the Yugoslavs "could serve as a model for the newly developing nations of Africa and Asia with problems similar to their own." (pp. 188–189). The possibility of a single, amalgamated Yugoslave culture apparantly has been abandoned due to local resistance and the need to rely on more nationalistically oriented republics like Croatia and Slovenia in order to implement economic reforms.

In his conclusion, the author suggests that na-

tion-building in Yugoslavia may never be completed. "It is clear that one transitional phase, at least, has come to an end with the breakdown of the effort to create a new type of nationalism based on the quasi-communist, quasi-democratic institutions of Titoist Communism." (p. 265). The book includes valuable appendices portraying nationality statistics for the population, the Yugoslav communist party, republic communist parties; opinions about relations among the various nationalities; the national origin of key government and party officials; and an excellent bibliography.—RICHARD F. STAAR, *The National War College.*

Russia's Protectorates in Central Asia: Bukhara and Khiva, 1865-1924. By SEYMOUR BECKER. (Cambridge: Harvard University Press, 1968. Pp. 416. $12.50.)

Since Churchill's famous dicta on Russia, much firstrate research has reduced the enigma to more manageable proportions. Some riddles, however, remain to engage our attention. These would include aspects of the decision-making process at various elite levels, the succession problem, and the theory and practice of the regime's policies toward the nationalities; Professor Beck's book is a valuable addition to the growing literature in the latter field.

Russia's Protectorates does not attempt to address itself to contemporary problems of Soviet politics. Yet it explicates these through a detailed, scholarly, immensely readable history of the two Central Asian Protectorates. On clear aspect of Russian policy in Bukhara and Khiva is that, instead of the the stereotyped "Prison House of Nations," St. Petersburg followed enlightened policies. The two Protectorates did eventually succumb to Russification, as the more advanced Russian state brought the benefits of civilization, and as the frontier between British and Russian spheres was stabilized; but "civilization" came as the result of a more advanced state coming into contact with a relatively more backward one.

Thus Bukhara and Khiva became part of Russia through a process of attrition, in the manner of the old truism that power abhors a vacuum. Yet the sovereignty of the two Protectorates, with the exception of their foreign relations over which they lost control at an early stage, was ended only when the Bolsheviks seized their territory as an aftermath of the Civil War. Another myth is shattered along the way: the Russian Governors in Central Asia were able, competent, far sighted men. Their policies were usually supported by St. Petersburg; when opposition arose, it came about either as a result of national preoccupation elsewhere, or local policy recommendations were ultimately adopted. But let Prof. Becker summarize the ultimate causes of Russification:

> In short, Russia was spurred on in Central Asia by a whole complex of motives—the quest for a secure frontier, the provocations offered by unstable neighbors, the fear of being excluded from the area by England, and the temptation of diplomatic leverage, economic profit, and military glory. (p. 23)

Advanced nations cannot expand their influence into less developed territories without eventuating political change. The coming of the railroad and telegraph first brought enclaves for the citizens of *Russia,* then granted them extraterritorial justice and law. These changes, beginning in the 1880s, were *ad hoc;* just the same they slowly, imperceptibly, but profoundly, changed the relationship between St. Petersburg and the Protectorates. If the latter retained their *de jure* independence, it was only because the Russians so willed it. In actual fact, both Bukhara and Khiva became dependencies; their respective rulers attempted to oppose the new trend, and to retain their previous personal perogatives. In this they were ultimately unsuccessful. They were, however, able to retard the process.

Nor was Russian policy consistent. A long period of neglect—or, more accurately, a period of classical *laissez faire* on part of the Russian state—came between times of active interest in the Protectorates. Becker gives a detailed analysis of economic, military and political factors in the changing relationships. The Russian regime was not to be rushed into colonization. The reason soon became clear:

> St. Petersburg did the inescapable minimum to protect and promote Russian private interests in the khanates and acted to restrain those interests when they threatened the foreign ministry's policy of non-intervention. Concern for her own strategic position was more important to official Russia than were the economic interests of her own subjects. (p. 191).

Nevertheless, Russian technology slowly gnawed into the Protectorates' independence. Even though the policy of Russification was renounced, the Russian presence inexorably led to loss of local autonomy and privileges.

After the turn of the century, however, the pressures toward greater Russian influence became increasingly hard to resist. New local rulers, especially in Khiva, lost the awe in which their fathers had held Russian military prowess. Rebellions against "foreign" rule multiplied. Successive defeats of the rebels drew the Russians ever deeper into local administration of the Protectorates. These trends suited the Leninists; after the defeat of the Whites, the region was gradually absorbed into the Soviet Union. Anti-Soviet activities and opposition to the Red Army in the Civil War may have hastened their end as semi-independent

states; but it is unthinkable that the old dispensation could have endured into the Stalinist period.

Professor Becker has written the definitive work of Bukhara and Khiva during the period discussed. This reviwer might wish that more attention had been devoted to foreign affairs (which he hopes Prof. Becker will cover in a subsequent volume), and the price, which will confine the book to libraries and specialists. All those with an interest in cultural and political history are urged to take this truly excellent work, both scholarly and exciting, from the shelves. Good maps, bibliography (exhaustive, and exhaustively read and skillfully applied), glossary of terms (essential to a work of this nature), adequate index, pertinent treaties given in appendixes, aid the reader and add to his enjoyment.—PETER MAYER, *Westinghouse Electric Corp.*

Architecture and Politics in Germany, 1918–1945. By BARBARA MILLER LANE. (Cambridge: Harvard University Press, 1968. Pp. 278. $15.00.)

The politics of culture is a new topic for most political scientists. Although other problems of social policy often seem more pressing than those relating to the arts, cultural and aesthetic questions are being inscribed on the agenda of public concerns in increasing numbers. Barbara Miller Lane's book, *Architecture and Politics in Germany,* 1918–1945, is the kind of work on which the study of politics and culture will thrive. It is thoroughly researched, beautifully illustrated, and a paradigm of objectivity—especially important in the chapters on Nazi architecture, usually dismissed as "piles of unwiedly blocks," or an "endless repetition of dead forms."

A major theme of this study is that in politicizing architecture, the Nazis were not fulfilling some imperitive of a fascist form of political organization but carrying on a tradition begun in the twenties. The most detailed chapters of the work are devoted to the two political disputes which resulted in the exiling of the Bauhaus, haven for radical architects in the twenties, from Thuringia to Dessau in 1924, and finally in its permanent dissolution in 1932. Using the large collection of material from Walter Gropius' personal archive, the author draws a vivid picture of the artistic ferment in Wiemar Germany. Radical architects, under the leadership of Groupius and Bruno Taut formulated their goals in clearly ideological terms. They sought a "new totalism" through the medium of a socially conscious architecture carried out by men washed of traditional ideas by submission to a new regimen of courses in abstract design, form and color. The first "Battle of the Bauhaus" pitted these architects against local politicians who accused them of foreign influence, cultural decadence, and "art bolshevism."

After 1924 the argument shifted somewhat for with the new prosperity proponents of the international style found themselves favored with many commissions. In Frankfort Ernst May constructed large tracts of public housing in the new style, and building societies such as the Gehag brought the new ideas to Berlin. But with national prominence came national enemies. Paul Schultze-Naumberg led the enlarged opposition by reviving claims that the new style fostered racial decline by offering the people a faulty "wish picture."

By the time the Nazis came to power in 1933, national opposition to the new architecture had been consolidated in the Kampfbund under the tutelage of Alfred Rosenberg. Soon, however, both Schultze-Naumberg and this group fell from favor, only their negative proposal (purging of the Bauhaus) being accepted. Sponsorship of architecture under the Nazis was never fully centralized. Under the leadership of its frustrated architect, the regime boldly proclaimed that its new building program would create a common will, foster a feeling of proud togetherness and contribute to political unification. Beneath the monolithic rhetoric, however several different styles flourished. Albert Speer's party congress buildings brought the strong simplifying impetus of the international sytle to a classical revival, Clemens Klotz enlarged on medieval themes in his design of party leadership schools, and a wide variety of minor party buildings were constructed in various regional folk idioms.

Readers will be rewarded by a special asset of this book, the excellence of its design and typography. The book is set in Nelior, a typeface with strong architectural elements, strong, column-like verticals and squared-off forms. Beginning and end pages are designed in forms reminiscent of the Bauhaus-style itself, and the body of the work is given interest by a scheme of organization which adjusts the masses of each page up- and centerward. The placement of the illustrations creates pleasing variations on this theme except perhaps on pp. 29, 91 and 124 where the compression of elements leads in other directions and tends to make for crowding. The book, however, offers strong encouragement to those resigned to finding mundane texts in beautiful books and worthy manuscripts in nondescript editions.

This study is a work of history. It would be wrong to complain that it contains little theorizing and no explicit models of personal behavior or political interaction. No enterprise of understanding can survive flaws in its assumptions, but history, unlike political science, can plead modesty in making a show of its theoretical framework. The

lack of explicit theory will tantalize political scientists, however. There are several indirect references to questions of space and behavior, for example. Nazi public housing projects returned to a "German" form of house plan with many small rooms form the plan popularized by radical architects which consolidated public rooms into a single area. Any comprehensive theory of architecture and politics will have to deal with the interplay of created environment and social behavior of the kind hinted at here. The dynamics of architecture as a political "issue area" provides another important theoretical viewpoint. This framework would ask, for example, whether the pluralism of building styles under the Nazis was inherent in the nature of architecture or a result of the "opportunism" of the Nazi regime. Evidence regarding the architectural programs of the other fascist regimes is becoming available and could be integrated into a larger theory of architecture and politics. It is a tribute to this book that it will be found not only on coffee tables and shelves of art books but in the working files of those interested in conceptual questions such as these. It is a work of many sensibilities.—W. CURTIS LAMB, *Yale University.*

Specialists and Generalists: A Comparative Study of the Professional Civil Servant at Home and Abroad by a Group of University Teachers. ED. BY F. F. RIDLEY. (London: George Allen and Unwin Ltd., 1968. Pp. 213. $5.00.)

The "professional" versus the "amateur" in administration continues to be a major concern of British public service. The Institution of Professional Civil Servants commissioned a study of the professional bureaucrat in Britain, Australia, France, West Germany, Sweden, and the United States to submit to the Fulton Committee reviewing the Structure, Recruitment and Management of the Home Civil Service. This volume, *Specialists and Generalists,* evolved from the submission.

Although the IPCS represents scientific, technical, professional and specialist classes in the British Civil Service and other public bodies, this symposium argues no case, but analyzes the role of the specialist in six governmental systems. Nevertheless, the study is prompted by the widely expressed fear that British administration is not coping successfully with the critical problems of a scientific and technological age. Professor F. F. Ridley, University of Liverpool, and co-author of *Public Administration in France,* directed the work of the group and contributed the introduction, summation, and excellent account of the professional in France. Other contributors were Dr. B. B. Schaffer, University of Sussex; Dr. Nevil Johnson, University of Warwick; Dr. H. Parris, University of Durham; and Professor R. H. Pear, University of Nottingham. The baseline paper on Great Britain was provided by T. H. Profitt, Deputy Secretary General of IPCS.

A study of this sort runs into problems of definition. What is considered generalist in one system may be specialist in another. Does education in political science, public administration or other social science, or in law, really produce a specialist for public service? To minimize different understandings of such terms as generalist, specialist, professional, administration, management, technology, etc., the studies proceed by illustrating the career ladders, pay and status, and access to politicians that may be expected by arts graduates, lawyers and jurists, engineers, scientists, accountants, and others in the various systems.

In the British context, both persons and functions are separated into generalist and specialist. Generalists advise Ministers, formulate administrative policy, and as Mr. Profitt states, control the Governmental machine. The professional carries out the work of specialist agencies under administrative and financial control of the generalist, and advises the generalist on policy. In Sweden, ministries function as policy-planning staffs and more routine administration is carried out by boards. In the other four countries, there is little structural separation of specialist or advisory work from general administration. In Australia, France, Germany, Sweden, and the U.S.A., the specialist and professional have greater opportunities at the top than in Britain. On the Continent and in Australia, some senior administrative posts are restricted to specialists. In particular, the Australian federal service is characterized by respect for specialists, and rights and opportunities for the professional are built into the structure of the service. The generalist-specialist dichotomy in British Civil Service is linked to the practice of selecting ministers, not for their expertise, but for their ability to make balanced judgments based on evidence submitted by experts. The senior administrator is expected to bring to the work of the department the same kind of judgment. Another important influence on the public service has been the generalist orientation of the British universities, where administrative class is recruited, and the development of technical education outside the universities. Certification in technical fields has been undertaken by professional and technical associations, and such certification often lacks the prestige of the university degree. The authors show that outside Britain, recruitment to the public service from "non-vocational" education is rare. As Prof. Ridley observes: "The notion that a man who has done well at Cherokee is *ipso facto* likely to make good civil servant is not found elsewhere."

Civil services must be seen against their wider institutional, social and historical backgrounds. The British system has been based on ninteenth century concepts of liberalism, private enterprise, and local self-government which saw the function of government as arbitration between interests, adjudication of proposals, and regulation of the activities of others. In France, there is a tradition since Napoleon for state intervention in many spheres of life, and it is accepted that the state has active responsibility for direct provision of services. In Germany, the home of professional public service, the administrative machine was designed for the management of important social and economic functions in addition to the maintenance of law and order. In Australia and America, strong egalitarian influences have had much to do with the doctrine and structure of the public service.

This little book is interesting not just for what it tells us about civil service but also for what it says about higher educational systems and how they relate to governments. Values inculcated in universities are reflected in the views of their graduates in public service and in what they consider to be most worthwhile and prestigious in the way of duties. In Britain and the United States, government has had little to say about university curricula; but in America, legislative action, particularly the Morrill Act, has given stature to the "applied arts," a prestige that has been carried over into civil service. French government assumes more direct responsibility for higher education that the British and has more often prescribed qualifications for professional practice, which has led to a more career-oriented higher education. Civil service career schools form an important part of the French system of higher education. Germany goes farther than any of the countries surveyed in linking university and professional training to particular public service careers. In law and medicine, university courses do not lead to a degree but to a state examination. There is no state examination for engineering, but the curriculum is under state supervision. In other technical fields, government will determine the qualification and in some areas will provide the training for it.

Some chapters of the book make generalizations without sufficient supporting data. At times, there is mere restatement of prevailing opinion about the effectiveness of public services. This is noticeably true in the United States paper where the argument is made for a high-level generalist administrative service of the sort outlined in the Second Hoover Commission Report. Bibliographic references are from the most standard texts and up-to-date citations are few. A number of important questions remain unanswered and perhaps cannot be answered. What kind of civil service organization can cope most effectively with modern problems? What education and aptitudes should a civil servant possess to perform best his functions in a modern society? One gets the uneasy feeling in reading these accounts that a service can be dedicated and efficient in performance of its duties and yet be irrelevant in many ways to the needs of the time. Conversely, states may be doing some of the right things though not always competently.

The book makes no prescription for British administration, but in the concluding chapter Prof. Ridley performs the best function of a generalist by ably summing up the evidence in the generalist-specialist debate and giving the whole subject a fuller and clearer statement than it has had. Dual hierarchy is not a central question in U. S. public administration, but American students will find the tracing of the broad theme of generalist-specialist through six governmental systems a valuable addition to the too-brief list of materials in comparative bureaucracies.—HUGH G. MACNIVEN, *University of Oklahoma.*

INTERNATIONAL POLITICS, LAW, AND ORGANIZATION

Alliances and Small Powers. By ROBERT L. ROTHSTEIN. (New York: Columbia University Press, 1968. Pp. 331. $10.00.)

This book is in essence a collection of essays unified only by the central proposition that "Small Powers are something more than or different from Great Powers writ small." (1) On the whole, it is a sound and carefully reasoned analysis of a thesis often obscured in the literature of international relations: that despite changes in the international system, Small Powers have continued to exhibit behavioral patterns different in kind and not merely in degree from those characteristic of Great Powers.

The author, discarding the "outmoded" method of calculating the distinction between Great and Small solely in terms of material capabilities, has defined Small Powers in a manner which takes account of psychological factors as well. Thus a Small Power is a state which in the eyes of itself and of other actors in the system is unable to "obtain security principally by use of its own capabilities," and which thus "must rely fundamentally on the aid of the other states, institutions, pro-

cesses, or developments to do so. . . ." (29)

But not *all* of the Small Powers so defined are examined in Professor Rothstein's study. He explicitly excludes from his analysis 1) Latin American states; 2) the behavior of all small states prior to 1815; 3) those small states which do not perceive a potential or actual threat from the policies of Great Powers.

The book is organized in three "self-contained" parts. The first, the most abstract and analytical, seeks to derive propositions about Small Power behavior "which are valid for any historical period." (7) Quickly dismissing as illusory or unattainable policies of neutrality, disarmament or world government, the author focuses on Small Power difficulties in achieving security by means of an alliance policy. He describes the unique advantages and disadvantages for Small Powers of three different types of alliances: unequal bilateral alliances between a Great Power and a Small Power, multilateral alliances among Small Powers, and multilateral pacts mixing Great and Small Powers.

Rothstein's analysis of Small Power behavior in the first two types of alliances is accompanied by two lengthy case studies, one tracing the history of the Belgian-French Military Accord of 1920, the second examining the rise and fall of the "Little Entente" between 1920 and 1938. The actual utility of these long and repetitive studies is difficult to discern. They are not well-told narratives, for they lack careful organization and fail to impart a clear sense of the flow of successive events. They seem excessively detailed for the author's purposes, and indeed only serve to obscure his basic points. (Especially irritating to this reviewer was the author's insistence on incorporating untranslated passages of French into the text, with only occasional translations included in the footnotes—a reversal of what would seem to be the proper procedure.)

Rothestein's subsequent generalizations about the two varieties of alliances not only (as he freely admits) are not limited to the lessons which can be drawn from the case studies, but, indeed, only occasionally refer to them. And his concluding generalization—that while the Small Power alliance is politically and strategically preferable to the unequal bilateral pact, both are inferior to a mixed, multilateral alliance—is, for some unexplained reason, unsupported by a study of the operation of this third type of alliance.

Part II examines the alliance strategy of Small Powers in the context of three historical international systems, those existing in 1815–1854, 1854–1914, and 1919–1939. Professor Rothstein utilizes systems theory not as an explanation of the activity of Small Powers, but as "merely another variable" setting limits to their behavior. (184) A basic difference in the actions of Great and Small Powers is discerned in the fact that the behavior of the latter is "conditioned, even determined, by the nature of the system itself." Though Small Powers can rarely "create their own opportunities" (182) and remain dependent on outside forces, some international systems are found to be relatively more conducive to the security and influence of these states.

Part III examines the central thesis in the context of the contemporary international setting, which the author declines to subsume under a single "systems" label. One section, most of which was published as an article in 1966, argues that the nonalignment of Small Powers in the contemporary world, viewed both as a tactical principle and as a "mystique," constitutes an order of behavior different from traditional "neutrality," fails to achieve "real" independence from Great Power interference, and is, in essence, a "scavenger" policy in new clothing.

The second section, dealing with the problem of nuclear proliferation among Small Powers, is taken largely from a 1967 article in which the author sought to establish that Great Powers and Small Powers both *ought* to and *do* calculate differently when deciding whether to acquire nuclear weapons. The analysis, focused on the behavior of the actors rather than on the prospects of stability for the system, concludes that while from a standpoint of military security the marginal utility of nuclear weapons for small powers is not high, when psychological and political factors are added in the option may become more attractive. Asserting that proliferation is the most critical problem of our time. Professor Rothstein suggests for the United States as the "minimum conceivable" measure for preventing proliferation a policy of bolstering nuclear guarantees with pledges to *give* nuclear forces to nuclear-capable countries facing a critical threat from a nuclear power. The author's policy prescriptions in this area are unfortunately dated by his failure to discuss the pending Non-Proliferation Treaty, which is simply dismissed in a footnote as "limited." (323)

One of the virtues of this book is the author's consciousness of its limitations and his great care in qualifying his analysis, which he describes as a "stationary target for future investigations." (8) He has built a well-reasoned argument in support of the conclusion that Small Powers, continually confronting the dilemma that "the necessity of insuring short-run security by supporting the strongest power" is in conflict with "the necessity of bolstering long-run security by supporting the weaker powers," (270) have indeed behaved in a manner unlike the Great Powers. And he has argued per-

suasively that, if the resulting Small Power policy of delaying commitment until the last possible moment seems unsatisfactory as a principle of action, then it at least illuminates "the impossibility of devising specific principles for Small Powers short of knowing all the circumstances affecting their situation." (270)—ROBERT H. DONALDSON, *Vanderbilt University.*

The Franco-Moroccan Conflict, 1943–1956. BY STÉPHANE BERNARD. (New Haven: Yale University Press, 1968, Pp. 680. $17.50.)

Is the "Academy of Colonial Sciences," which Marshal Alphonse Juin (who was then France's Resident-General in Morocco) addressed on the theme of Franco-Moroccan "cosovereignty" in November 1949, still holding sessions? If so, its program director might well invite sociologist Stéphane Bernard to present his conclusions on the irreversibility of the forces promoting decolonization at its next meeting. For though his analysis offers little solace to any remaining partisans of schemes like cosovereignty, certainly Bernard has brought to bear an imposing array of social science concepts and techniques upon one major episode in the history of colonialism. In attempting to reconstruct the "sociological mechanism" of the Franco-Moroccan conflict from 1943–1956, he presents an elaborate "theory of a political system in the process of decolonization" (p. xvii), a theory grounded in the structural-functional approach.

This edition is a translation of volumes I and II of a three volume work published in Brussels in 1963 as part of a series of studies of international conflict commissioned by the Carnegie Endowment for International Peace. The research was carried out by a team under the direction of Bernard, who is Director of the Center for Political Theory of the Institute of Sociology of the Free University of Brussels. The work is an ambitious one, uniting disciplines in order to define a theoretical perspective upon decolonization, a political process of interest to students of both comparative and international politics. Bernard seeks to embrace both these dimensions in his model of the conflict. While the total achievement of the work is impressive, the theoretical edifice will perhaps be more satisfying to sociologists than to political scientists.

Bernard offers us in volume I a detailed history of the period in Franco-Moroccan relations that began when President Roosevelt met Sultan Mohammed V in 1943 and ended with the reestablishment of Moroccan independence in 1956. Shortly after the termination of the protectorate, Bernard's researchers began conducting interviews with officials from Morocco's two communities, European and Muslim, and within French political circles. The resultant historical account is fresh and authoritative. Nevertheless it focuses primarily upon the evolving strategy of successive Residents—General in their attempt to contain Moroccan nationalism—from the thwarted reforms of Labonne to the hard line of Juin to the conspiracies of the Guillaume period that led to the deposition of Mohammed V.

Less attention is given to the nationalist leadership, relatively little emphasis is placed upon the nationalist conception of the struggle or the evolution of nationalist strategy. This characteristic of Bernard's account reflects his conviction that forces inherent in the social situation determined the eventual outcome. Implicit throughout his argument is the idea that the fundamental anti-French attitude of the Moroccan masses was the prime mover of the course of events, and the nationalist leadership essentially a secondary agent of this force. Bernard's interpretation is at once apt and misleading. On the one hand, most observers would agree that the social force underlying nationalist movements is the most important explanation of the general phenomenon of decolonization, and that nationalist leaders are expressions of such movements. Yet precisely because of this agreement, students of politics have turned their attention to more subtle mechanisms, concerning themselves for example with the determinants and the consequences of particular decisions of tactics and strategy which nationalist leaders have been forced to make. Something of the political drama of the struggle for independence as seen from Istiqlal headquarters is lost at the same time that we gain an appreciation of the view from the Residency-General as it waged its "stubborn struggle against an inevitable process." (p. 16).

Volume II presents the model. Bernard argues that a "valid theory of colonial conflict" requires answers to three questions, namely to what extent are political attitudes a function of the social situation, to what extent is decision-making capacity a function of these attitudes, and to what extent can a colonial regime, given its specific milieu, fulfill its problem-solving function (p. 422). These questions are indeed pertinent ones to formulate, and Bernard answers them comprehensively for both France and the protectorate. Yet the 200 odd pages in which he treats them do not clearly demonstrate why these three sets of data constitute a "valid theory of colonial conflict." Why not ask only the third inasmuch as the first two seem germane to a behavioral analysis of any political system, colonial or otherwise? Or why not pose other questions such as to what extent is the "consensual potential" (to evoke another term and theme central to Bernard's analysis: the relationship be-

tween authority and consent) of a colonial regime diminished by a given nationalist strategy? Perhaps Bernard would argue that this question is subsumed under one of his three categories; my point is that he does not adequately demonstrate the theoretical necessity and sufficiency of these three.

Bernard concludes that one can view the decolonization of Morocco either in mechanistic or functional terms as a transformation of political control. Both of these perspectives bring into focus instructive aspects of the conflict, the dwindling consent enjoyed by the residential authority or the decline in its ability to solve problems. Yet though Bernard may elucidate the sociological mechanism of this transformation, the political scientist is likely to stress that colonial legitimacy dwindled as nationalist leaders built effective organizations, and problem-solving capability diminished as the nationalists succeeded in defining the problems more in political terms than in technological terms. Bernard is to be sure well aware of this dimension of colonial conflict, but his emphasis lies elsewhere.

Indeed as a sociologist concerned with political theory, Bernard brings a valuable perspective to political scientists concerned with the various consequences of decolonization for Morocco and France. The total impact of his enterprise is engaging and impressive. It contributes valuably to our understanding of the processes of colonial systems coping with the very movements of social change that colonialism accelerated.—ROBERT A. MORTIMER, *Haverford College.*

The Nuclear Revolution In Soviet Military Affairs. TRANS. AND ED. BY WILLIAM R. KINTNER AND HARRIET FAST SCOTT. (Norman: The University of Oklahoma Press, 1968. Pp. 420. $6.95).

Several years ago the Soviet Union deployed the world's first anti-ballistic missile defense system. During 1968 the numbers of Soviet land-based intercontinental ballistic missiles surged close, or possibly equalled, the 1054 ICBM's possessed by the United States. At the same time the number of Soviet missile launching submarines increased also, but at a lesser rate. The undeniable Soviet activity regarding strategic nuclear weapons (increases also were noticed in the size and equipment of conventional forces) raises grave questions for the United States as it looks to its security for the 1970's.

To some, most notably the military and their supporters in the Congress and elsewhere, the Soviet actions are interpreted to mean the Russian Communists are still doggedly pursuing the goal of world domination, although with a new emphasis being placed upon strategic nuclear war as being "the continuation of policy by other means." Beneath the concern of these individuals is the belief that a major cause for the American success in the Cuba missile confrontation was the knowledge in both Washington and Moscow that the former possessed strategic nuclear superiority, as well as conventional superiority in the immediate zone of possible conflict. The fear expressed by this group is that the Soviets mean to surpass the United States for the first time in strategic nuclear weapons and that Moscow will then use the new relationship to advance its hegemonic objectives. On the other hand those who fear nuclear war more than they do Soviet expansionist designs are voicing alarm that the United States will overly and unnecessarily respond to the Soviet activity; and that in so doing the United States will launch a new, costly, and dangerous arms race.

In regard to the debate which is shaping up over what should be the proper American response to the Soviet activity it comes as no surprise that Kintner, Deputy Director of the Foreign Policy Research Institute, and Mrs. Scott an associate who is fluent in the Russian language, assign ominous intentions to the Soviet Union. Some may be tempted to lightly dismiss Kintner's effort as simply another "hawkish" book consistent with his views expressed together with those of Robert Strausz-Hupé *et al* in books such as *Protracted Conflict* and *A Forward Strategy For America.* This would be unfair because, although the message is much the same (the United States must be ever on guard against Soviet expansionism), the technique is different. Instead of telling the reader why the Soviets are dangerous Kintner and Scott offer translations of Soviet doctrinal and strategic writing which will, in the editors' view, "afford the reader an opportunity to examine for himself Soviet views on their 'revolution in military affairs' and to relate this to the future security of the non-Communist world and to the United States in particular."

The translations which are presented are drawn from recent [October 1964 to March 1967] authoritative Soviet military sources such as *Red Star,* the newspaper published daily by the Defense Ministry; *Communist of the Armed Forces,* the theoretical journal published by the Army and Navy; a set of volumes on military affairs entitled *Officer's Library;* several books published by the Soviet military press, and speeches made before the XXIII Congress of the Communist Party of the Soviet Union (1966). The translations are in the form of most Soviet official writing, turgid and repetitive. Of much more importance is the fact the reader may not interpret them as have the editors. For example, Kintner and Scott present a translation of an attack upon the much read, at

least in the West, 1965 article by General N. O. Talensky in which Talensky stated it was a "dangerous illustration" to believe thermonuclear war can advance a nation's political goals. In the translated article the Talensky thesis is criticized as follows, "Such an assertation is not only in error, but it is harmful because it can shake one's assurance in our victory over the aggressor, and the consciousness of the necessity to be ready at any moment for armed struggle with the use of nuclear rocket weapons." Whether such statements, and similar ones, should be interpreted as suggesting the Soviets mean to surpass the United States in strategic weapons as a prelude to advancing Soviet hegemony is open to argument. Another explanation is that the cited statement, and the others of like character which abound in the book, compromise nothing more than the normal expression of a military man's confidence in his nation's capacity for victory. Herein one finds the major problem with *The Nuclear Revolution In Soviet Military Affairs,* and indeed with all efforts to understand Soviet writing—as every intelligence officer knows it is much easier to determine an enemy's capabilities (in this case how many ICBM's he has) than to learn his intentions.

Because of its contents, and the implications for American policy suggested by Kintner and Scott, their book would normally be provocative and important. However both characteristics have been heightened by the election of Richard Nixon to the Presidency. It will be recalled that Mr. Nixon charged during his campaign that the Democrats had permitted a "security gap" to develop, and he stated if elected he would deal with the Soviets from a position of superior military strength. Unless Mr. Nixon's statements were only campaign oratory carrying little substance, it is logical to expect that the school of thought whose views are supported by *The Nuclear Revolution In Soviet Military Affairs* will receive sympathetic reading in Washington—unless of course someone steps forward to refute its message, and that being ascribed to the Soviet military activity.—ROBERT M. LAWRENCE, *University of Arizona.*

The "Other" State Department: The United States Mission to the United Nations—It's Role in the Making of Foreign Policy. By Arnold Beichman. (New York: Basic Books, 1968. Pp. 221. $5.95.)

This short volume by a verteran journalist is a contribution to the existing literature on the policy-making role of the United States Mission to the United Nations (USUN). Though limited, this literature is characterized by a split between those who find USUN and the missions of other nations playing a substantial policy-making role and those who judge these roles to be marginal. Professor Robert E. Riggs seemed to have joined the latter group when he stated in his *Politics in the United Nations* published in 1958 that he could find only one or two instances when the U.S. Delegation* exercised an important influence upon major questions of policy. The following year, after having served on the U.S. Delegation to the United Nations, Senators Hickenlooper and Mansfield complained of exessive control from Washington. They charged that Washington spelled out the official U.S. position in minute detail on almost every issue and when new developments required changes, these had to be cleared in Washington. Professor David Kay and Mr. Richard F. Pedersen disagree. The latter, former Chief of the Political Section of USUN, argues that national missions (presumably including USUN) enjoy a large measure of discretion in tactics and considerable influence on policy. He alleges that their role in the policy area, though advisory, is significant, particularly after an issue has been joined.

With the publication of *The "Other" State Department* Mr. Arnold Beichman has taken his stand with the latter group. He views USUN as a major factor in the decision-making of American foreign policy, reaching a position suggesting autonomy. Its position will continue to grow, so he avers, so long as it continues to enjoy the support of Congress and the American public, and so long as influential Americans to whom the President feels beholden continue to head it. Given their special relationship with the President, the heads of USUN have not had to fear reprisals from the State Department. Mr. Beichman alleges that they can commit the United States to a course of action, contrary to the wishes of the State Department. He recounts a most dramatic example of this: the successful attempt of Adlai Stevenson to reverse administration policy to enforce article nineteen of the Charter against the defaulting Soviet Union by depriving her of her vote in the General Assembly. The author maintains that this policy had received support from every appropriate official in Washington and New York including the President. By the manner of his negotiations with the Soviet Union in New York, however, Mr. Stevenson undermined any belief that the United States would hold firm. These he initiated without prior consultation with State, and they sucessfully undercut settled American policy.

The author envisages USUN as an actor in several subststems, principally those of the U.N. and the State Department, and he alleges that its role in the U.N. often conflicts with its role in the Department. He finds the milieu of the U.N. with its

* Although USUN is not the same as the U.S. Delegation to the U.N., the two can be used interchangeably in this review without affecting the essential meaning.

multilateral diplomacy to be a powerful force luring USUN away from its Washington connection. Mission personnel are constantly in danger of being "U.N. ationized." If this force is as powerful as Mr. Beichman contends and if USUN is as independent as he alleges, then we could logically expect many policy differences between USUN and State. But he is able to point to only a limited number of instances of such divergence—the one already mentioned plus mainly Ambassador Goldberg's iniatives with respect to South Africa and the Soviet Union. James J. Wadsworth, who has served with USUN, confirms the impression that these differences seldom occur. He estimates that USUN only once or twice a session finds its position sufficiently at odds with that of State to appeal for a change in official policy.

Mr. Beichman does provide evidence, some of it new, for his major thesis: that USUN enjoys an important policy-making role. But the evidence, consisting of a few case studies and the opinions of experts, is not systematic, nor is it conclusive. His principle sources are his own experience, memoir literature, and the statements of confidential informants. Some members of the political science fraternity will be happy to learn that he makes use of the latest concepts and terminology, perhaps the result of his recent graduate study at Columbia. His statement that all nations at the United Nations are equal we can excuse as carelessness or undue preoccupation with the formal rules of voting in the Assembly. Professor Goodrich's comment in the foreword about Mr. Beichman's contribution is close to the mark: while not the last word, this volume is a substantial introduction to the subject.—FREDERICK H. GAREAU, *Florida State University.*

America and Swaraj: The U.S. Role in Indian Independence. BY A. GUY HOPE. (Washington, D.C. Public Affairs Press, 1968. Pp. 106. $4.50)

This book demonstrates both the value and the danger of research which concentrates on a narrow slice of a complex, slowly-unfolding historical development. Guy Hope is, as the subtitle suggests, concerned with the specifically American contribution to the long and complicated series of events which eventuated in the independence of India.

Mr. Hope outlines exhaustively the activities of various groups in this country who were interested in independence for India. This is the great value of his work for it presents in one place a detailed and comprehensive account of the American concern. This is also the danger of this type of research for the narrow concentration of attention leads the author on occasion to recount every detail without sufficient evaluation of its significance. The result is the implication, and occasionally the imputation, of a greater American influence on the events leading to Indian independence than seems warranted if one studies these events in a broader context.

Nearly half of the book consists of a detailed account of the activities of different groups in the United States who, Mr. Hope argues, helped to influence the course of events in India. The best sections concern the role of Indian Nationals, such as the energetic and persuasive J. J. Singh, who were resident in this country and the internal policy discussion within the State Department. Mr. Hope's careful study of State Department documents, in particular, produces a detailed and interesting account of the evolution of official views on the Indian question and some penetrating comments on the dilemmas confronting the decision-maker in the foreign policy area. He also discusses sensitively the impact which an individual's official position in the State Department hierarchy has upon his perspective as he approaches a decision.

Some of the other sections are less impressive. In his commendable effort to provide a comprehensive account of the American role, the author frequently fails to distinguish adequately between the trivial and the significant. There is much raw data here and too little critical evaluation of it.

The discussion on activities of the American academic community, for example, includes an account of a petition signed by ninety-nine students from Miami University in Ohio. Student activism, it seems, is not so new and revolutionary as some of its exponents today assume. But how significant was this petition in securing Indian independence? Does it merit a quarter of a page in a book of slightly more than one hundred pages? One has no feeling that the author has distinguished in his research between mere activity and activity which has a significant impact upon historical events. Can he then convincingly specify the impact which America did, indeed, have on Indian independence?

The section on the role of Congress raises similar methodological concerns. The author has made a detailed study of the *Congressional Record* to document the influence of Congress. A study based largely upon insertions in the *Record* can, however, be exceedingly misleading as anyone who has served as a Congressional intern and prepared items for inclusion in the *Record* knows. Mr. Hope provides a wealth of raw data from the *Record,* but he fails adequately to evaluate the nature and strength of the Congressional concern and, as a result, its impact on the achievement of Indian independence. He does, however, understand the role of Congress in this situation—that of a forum through which voices supporting Indian independence could speak.

The best and most valuable portion of the book

is concerned with the activities of three unusual diplomats during World War II. Mr. Hope examines in detail the subtle and sensitive contribution of Sir Girja Shankar Bajpai, the first official representative in Washington of the British Government in India, a man aware of his responsibilities and loyal in his discharge of them, but also an Indian with strong personal convictions. The discussion of Louis Johnson's mission is also good, but the most significant new contribution to our understanding of American involvement in India's struggle for independence comes in the section on Ambassador William Phillips where the author has used material gathered in personal interviews with the former diplomat.

Mr. Hope concludes with what he describes as a theory "to explain how American influence meshed into the pattern of British policy making." He argues that activity by Americans in India's cause "was strongly inspired by progressive elements in British politics." The result was "astute prompting . . . of public statements and private and official diplomacy by Americans at various levels of authority." These statements were then used by the group in the British Parliament as a "principal resource in the campaign to achieve liberalized treatment of Indian demands."

One might question the words, "principal resource," and still agree that this was indeed the American role. That leaves unanswered, however, the significance of the American role in the totality of pressure and events which led to the independence of India.—PHILIP M. PHIBBS, *Wellesley College.*

Arms Control Agreements: Designs for Verification and Organization. By DAVID W. WAINHOUSE in association with BERNHARD G. BECHHOEFER, HARRY D. HUGHES, BYRON V. LEARY, THEODORE ROCKWELL III, ANNE P. SIMONS, AND ARNOLD WOLFERS. (Baltimore: The Johns Hopkins Press, 1968. Pp. 179. $6.95.)

Further evidence of this kind is probably superfluous, but if this book accomplishes nothing else, it should help to disarm the Senatorial inquisition that has asked, for several years now, why the U.S. Arms Control and Disarmament Agency (ACDA) subsidizes work that is more properly the domain of Ph. D. theses. The volume at hand is the work of a team skilled mainly in law and foreign affairs, but including also one technological and one military expert. Their study has assimilated a large and complicated body of technical and political data; analyzed it in four detailed case studies; and derived operating principles to guide the design of an International Disarmament Organization (IDO) to verify the implementation of both narrow and broad gauge arms control measures. Initially a report to the ACDA by the Washington Center of Foreign Policy Research, the work has been revised and condensed for publication.

The book is an important contribution for many reasons. First, it appears at a time when the superpowers and others may escalate the arms race to new levels unless mutually acceptable arms controls can be reached. While political obstacles are more severe than technical ones, solutions to the latter must also be overcome. The Wainhouse study suggests ways to reconcile the U.S. concern with reliability and the Soviet interest in minimum instrusion. To begin with, the authors believe that the intrusion required for adequate reliability is less than earlier assumed. More particularly, if one considers the objects needing inspection given a superpower cut-off of fissionable material production or a Central European atom-free zone, for example, it turns out that a far smaller inspection staff is needed in the Communist than in the Western states concerned. Unless the Communist regimes decide they want parity, this asymmetry might make inspection more palatable for them.

A book such as this may easily be criticized for its emphases and its omissions. More space is given to U.S. proposals such as a production cut-off and a deployment freeze than to Communist proposals. Although the aughors indicate they will not discuss the political-military implications of the Rapacki and Gomulka plans (p. 44), they do in fact provide a negative assessment of these implications (pp. 47–49). By comparison, the book tends to presume the utility of most measures endorsed by the White House (although there is no visible enthusiasm for the fourth case studied: the first stage of the general disarmament plans submitted by Washington and Moscow).

It is unfortunate that this book does not consider the problems that have already arisen regarding compliance with the 1963 nuclear test ban, the 1964 understanding of fissionable material production, and the 1967 space treaty. Surely these difficulties are significant for the proposed IDO. Although Wainhouse has directed another study for ACDA entitled *Alternative Methods for Dealing with Breaches of Arms Control Agreements,* the conclusions of that study are not spelled out in the present work.

Similarly, the book does not consider the suggestions made to surmount the problem of verifying an underground test ban. One would think that such notions as "challenge inspection" and "international detection club" would have relevance to the proposed IDO.

Despite the heroic effort here to bridge the gap between U.S. and Soviet positions, a great distance remains to be travelled. The authors have elucidated their answer to some extremely complex

problems, but alternative answers are also conceivable. The book may spur others (hopefully including Soviet researchers) to produce such answers, while in the interval technological and military developments change the nature of the problem. For example, Wainhouse *et al.* would rely upon gross physical inspection techniques to verify a freeze on strategic delivery vehicles, but is this approach adequate at the moment when multiple warheads could be mounted on each missile?

Looking beyond the immediate concerns of the study, one wonders whether an IDO—*if* created on a model like that recommended here—would not move the nations toward a limited form of world government much more rapidly than fuctional collaboration confined to technical spheres. The IDO inspectorate is supposed merely to report the facts to the great power members of the organization, who, in turn, must determine whether a violation has occurred and what response to make. This distinction is too facile, however, for the content and tone of the reporting would prove to be sensitive political acts. Because of this sensitivity potential, the IDO may never be established; but if it is, nations may well feel strong incentives to keep the system from collapsing.—WALTER C. CLEMENS, JR., *Boston University.*

The Aland Islands Question: Its Settlement by the League of Nations. By JAMES BARROS. (New Haven: Yale University Press, 1968. Pp. 362. $10.00.)

Despite nearly a quarter century of evidence and argument to the contrary, many scholars, most statesmen, and, one suspects, nearly all of the otherwise shrewd men in the street persist in thinking of the United Nations as an appropriate dumping ground for the world's major conflicts. And if the overburdened organization, with almost no resources of power of its own, doesn't come up with a remedy to these often insoluble dilemmas, the Russians are accused of obstructionism or worse, and the organization itself is judged a failure.

Fortunately, though, scholars and statesmen are beginning to recognize that the UN was never expected to resolve clashes between the Great Powers if the peculiar WW II alliance came apart. Increasingly, we are recognizing that the UN should not be adjudged a failure when it doesn't do what it was never meant to do. And there is a slowly growing awareness that it is a mistake to bring all the major world problems to the UN for solution. Often the problems simply are exacerbated there and the organization staggers dangerously under their weight.

But, while the UN has made little contribution to easing Cold War confrontations in Europe and Asia, it has proved quite helpful in resolving, or at least managing, peripheral clashes and problems, most notably with its peace-keeping operations for the Suez, Congo, and Cyprus imbroglios. Under certain rather special conditions, the UN can be a useful instrument of conflict management.

In these general respects, the UN and the League of Nations are quite similar. The League was no more able to stop Hitler and the Japanese imperialists than the UN has been capable of ending the Cold War. But the League did register some modest achievements during its brief lifetime. One of these was the League's contribution to the settlement of the Aland Islands dispute between Finland and Sweden. James Barros has presented us with what must certainly be the definitive analysis of this dispute and its resolution. As such, his book is a welcome addition to the literature on the modest role international organizations can occasionally play in conflict management.

The Aland Islands, a group of small islands in the Baltic Sea, had been disputed territory for over 60 years when both Finland and Sweden claimed them after WW I. When bilateral negotiations broke down in the early summer of 1920, it appeared that Sweden, militarily much stronger than Finland but protective of its neutralist image, might resort to force. At that point, however, the British and French brought the matter to the League which established two special commissions that led to agreement a year later: the islands went to Finland, but a large measure of autonomy was given to the islanders who are Swedish in language and culture. The League enabled the parties to continue the diplomatic struggle in another forum rather than go to war. League involvement provided a safety valve for the tense situation and later a face-saving aura of justice for the compromise settlement. Concessions were made through the League that could not have been made directly by one party to the other. Each government could justify its acceptance of half a loaf since the agreement bore League sanctification. Inis L. Claude has pointed out that this function of collective legitimization has been even further developed by the United Nations. But the rudiments of the process were worked out during the 1920s as Barros demonstrates.

One might have wished for an arrangement of the materials in Professor Barros' book other than the day-by-day, blow-by-blow chronicle which is exhausing as well as exhaustive. Moreover, Professor Barros fails to highlight adequately the key points he wants to make along the way which it is even more necessary to do in a chronologically-ordered account than it is in a topically-organized essay since in the latter form we at least have the

selected topics for clues as to what the author thinks is important. Finally, the surprisingly short final chapter of conclusions is not really the chapter of conclusions about the League's settlement of the Aland Islands problem that we should have; rather, it is a collection of conclusions and assertions about the role of international organizations in today's world that would be more appropriate and more credible at the end of a volume of supporting case studies of League and UN interventions—a book still needed, incidentally.—JAMES A. STEGENGA, *Purdue University.*

European Community Law and Organizational Development. By W. ANDREW AXLINE. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1968. Pp. 214. $7.50.)

There are many possible approaches to studying political integration either as a process or a fait accompli. Mr. Axline has chosen to employ law and organization as relevant measures of determining the existence of European integration or social development toward it. In constructing his theoretical framework and justifying his methods, he appears to argue that a society is built upon organizations and institutions reflecting the existence of "valid" normative (ought) as well as actually "effective" (is) law. These organizations, in turn, can further generate such law. Through this interaction each can promote the other. A point may finally be reached where the nature of the organization, its law-making capacity, and the "effectiveness" of the law evidence a fully integrated society. Evolution of the society toward that point can be recognized by examining first the structure and functions of the society's organization with the methods of the Functionalists; second the existence of "valid" law with the help of the methods elaborated by Analytical Jurisprudence; and third the prevalence of "effective" law as Sociological Jurisprudence understands this. Since the "effectiveness" of law lies in its ability to determine actual behavior of the society's members, it can serve as a mirror of the society's integration. To the extent that it possesses three qualities, to that extent integration can be said to exist. (1) "Effective" law must have the same significance for all members of the European Community. The author is satisfied that in a large measure it does. (2) It must be equally binding upon all members and have equal meaning for them. (3) The Community's law must be superior to the constitutions of the members; it must have primacy within their own legal systems. In this respect, the author finds the situation differs from state to state and only a trend toward the growth of these qualities can be discovered. The data for these findings of the author are the decisions of the national European courts and the Court of Justice of the European Communities. As measured by these criteria, European Community integration remains incomplete. The author thinks his analysis and findings justify the suggestion that national judges have it in their power to further European Community integration by interpreting laws whenever possible in favor of the European Community rather than national sovereignty.

Because of the obfuscous style of the book I cannot be certain that this summary is fair to the author. If it is, I should maintain that the extreme narrowness and formalism of the author's approach can only result in conclusions of limited significance. The vast range of social behavior which may contain important clues to social integration is inadequately reflected in "effective" law, especially if this law is culled only from court decisions. Moreover, upon the author's own grounds, I have serious doubts about the general validity of his particular approach. The nature of behavior revealed by court decisions has little relation to objective or subjective factors leading to or constituting integration. The same overt, objective behavior may or may not be indicative of integration. It is, however, always indicative of "effective" law. How then can "effective" law as a mirror of integration be distinguished from "effective" law arising from any multilateral treaty having no goal of integrating its signatories? No amount of argument about the legal significance of Community law for the national legal systems of the member states—its superiority or primacy—can answer this question. It cannot be answered in legal terms because integration is not a legal question. The answer requires an examination of the motivation and the social context of the behavior. They determine whether a given behavior is related to integration or not. The author recognizes this problem but treats it cavalierly. There is a brief reference, out of context with this particular problem, to an alleged community of feelings among Europeans potentially supportive of integration. The reader may thus be expected to share with the author the assumption that because there are feelings in common within the European Community and because these feelings are allegedly favorable to integration, the "effective" law to be found in the Community makes it identical with integration. However in this case, the integration rests not on a legal foundation but on psychological grounds for which no empirical support is provided.

This is not the only occasion on which the reader must complete the author's argument by assembling points scattered throughout the book. Loose organization, aggravated by indifference to the English language, makes it difficult to follow

the author's thoughts and leads him to be imprecise regarding his central concepts. A few references to particulars must suffice to demonstrate that this criticism is more than subjective reaction.

The concept of integration is crucial to the author's presentation. It is nowhere analyzed in depth or made operational. On page two it is called a "fundamentally sociological concept," on page six a "psychological" concept. On page fourteen integration is declared indentical with "effective" law while on page 62 "effective" law is said to represent "political integration." Definitions by other authors which Mr. Axline quotes fail to clarify his own understanding and use of the concept of integration and are, in fact, ignored in his analysis. The literature most relevant to Mr. Axline's subject—for instance Rudolf Smend's *Verfassung and Verfassungsrecht,* Hermann Heller's writings, and the vast literature on legal philosophy—is nowhere taken into account. "Cohesion," "sense of community," "solidarity," "shared values" are unquestioningly accepted as prerequisites of integration, but either not defined or not operationalized. Yet the author does not hesitate to talk about them in quantitative terms: "amounts" and "degrees" of integration, "a great deal" of solidarity, etc. (pp. 3, 5, 83, 96). Conclusions are drawn from such undefined concepts as "primitive" or "elementary" societies and law. No meaning is given to such terms as "stable legal systems," a "real common market," a "positive peace," a "higher order" of rules (pp. 7, 70, 84, 95). And not least as long as language is an important tool of one's trade: the English language is violated by dangling participles, misused prepositions, ambiguous referents of such pronouns as "this" and "it," and the inexact use of words (there are several "primary" concerns; "in terms of" is used ad nauseam—ten times on page 64—and often incorrectly).

In principle Mr. Axline's project is interesting and has some value for the study of social integration. One can only wish that the author had handled it with more precision, in a wider context, and with a better sense of aesthetics.—WERNER LEVI, *University of Hawaii.*

International Systems and the Modernization of Societies. By J. P. NETTL AND ROLAND ROBERTSON. (New York: Basic Books, 1968. Pp. 216. $5.95.)

Apriori frameworks for the analysis of political development are in such abundance nowadays that each new proposal tends to be perceived as having declining marginal utility within the literature. The present volume, though aimed at presenting new schemas, tends to be more of an eclectic adventure in ideas. The three chapters are separate essays that for the most part do not follow cumulatively or logically from one another but instead explore a number of problems, both conceptual and theoretical. Just as the commentary becomes most exciting and promising one's train of thought is terminated abruptly by fascinating footnote references. No specific connection between their ideas and a strategy for empirical research is provided, despite their recognition in the preface of the need for empirical testing. Illustrative material is used in order to point out conceptual difficulties rather than to concretize or to clinch arguments.

Chapter I points out pitfalls in defining three terms—industrialization, modernization, and development—an exercise more in keeping with concept formation by intuition than by postulation. The definitions, the authors claim, are more in accord with their actual usage within developing countries than with the alternative frameworks in vogue among academicians, an assertation that is not matched with empirical evidence.

Chapter II sets forth a paradigm, based upon the legal concept of inheritance, which attempts to account for two postcolonial alternatives—reform and revolution. Elite (or mass public) perceptions of the role of the newly independent government vis-à-vis the departing imperial power, especially the level of hostility held by the former toward the latter are postulated as predictors of revolutionary inheritance, in which militant groups contest the will drawn up by the power granting independence to a former colony. Although the authors admit that their paradigm "does not apply to Latin America," and their explication lacks references to actual statements by decision makers in the newer states, their stress on the attitudinal component of political development remedies a lack of such a *systematic* focus among those currently applying the frameworks of Almond and Riggs.

Chapter III proposes a societal system framework for analyzing international systems, and it overzealously purports to solve the problem of linkage between the political subsystem and other subsystems of the international societal system by concentrating on attitudinal cleavages. The most discussed dependent variables are international stratification systems. Galtung's rank disequilibrium approach is turned upside down into a quest for factors that account for compliance or noncompliance to the existing stratification systems on the part of various types of states.

Alternative natural languages and approaches within the social sciences, however, can best be evaluated in terms of their role in an ongoing research process. A proposal for a new symbol sys-

tem is always welcome if it alerts a scholarly community to a new field for inquiry or if it orders previous partial frameworks into a homogeneous synthesis. After alternative languages (such as those of Almond and Riggs) reach the stage of competing schools of thought, and once empirical research has begun, a new language may often be suggested out of an awareness for the procrusteanness of the models as guides to the reality under investigation. Whereas the authors of this volume appear to feel that no previous framework adequately encompasses evaluative aspects of domestic political and international societal development, one could counter that now is the time for research to uncover data that are consistent or inconsistent with the propositional theories advanced by Almond, Riggs, and others, which do in fact have attitudimal orientations. Whether more scholars will take the path of empirical research or will prefer to await the formulation of additional natural languages may be a loaded question, yet it will be asked by the reader of this preliminary formulation.—MICHAEL HAAS, *University of Hawaii.*

Conference Diplomacy. By JOHN KAUFMANN. (Leyden: A. W. Sijthoff, 1968. Pp. 222. $6.90.)

Given the ever increasing number of international conferences, a volume on conference diplomacy does not seem untimely. The author of this book, a Dutch diplomat of considerable experience, presents essentially a guide or handbook to the organization and operation of international conferences.

Hardly an element of conference diplomacy is missed, though some are dealt with in a rather cursory fashion. The variety of delegations, their composition, and their organization are examined, as well as the requirements and characteristics of conference diplomats. Special attention is given a somewhat neglected area of scholarship, the role of the secretariat in conference diplomacy. This emphasis reflects, perhaps, the increasing importance of secretariats as international conferences become larger and their subject matter more complex. Special attention is also given the role of the presiding officer at international conferences.

Other sections of Kaufmann's book are concerned with some of the principal areas of conflict at international conferences, conference leadership—by individual delegations, the president, an "inner circle," groups, and the secretariat, and types and functions of groups and their role in the negotiating process.

This book in many ways fails to capture the dynamics of conference diplomacy, however. Granted, discussions of missions, protocol, the nationality of delegations and the like are not very stimulating subjects, but when the reader gets to a section on tactics, he expects more than he finds in this volume. Most of the methods employed by diplomats to get a proposal adopted or to cause it to be rejected are mentioned, but the description is of rather static processes. The reader might also expect a little more from a chapter on dicision-making. And one wonders why, if it is to be included at all, a discussion of summit diplomacy is allotted only thirteen lines. We learn here, simply, that decisions of an intergovernmental conference are not usually affected by the presence of a head of state or a head of government.

The last chapter of this book deals with three case studies in conference diplomacy, and this is the author's most successful chapter. The first two sections are concerned with trade negotiations and disarmament negotiations, and include a summary of developments in each of these areas since the Second World War. In section three the author compares trade and disarmament negotiations, the General Agreement on Tarriffs and Trade, especially the Kennedy Round and the Eighteen-Nation Disarmament Committee focusing on the elements of conference diplomacy examined in the preceding pages, such as the role of the large powers, the role of the smaller powers, participation of UN organs and the Secretariat, group activities, outside influences, and publicity and privacy. The final section of this chapter analyzes an international conference held in February, 1967, called by the Netherlands to consider Indonesia's economic future. Again the discussion centers on the elements mentioned earlier in the book.

Kaufmann suggests in this chapter that contrary to the disarmament negotiations where it was the basic interests of the super-powers that caused constraints, in the Kennedy Round it was the structure of the negotiating machinery that became a delaying factor. The author's analysis of the Kennedy Round negotiations also reveals the special role that the Director-General of GATT played in bringing about a compromise agreement when negotiations on certain major issues seemed at a standstill.

Kaufmann's third case study is perhaps his best choice. This section could be subtitled: "How to Have a Successful International Conference." Kaufmann asserts that the success of the conference concerned with Indonesia's economic problems was due to the fact that the "right conference diplomacy techniques" were employed. He lists the following: careful preparation; limited objective and simple agenda; the right type of documents ("neither too voluminous nor too brief"); unity of meeting place and delegate residence; high quality of services; efficient chairmanship; and full cooperation of important interna-

tional organizations interested in the subjects discussed.

Conference Diplomacy is especially strong on the organizational aspects of international conferences. In a sense, as already suggested, it is a "How to" book. It also convincingly indicates how intergovernmental conferences have modified some of the traditional methods of diplomacy. The book seems to take for granted, however, that international conferences are good and useful. At no point does the author take a hard look at some of the disadvantages of conference diplomacy. This is a major omission. Kaufmann has put together a rather complete book but not a very stimulating one. Perhaps this is because of his attempt to keep the book concise. Concise it is, though the illustrations used are excellent and the notes indicate a thorough familiarity with the literature on the subject and on international organization generally. Exactly for whom this work was written is not quite clear. It seems rather elementary even for those technical experts or private citizens who might be attending an international conference for the first time, a use suggested in the Forward. This book will, also, not detain the scholar long. Perhaps it will find its greatest use in an introductory international organization or international relations course in conjunction with other works.—JOHN A. EILERS, *Arizona State University.*

Capitulación sin guerra. By FERDINAND OTTO MIKSCHE. (Madrid: Instituto de Estudios Políticos. Translation from the French and Preface by Luis Cano Portal. 1967. Pp. 273.)

In the first part of the book Mr. Miksche discusses the present situation of the West since the floodgates were opened to Soviet Imperialism in 1945, and he feels that the initiative has passed to Moscow after the Suez crisis of 1956. Two factors are of prime importance in this development. One, internal, the exaggeration of liberty, has turned into increasing Byzantine confusion and, together with pacisfism, appeasement and paralysis, extends its influence all over the Western world, so that the West is well on its way to perish despite its unsurpassed economic and technical advances. The other, external, concerns decolonization, relentlessly pursued by the Soviet Union and the United States. He feels that the early departure of the Western powers from several strategic areas was just as unwise as allowing the Soviets to get as far as the Elbe, and the author blames the Europeans for their lack of solidarity and the Americans for their political naivete. In view of growing Soviet threats and the different strategic concentration of Western Europe and the United States, whose interests are no longer identical, a united Western Europe is an absolute *sine qua non,* otherwise Europe will not survive the new division of the world by the three superpowers and the extraordinary changes in the world which will develop dramatically the closer we come to the year 2000.

The second part of the book which deals with the security problems of Western Europe, especially the success of Soviet co-existance policies after the death of Stalin resulting in the psychological disarmament of Western Europe, is essentially a realistic appraisal of the situation. Nuclear dissuasion is considered by the author as bluff, since he believes neither side would ever employ atomic weapons; the balance of terror by necessity has given conventional forces a greater significance. Moreover, United States strategy is meant to be purely defensive and therefore, the author feels, is condemned to a progressive weakening.

The author discusses the decline of NATO which he links essentially to the refusal of the United States to treat France on a par with Britain and to the "nuclear complicity" between Washington and Moscow. Should the United States continue with this policy it will not only lose control over the production but likewise over the military use of the atom. Instead of pursuing these aims, the United States should put the Soviets on notice that, since it cannot stay forever in Europe, it intends to help Western Europe with nuclear arms. The author does not foresee any major war in Central Europe, but instead a great likelihood of civil wars throughout the world, possibly in Germany or the Mediterranean. Therefore, Western strategy, he asserts, is not realistic, since NATO offers no guarantees, and in the mind of the masses the security of Western Europe is identified with a line—the Iron Curtain—whereas what should be defended are not geometrical lines but geographical positions outside the geographic limitations of NATO. Hence, a global outlook is necessary, otherwise the West will not be able to survive. The idea that in a world of quick change nations can defend their interests with a purely defensive attitude is nothing but a dream.

For a reform of NATO the author proposes: *De-Americanization* (defense of Europe by Europeans), *integration* (Continental Western Europe), and *simplification* (armaments). The new NATO would be divided into two blocks: one, *Anglo-American* (United States, Canada, Britain, Iceland), the other, *Western European* (France, West Germany, Benelux, Italy, Spain). Besides these *active* members, there would be *associate* members like Norway or Greece, countries which would not enjoy the treaty's guarantees. It might also be possible to establish a Nordic Defense Community with Norway, Denmark and neutral Sweden. These are certainly interesting proposals which

seem to offer some solution to the present disagreements between France and the United States.

This reviewer feels that Mr. Miksche's analysis of the West's decline is valid, since nowhere have we seen so far any successful reaction to Soviet strategy. On the other hand, while certainly the wisdom of the United States' refusal to aid France in the development of its nuclear arsenal and to treat it on an equal basis with Britain can be questioned, it is also true that the French were equally not willing to share nuclear weapons with West Germany, a fact that Mr. Miksche omits in his discussion. Finally, while the inclusion of Spain into the new NATO seems to this reviewer pertinent and logical, it is doubtful that it will be accepted, since many Western Europeans and Americans seem to be more obsessed with pure democracy in Spain (and Greece) than with the dangerous threat from the East.

All in all, a fascinating and passionately-written book which should receive the widest circulation.—O. CARLOS STOETZER, *Fordham University.*

ANNOUNCEMENTS

The sixty-fifth Annual Meeting of the Association will be held September 2–6, 1969, at the Commodore Hotel, New York City.

1969 ANNUAL MEETING AND SLATE OF OFFICERS FOR 1969–70

The 1969 Annual Meeting of the Association will be held September 2–6 at the Commodore Hotel, New York City. Professor Robert E. Lane, Yale University, is Chairman of the Program Committee.

At the Annual Business Meeting of the Association, to be held at 4:00 P.M. Wednesday, September 3 (the room will be announced in the Final Program) the Nominating Committee will propose a slate of officers for 1969–70. The Committee is composed of three members appointed by President Merle Fainsod: Harry Eckstein, Princeton University; Leon D. Epstein, University of Wisconsin; and William S. Livingston, University of Texas; and three members appointed by President David Easton: Frederic N. Cleaveland, University of North Carolina, Chairman; Duncan MacRae, University of Chicago; and Sheldon S. Wolin, University of California, Berkeley. The Committee will propose the following officers for 1969–70:

President Elect: Robert E. Lane, Yale University

Vice Presidents: Grant McConnell, University of California, Santa Cruz

William H. Riker, University of Rochester

Robert E. Ward, University of Michigan

Secretary: Thomas R. Dye, Florida State University

Treasurer: Francis E. Rourke, Johns Hopkins University

Members of the Council for two years:

Samuel P. Huntington, Harvard University

Henry S. Kariel, University of Hawaii

John H. Kessel, Allegheny College

Lewis Lipsitz, University of North Carolina

John Meisel, Queens University, Kingston

Jewel L. Prestage, Southern University

Robert H. Salisbury, Washington University, St. Louis

Ellis Waldron, University of Montana

PROGRAM COMMITTEE: 1970 ANNUAL MEETING

The 1970 Annual Meeting of the Association will be held in Los Angeles, California, September 1–5 (the hotel will be announced later). Suggestions for papers to be presented at the 1970 Annual Meeting are earnestly solicited. Unlike previous years, the 1970 program will comprise more than two dozen sections, each of which will have as many as four panels, with each panel providing an opportunity for one or more papers. No section chairman can possibly be familiar with all of the work going on in his domain. Thus it becomes necessary for scholars whose work is just coming to fruition to make this work known either to the Program Chairman or to the relevant section chairmen. This is particularly true when the research forms part of a dissertation currently under way.

Please advise the Program Chairman or the section chairmen listed below of research which, either now or within a year, will very likely form the basis for a high quality research report at the meetings. Since the program is currently being shaped, it is not too early to anticipate events more than a year in the future.

Chairman, Richard L. Merritt, University of Illinois

A. *General*

American Political Science: Growth and Prospects, 1945–1970–1995—Austin Ranney, University of Wisconsin

Political Knowledge and Policy Decisions—Davis B. Bobrow, Office of the Deputy Director, Research and Technology (Room 3D 1021, The Pentagon)

Formal Political Analysis .....Hayward R. Alker, Jr., Massachusetts Institute of Technology

History of Theory .....Judith N. Shklar, Harvard University

Data and Analysis .....

B. Interaction among National Systems

Patterns of Interaction .....Ole R. Holsti, University of British Columbia

Community-Building .....Ernst B. Haas, University of California, Berkeley

Strategy and Bargaining .....Jeremy J. Stone, Council on Foreign Relations Fellow (mailing address % APSA, Washington, D.C.)

Conflict and Peace Research ..... Bruce M. Russett, Yale University

International Communication .....Richard L. Merritt, University of Illinois

C. Cross-National Comparisons of System Performance

Cross-National Comparison .....Samuel H. Barnes, University of Michigan

Comparative Communist Systems .....Jan F. Triska, Stanford University

Foreign Policy: Formulation and Implementation .....
.....Wolfram F. Hanrieder, University of California, Santa Barbara

Political Development .....Martin Kilson, Harvard University

Civil Violence and Revolution .....Raymond Tanter, University of Michigan

State and Local Government .....Thomas J. Anton, University of Michigan

Urban and Metropolitan Affairs .....
.....Robert R. Alford, Sociology Department, University of Wisconsin

D. Cross-National Comparisons of National Processes

Political Socialization .....
..... Fred I. Greenstein, Wesleyan University (current address: Government Department, University of Essex, Colchester, Essex, England)

Parties and Politics .....Theodore J. Lowi, University of Chicago

Leadership .....
.....James David Barber, Yale University (current address: Center for Advanced Study in the Behavioral Sciences, Stanford, California)

Legislatures and Legislative Behavior .....G. Robert Boynton, University of Iowa

Administrative Behavior .....Robert T. Golembiewski, University of Georgia

Judicial Systems and Behavior .....Stuart S. Nagel, University of Illinois

Intergovernmental Relations .....Samuel H. Beer, Harvard University

NOTICE

RESOLUTIONS AND CONSTITUTIONAL AMENDMENTS AT THE ANNUAL BUSINESS MEETING

In accordance with the Constitution of the American Political Science Association, the attention of members of the Association is hereby directed to the provisions of the Constitution that:

"All resolutions shall be referred to the Council for its recommendations before submission to the vote of the Association at its Annual Business Meeting." (Article VIII)

"Amendments to this Constitution shall be proposed by the Council and adopted by a majority vote of the members present at any regular or special meeting of the Association.

"Ten members of the Association may propose amendments to this Constitution. Such proposed amendments shall be referred to the Council for consideration, and by them referred to the Association for its action thereon at the next Annual Business Meeting with such recommendations as the Council may see fit to make." (Article IX)

The Council of the Association will meet during the afternoon of Sunday, August 31, the afternoon of Monday, September 1, and the morning of Tuesday, September 2 (the room will be announced in the Final Program).

The Constitution of the Association is printed in both the 1961 and 1968 editions of the Association's Biographical Directory.

The American Political Science Association's Committee on the Status of Women in the Profession will hold an open meeting at the Annual Meeting on Wednesday, September 3, at 11:30 a.m. The purpose of this meeting will be to review (1) the problems faced by women in the profession, (2) questionnaires concerning these problems which will be sent to members of the profession, and (3) possible means of dealing with these problems.

ERRATUM

In Arthur S. Goldberg's article on "Social Determinism and Rationality as Bases of Party Identification," published in the issue of March, 1969, line 4 in footnote 17, p. 10, should read: "see Arthur S. Goldberger, *Econometric Theory. . . .*"

ARTICLES ACCEPTED FOR FUTURE PUBLICATION*

September, 1969

Graham T. Allison, Harvard University, "Conceptual Models and the Cuban Missile Crisis"

Charles F. Cnudde and Donald J. McCrone, University of Wisconsin, "Party Competition and Welfare Policies in the American States"

Wayne A. Cornelius, Jr., Stanford University, "Urbanization as an Agent in Latin American Political Instability: The Case of Mexico"

Ralph M. Goldman, San Francisco State College, "A Transactional Theory of Political Integration and Arms Control"

Henry S. Kariel, University of Hawaii, "Expanding the Political Present"

Manfred Kochen, University of Michigan, and Karl W. Deutsch, Harvard University, "To-

* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

ward a Rational Theory of Decentralization: Some Implications of a Mathematical Approach"

Richard M. Merelman, University of Wisconsin, "The Development of Political Ideology: A Framework for the Analysis of Political Socialization"

Norman H. Nie, University of Chicago, G. Bingham Powell, University of California, Berkeley, and C. Kenneth Prewitt, University of Chicago, "Social Structure and Political Participation: Developmental Relationships, II"

Nelson W. Polsby, University of California, Berkeley, Miriam Gallaher, Center for Advanced Study in the Behavioral Sciences, and Barry Spencer Rundquist, Stanford University, "The Growth of the Seniority System in the U.S. House of Representatives"

Ira Sharkansky, University of Wisconsin, and Richard I. Hofferbert, Cornell University, "Dimensions of State Politics, Economics, and Public Policy"

Jack L. Walker, University of Michigan, "The Adoption of Innovations by the American States"

Michael Walzer, Harvard University, "Prisoners of War: Does the Fight Continue After the Battle?"

December, 1968

James W. Clarke, Florida State University, "Environment, Process and Policy: A Reconsideration"

Philip E. Converse and Warren E. Miller, University of Michigan, "The 1968 Elections"

Jeff Fishel, American University, "Party, Ideology, and the Congressional Challenger"

John G. Gunnell, State University of New York, Albany, Arthur S. Goldberg, University of Rochester, and A. James Gregor, University of California, Berkeley, Symposium on "Explanation, Deduction, and Social Scientific Inquiry"

Alex Inkeles, Harvard University, "Participant Citizenship in Six Developing Countries"

Edward J. Mitchell, Institute for Advanced Study, "Some Econometrics of the Huk Rebellion"

John E. Mueller, University of Rochester, "Voting on the Propositions: Ballot Patterns and Historical Trends in California"

James C. Scott, University of Wisconsin, "Corruption, Machine Politics, and Social Change"

Michael J. Shapiro, University of California, Berkeley, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives"

W. Phillips Shively, Yale University, " 'Ecological' Inference: The Use of Aggregate Data to Study Individuals"

Sheldon S. Wolin, University of California, Berkeley, "Political Theory as a Vocation"

March, 1970

Joel D. Aberbach and Jack L. Walker, University of Michigan, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan"

Barry Ames, Stanford University, "Bases of Support for Mexico's Dominant Party"

Michael Haas, University of Hawaii, "International Subsystems: Stability and Polarity"

Howard D. Hamilton, Bowling Green State University, "Direct Legislation: Some Implications of Open Housing Referenda"

John E. Mueller, University of Rochester, "Presidential Popularity from Truman to Johnson"

Paul F. Power, University of Cincinnati, "On the Grounds for Civil Disobedience within American Democratic Thought"

Kenneth Prewitt, University of Chicago, "Political Ambition and Democratic Accountability"

William H. Riker, University of Rochester, "Rational Behavior in Politics"

Patrick Riley, Harvard University, "A Possible Explanation of Rousseau's General Will"

Stanley Rothman, Smith College, "Barrington Moore and the Dialectics of Revolution"

Martin Shubik, Yale University, "Voting, or a Price System in a Competitive Market Structure"

Duff Spafford, University of Saskatchewan, "The Electoral System of Canada"

Professional Placement Service at 1969 Annual Meeting

(September 2-6, 1969)

A professional placement service will again be available to members of The American Political Science Association attending the New York meeting. The placement service will be provided by the U. S. Employment Service. The primary purpose is to provide an efficient means for employers to meet prospective employees and vice versa.

The Professional Placement Service is entirely separate from the APSA's Personnel Service and requires separate registration. There is no fee for use of the Professional Placement Service by employers or applicants at the Convention.

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If you plan to attend the Conference and utilize the Placement Service, please mail the form below as soon as possible, *but not later than August 1, 1969.* Application and/or employer order forms will be forwarded to you upon receipt of the following information.

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The Tour-Seminar will leave July 14 and return August 18 for a 35 day tour which will include: Budapest, Sophia, Bucharest, Moscow, Leningrad, Warsaw, Prague, and Berlin.

COST:

The cost to Association members under a group plan with a minimum of 25 participants will be under $1,600.00. Included in the fare will be round-trip tourist air transportation, first class hotel accommodations, three meals a day except in Berlin where only breakfast will be included, baggage handling, sightseeing, and airport and meeting transportation. (On the basis of past experience, participants can expect that the cost of this educational activity will be accepted as deductible expense on Federal tax return).

SEMINAR PROGRAM:

In cooperation with the governments of the nations on the itinerary and the relevant U.S. departments, the Association will arrange meetings with leading public officials, members of university faculties, U.S. Foreign Service Personnel, and representatives of the press.

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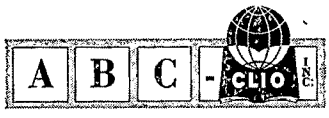
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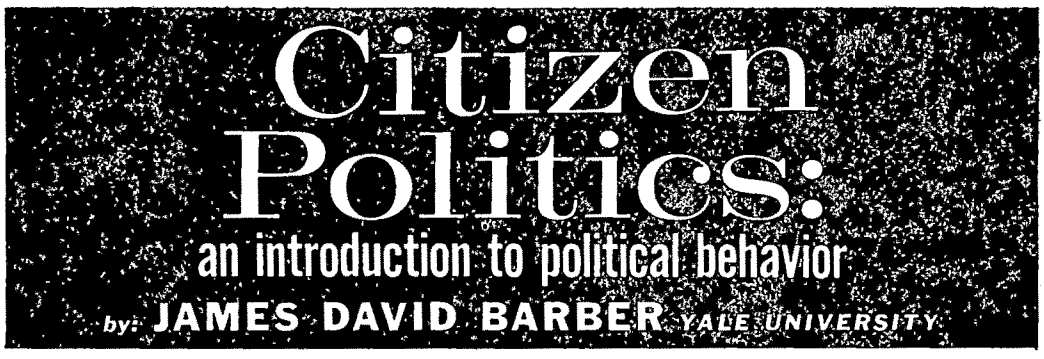
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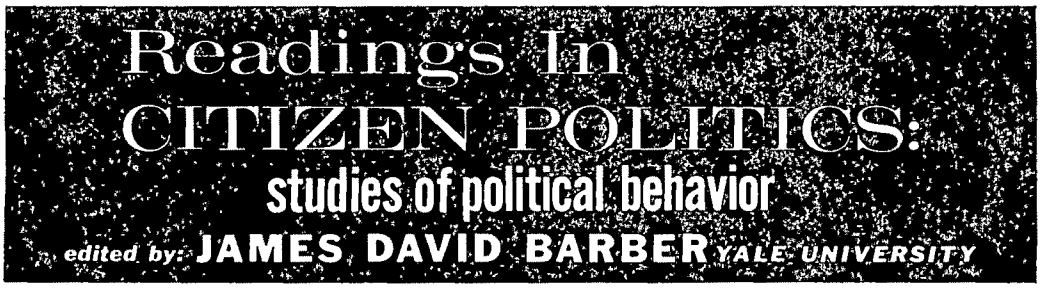
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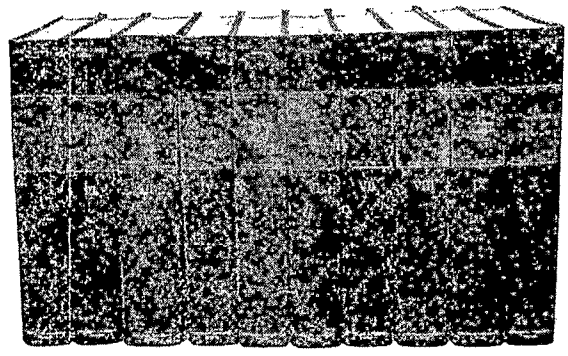
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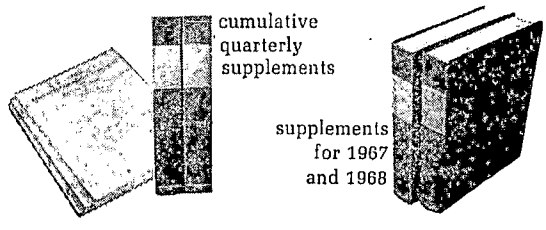
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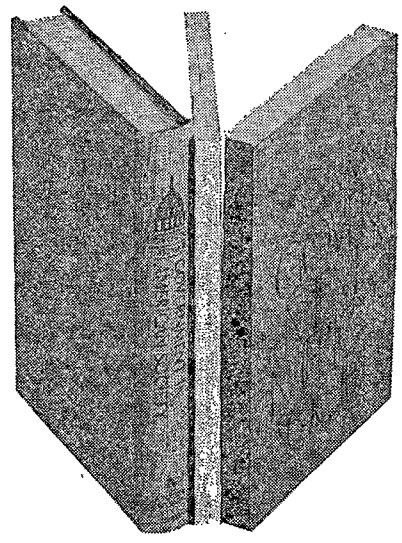
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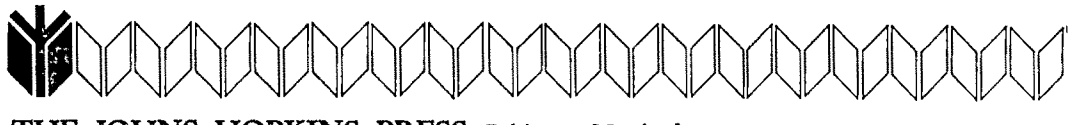
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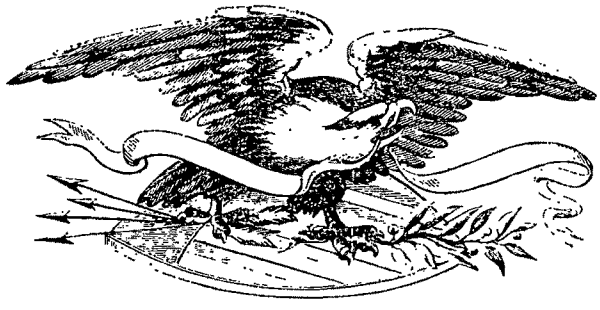
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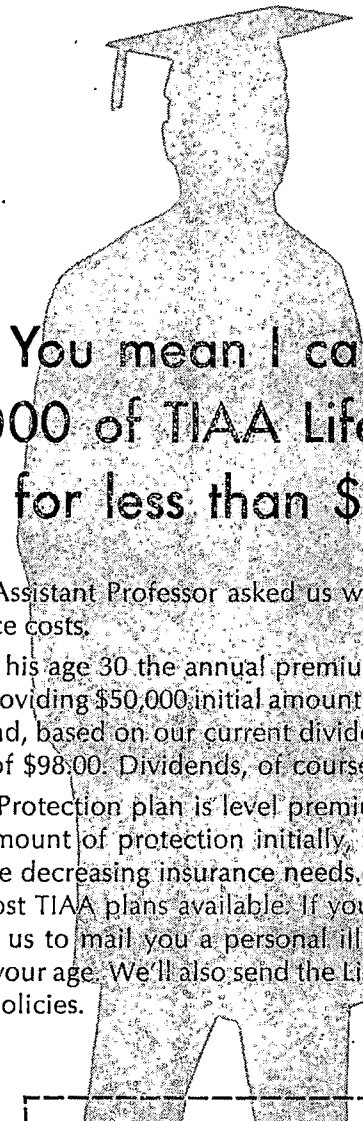
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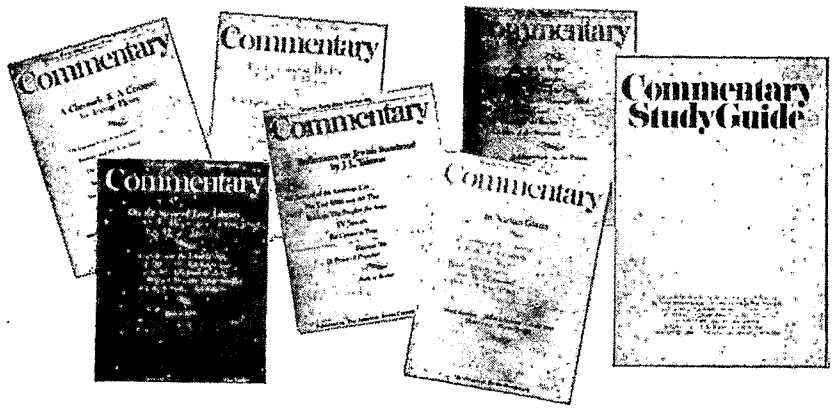
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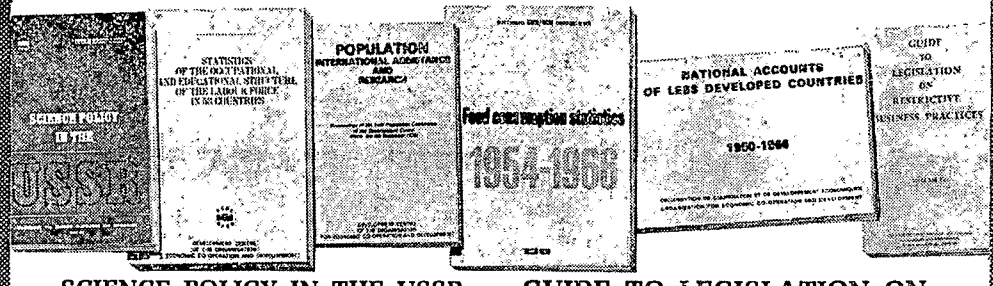
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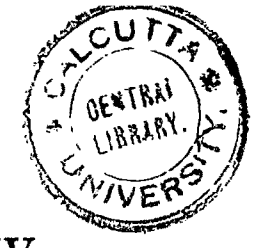
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CONCEPTUAL MODELS AND THE CUBAN MISSILE CRISIS*

GRAHAM T. ALLISON
Harvard University

The Cuban missile crisis is a seminal event. For thirteen days of October 1962, there was a higher probability that more human lives would end suddenly than ever before in history. Had the worst occurred, the death of 100 million Americans, over 100 million Russians, and millions of Europeans as well would make previous natural calamities and inhumanities appear insignificant. Given the probability of disaster—which President Kennedy estimated as "between 1 out of 3 and even"—our escape seems awesome.[1] This event symbolizes a central, if only partially thinkable, fact about our existence. That such consequences could follow from the choices and actions of national governments obliges students of government as well as participants in governance to think hard about these problems.

Improved understanding of this crisis depends in part on more information and more probing analyses of available evidence. To contribute to these efforts is part of the purpose of this study. But here the missile crisis serves primarily as grist for a more general investigation. This study proceeds from the premise that marked improvement in our understanding of such events depends critically on more self-consciousness about what observers bring to the analysis. What each analyst sees and judges to be important is a function not only of the evidence about what happened but also of the "conceptual lenses" through which he looks at the evidence. The principal purpose of this essay is to explore some of the fundamental assumptions and categories employed by analysts in thinking about problems of governmental behavior, especially in foreign and military affairs.

The general argument can be summarized in three propositions:

1. Analysts think about problems of foreign and military policy in terms of largely implicit conceptual models that have significant consequences for the content of their thought.[2]

Though the present product of foreign policy analysis is neither systematic nor powerful, if one carefully examines explanations produced by analysts, a number of fundamental similarities emerge. Explanations produced by particular analysts display quite regular, predictable features. This predictability suggests a substructure. These regularities reflect an analyst's assumptions about the character of puzzles, the categories in which problems should be considered, the types of evidence that are relevant, and the determinants of occurrences. The first proposition is that clusters of such related assumptions constitute basic frames of reference or conceptual models in terms of which analysts

* A longer version of this paper was presented at the Annual Meeting of the American Political Science Association, September, 1968 (reproduced by the Rand Corporation, P-3919). The paper is part of a larger study, scheduled for publication in 1969 under the title *Bureaucracy and Policy: Conceptual Models and the Cuban Missile Crisis*. For support in various stages of this work I am indebted to the Institute of Politics in the John F. Kennedy School of Government and the Center for International Affairs, both at Harvard University, the Rand Corporation, and the Council on Foreign Relations. For critical stimulation and advice I am especially grateful to Richard E. Neustadt, Thomas C. Schelling, Andrew W. Marshall, and Elisabeth K. Allison.

[1] Theodore Sorensen, *Kennedy* (New York, 1965), p. 705.

[2] In attempting to understand problems of foreign affairs, analysts engage in a number of related, but logically separable enterprises: (a) description, (b) explanation, (c) prediction, (d) evaluation, and (e) recommendation. This essay focuses primarily on explanation (and by implication, prediction).

both ask and answer the questions: What happened? Why did the event happen? What will happen?[3] Such assumptions are central to the activities of explanation and prediction, for in attempting to explain a particular event, the analyst cannot simply describe the full state of the world leading up to that event. The logic of explanation requires that he single out the relevant, important determinants of the occurrence.[4] Moreover, as the logic of prediction underscores, the analyst must summarize the various determinants as they bear on the event in question. Conceptual models both fix the mesh of the nets that the analyst drags through the material in order to explain a particular action or decision and direct him to cast his net in select ponds, at certain depths, in order to catch the fish he is after.

2. Most analysts explain (and predict) the behavior of national governments in terms of various forms of one basic conceptual model, here entitled the Rational Policy Model (Model I).[5]

In terms of this conceptual model, analysts attempt to understand happenings as the more or less purposive acts of unified national governments. For these analysts, the point of an explanation is to show how the nation or government could have chosen the action in question, given the strategic problem that it faced. For example, in confronting the problem posed by the Soviet installation of missiles in Cuba, rational policy model analysts attempt to show how this was a reasonable act from the point of view of the Soviet Union, given Soviet strategic objectives.

3. Two "alternative" conceptual models, here labeled an Organizational Process Model (Model II) and a Bureaucratic Politics Model (Model III) provide a base for improved explanation and prediction.

Although the standard frame of reference has proved useful for many purposes, there is powerful evidence that it must be supplemented, if not supplanted, by frames of reference which focus upon the large organizations and political actors involved in the policy process. Model I's implication that important events have important causes, i.e., that monoliths perform large actions for big reasons, must be balanced by an appreciation of the facts (a) that monoliths are black boxes covering various gears and levers in a highly differentiated decision-making structure, and (b) that large acts are the consequences of innumerable and often conflicting smaller actions by individuals at various levels of bureaucratic organizations in the service of a variety of only partially compatible conceptions of national goals, organizational goals, and political objectives. Recent developments in the field of organization theory provide the foundation for the second model. According to this organizational process model, what Model I categorizes as "acts" and "choices" are instead *outputs* of large organizations functioning according to certain regular patterns of behavior. Faced with the problem of Soviet missiles in Cuba, a Model II analyst identifies the relevant organizations and displays the patterns of organizational behavior from which this action emerged. The third model focuses on the internal politics of a government. Happenings in foreign affairs are understood, according to the bureaucratic politics model, neither as choices nor as outputs. Instead, what happens is categorized as *outcomes* of various overlapping bargaining games among players arranged hierarchically in the national government. In confronting the problem posed by Soviet missiles in Cuba, a Model III analyst displays the perceptions, motivations, positions, power, and maneuvers of principal players from which the outcome emerged.[6]

[3] In arguing that explanations proceed in terms of implicit conceptual models, this essay makes no claim that foreign policy analysts have developed any satisfactory, empirically tested theory. In this essay, the use of the term "model" without qualifiers should be read "conceptual scheme."

[4] For the purpose of this argument we shall accept Carl G. Hempel's characterization of the logic of explanation: an explanation "answers the question, '*Why* did the explanadum-phenomenon occur?' by showing that the phenomenon resulted from particular circumstances, specified in C_1, C_2, . . . C_k, in accordance with laws L_1, L_2, . . . L_r. By pointing this out, the argument shows that, given the particular circumstances and the laws in question, the occurrence of the phenomenon was to be *expected;* and it is in this sense that the explanation enables us to understand why the phenomenon occurred." *Aspects of Scientific Explanation* (New York, 1965), p. 337. While various patterns of explanation can be distinguished, *viz.,* Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation,* New York, 1961), satisfactory scientific explanations exhibit this basic logic. Consequently prediction is the converse of explanation.

[5] Earlier drafts of this argument have aroused heated arguments concerning proper names for these models. To choose names from ordinary language is to court confusion, as well as familiarity. Perhaps it is best to think of these models as I, II, and III.

[6] In strict terms, the "outcomes" which these three models attempt to explain are essentially actions of national governments, i.e., the sum of activities of all individuals employed by a government relevant to an issue. These models focus not on a state of affairs, i.e., a full description of the

A central metaphor illuminates differences among these models. Foreign policy has often been compared to moves, sequences of moves, and games of chess. If one were limited to observations on a screen upon which moves in the chess game were projected without information as to how the pieces came to be moved, he would assume—as Model I does—that an individual chess player was moving the pieces with reference to plans and maneuvers toward the goal of winning the game. But a pattern of moves can be imagined that would lead the serious observer, after watching several games, to consider the hypothesis that the chess player was not a single individual but rather a loose alliance of semi-independent organizations, each of which moved its set of pieces according to standard operating procedures. For example, movement of separate sets of pieces might proceed in turn, each according to a routine, the king's rook, bishop, and their pawns repeatedly attacking the opponent according to a fixed plan. Furthermore, it is conceivable that the pattern of play would suggest to an observer that a number of distinct players, with distinct objectives but shared power over the pieces, were determining the moves as the resultant of collegial bargaining. For example, the black rook's move might contribute to the loss of a black knight with no comparable gain for the black team, but with the black rook becoming the principal guardian of the "palace" on that side of the board.

The space available does not permit full development and support of such a general argument.[7] Rather, the sections that follow simply sketch each conceptual model, articulate it as an analytic paradigm, and apply it to produce an explanation. But each model is applied to the same event: the U.S. blockade of Cuba during the missile crisis. These "alternative explanations" of the same happening illustrate differences among the models—*at work*.[8] A crisis decision, by a small group of men in the context of ultimate threat, this is a case of the rational policy model *par excellence*. The dimensions and factors that Models II and III uncover in this case are therefore particularly suggestive. The concluding section of this paper suggests how the three models may be related and how they can be extended to generate predictions.

MODEL I: RATIONAL POLICY

RATIONAL POLICY MODEL ILLUSTRATED

Where is the pinch of the puzzle raised by the *New York Times* over Soviet deployment of an antiballistic missile system?[9] The question, as the *Times* states it, concerns the Soviet Union's objective in allocating such large sums of money for this weapon system while at the same time seeming to pursue a policy of increasing détente. In former President Johnson's words, "the paradox is that this [Soviet deployment of an antiballistic missile system] should be happening at a time when there is abundant evidence that our mutual antagonism is beginning to ease."[10] This question troubles people primarily because Soviet antiballistic missile deployment, and evidence of Soviet actions towards détente, when juxtaposed in our implicit model, produce a question. With reference to what objective could the Soviet government have rationally chosen the simultaneous pursuit of these two courses of actions? This question arises only when the analyst attempts to structure events as purposive choices of consistent actors.

world, but upon national decision and implementation. This distinction is stated clearly by Harold and Margaret Sprout, "Environmental Factors on the Study of International Politics," in James Rosenau (ed.), *International Politics and Foreign Policy* (Glencoe, Illinois, 1961), p. 116. This restriction excludes explanations offered principally in terms of international systems theories. Nevertheless, this restriction is not severe, since few interesting explanations of occurrences in foreign policy have been produced at that level of analysis. According to David Singer, "The nation state —our primary actor in international relations . . . is clearly the traditional focus among Western students and is the one which dominates all of the texts employed in English-speaking colleges and universities." David Singer, "The Level-of-Analysis Problem in International Relations," Klaus Knorr and Sidney Verba (eds.), *The International System* (Princeton, 1961). Similarly, Richard Brody's review of contemporary trends in the study of international relations finds that "scholars have come increasingly to focus on acts of nations. That is, they all focus on the behavior of nations in some respect. Having an interest in accounting for the behavior of nations in common, the prospects for a common frame of reference are enhanced."

[7] For further development and support of these arguments see the author's larger study, *Bureaucracy and Policy: Conceptual Models and the Cuban Missile Crisis* (forthcoming). In its abbreviated form, the argument must, at some points, appear overly stark. The limits of space have forced the omission of many reservations and refinements.

[8] Each of the three "case snapshots" displays the work of a conceptual model as it is applied to explain the U.S. blockade of Cuba. But these three cuts are primarily exercises in hypothesis generation rather than hypothesis testing. Especially when separated from the larger study, these accounts may be misleading. The sources for these accounts include the full public record plus a large number of interviews with participants in the crisis.

[9] *New York Times*, February 18, 1967.

[10] *Ibid.*

How do analysts attempt to explain the Soviet emplacement of missiles in Cuba? The most widely cited explanation of this occurrence has been produced by two RAND Sovietologists, Arnold Horelick and Myron Rush.[11] They conclude that "the introduction of strategic missiles into Cuba was motivated chiefly by the Soviet leaders' desire to overcome . . . the existing large margin of U.S. strategic superiority."[12] How do they reach this conclusion? In Sherlock Holmes style, they seize several salient characteristics of this action and use these features as criteria against which to test alternative hypotheses about Soviet objectives. For example, the size of the Soviet deployment, and the simultaneous emplacement of more expensive, more visible intermediate range missiles as well as medium range missiles, it is argued, exclude an explanation of the action in terms of Cuban defense—since that objective could have been secured with a much smaller number of medium range missiles alone. Their explanation presents an argument for one objective that permits interpretation of the details of Soviet behavior as a value-maximizing choice.

How do analysts account for the coming of the First World War? According to Hans Morgenthau, "the first World War had its origin exclusively in the fear of a disturbance of the European balance of power.[13] In the period preceding World War I, the Triple Alliance precariously balanced the Triple Entente. If either power combination could gain a decisive advantage in the Balkans, it would achieve a decisive advantage in the balance of power. "It was this fear," Morgenthau asserts, "that motivated Austria in July 1914 to settle its accounts with Serbia once and for all, and that induced Germany to support Austria unconditionally. It was the same fear that brought Russia to the support of Serbia, and France to the support of Russia."[14] How is Morgenthau able to resolve this problem so confidently? By imposing on the data a "rational outline."[15] The value of this method, according to Morgenthau, is that "it provides for rational discipline in action and creates astounding continuity in foreign policy which makes American, British, or Russian foreign policy appear as an intelligent, rational continuum . . . regardless of the different motives, preferences, and intellectual and moral qualities of successive statesmen."[16]

Stanley Hoffmann's essay, "Restraints and Choices in American Foreign Policy" concentrates, characteristically, on "deep forces": the international system, ideology, and national character—which constitute restraints, limits, and blinders.[17] Only secondarily does he consider decisions. But when explaining particular occurrences, though emphasizing relevant constraints, he focuses on the choices of nations. American behavior in Southeast Asia is explained as a reasonable choice of "downgrading this particular alliance (SEATO) in favor of direct U.S. involvement," given the constraint: "one is bound by one's commitments; one is committed by one's mistakes."[18] More frequently, Hoffmann uncovers confusion or contradiction in the nation's choice. For example, U.S. policy towards underdeveloped countries is explained as "schizophrenic."[19] The method employed by Hoffmann in producing these explanations as rational (or irrational) decisions, he terms "imaginative reconstruction."[20]

Deterrence is the cardinal problem of the contemporary strategic literature. Thomas Schelling's *Strategy of Conflict* formulates a number of propositions focused upon the dynamics of deterrence in the nuclear age. One of the major propositions concerns the stability of the balance of terror: in a situation of mutual deterrence, the probability of nuclear war is reduced not by the "balance" (the sheer equality of the situation) but rather by the *stability* of the balance, i.e., the fact that neither opponent in striking first can destroy the other's ability to strike back.[21] How does Schelling support this proposition? Confidence in the contention stems not from an inductive canvass of a large number of previous cases, but rather from two calculations. In a situation of "balance" but vulnerability, there are values for which a rational opponent could choose to strike first, e.g., to destroy enemy capabilities to retaliate. In a

[11] Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy* (Chicago, 1965). Based on A. Horelick, "The Cuban Missile Crisis: An Analysis of Soviet Calculations and Behavior," *World Politics* (April, 1964).

[12] Horelick and Rush, *Strategic Power and Soviet Foreign Policy*, p. 154.

[13] Hans Morgenthau, *Politics Among Nations* (3rd ed.; New York, 1960), p. 191.

[14] *Ibid.*, p. 192.

[15] *Ibid.*, p. 5.

[16] *Ibid.*, pp. 5–6.

[17] Stanley Hoffmann, *Daedalus* (Fall, 1962); reprinted in *The State of War* (New York, 1965).

[18] *Ibid.*, p. 171.

[19] *Ibid.*, p. 189.

[20] Following Robert MacIver; see Stanley Hoffmann, *Contemporary Theory in International Relations* (Englewood Cliffs, 1960), pp. 178–179.

[21] Thomas Schelling, *The Strategy of Conflict*, (New York, 1960), p. 232. This proposition was formulated earlier by A. Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* (January, 1959).

"stable balance" where no matter who strikes first, each has an assured capability to retaliate with unacceptable damage, no rational agent could choose such a course of action (since that choice is effectively equivalent to choosing mutual homicide). Whereas most contemporary strategic thinking is driven *implicitly* by the motor upon which this calculation depends, Schelling explicitly recognizes that strategic theory does assume a model. The foundation of a theory of strategy is, he asserts: "the assumption of rational behavior—not just of intelligent behavior, but of behavior motivated by conscious calculation of advantages, calculation that in turn is based on an explicit and internally consistent value system."[22]

What is striking about these examples from the literature of foreign policy and international relations are the similarities among analysts of various styles when they are called upon to produce explanations. Each assumes that what must be explained is an action, i.e., the realization of some purpose or intention. Each assumes that the actor is the national government. Each assumes that the action is chosen as a calculated response to a strategic problem. For each, explanation consists of showing what goal the government was pursuing in committing the act and how this action was a reasonable choice, given the nation's objectives. This set of assumptions characterizes the rational policy model. The assertion that Model I is the standard frame of reference implies no denial of highly visible differences among the interests of Sovietologists, diplomatic historians, international relations theorists, and strategists. Indeed, in most respects, differences among the work of Hans Morgenthau, Stanley Hoffmann, and Thomas Schelling could not be more pointed. Appreciation of the extent to which each relies predominantly on Model I, however, reveals basic similarities among Morgenthau's method of "rational reenactment," Hoffmann's "imaginative reconstruction," and Schelling's "vicarious problem solving;" family resemblances among Morgenthau's "rational statesman," Hoffmann's "roulette player," and Schelling's "game theorist."[23]

Most contemporary analysts (as well as laymen) proceed predominantly—albeit most often implicitly—in terms of this model when attempting to explain happenings in foreign affairs. Indeed, that occurrences in foreign affairs are the *acts* of *nations* seems so fundamental to thinking about such problems that this underlying model has rarely been recognized: to explain an occurrence in foreign policy simply means to show how the government could have rationally chosen that action.[24] These brief examples illustrate five uses of the model. To prove that most analysts think largely in terms of the rational policy model is not possible. In this limited space it is not even possible to illustrate the range of employment of the framework. Rather, my purpose is to convey to the reader a grasp of the model and a challenge: let the reader examine the literature with which he is most familiar and make his judgment.

The general characterization can be sharpened by articulating the rational policy model as an "analytic paradigm" in the technical sense developed by Robert K. Merton for sociological analyses.[25] Systematic statement of basic assumptions, concepts, and propositions employed by Model I analysts highlights the distinctive thrust of this style of analysis. To articulate a largely implicit framework is of necessity to caricature. But caricature can be instructive.

RATIONAL POLICY PARADIGM

I. Basic Unit of Analysis: Policy as National Choice

Happenings in foreign affairs are conceived as actions chosen by the nation or national

[22] Schelling, *op. cit.*, p. 4.

[23] See Morgenthau, *op. cit.*, p. 5; Hoffmann, *Contemporary Theory*, pp. 178–179; Hoffmann, "Roulette in the Cellar," *The State of War;* Schelling, *op. cit.*

[24] The larger study examines several exceptions to this generalization. Sidney Verba's excellent essay "Assumptions of Rationality and Non-Rationality in Models of the International System" is less an exception than it is an approach to a somewhat different problem. Verba focuses upon models of rationality and irrationality of *individual* statesmen: in Knorr and Verba, *The International System.*

[25] Robert K. Merton, *Social Theory and Social Structures* (Revised and Enlarged Edition; New York, 1957), pp. 12–16. Considerably weaker than a satisfactory theoretical model, paradigms nevertheless represent a short step in that direction from looser, implicit conceptual models. Neither the concepts nor the relations among the variables are sufficiently specified to yield propositions deductively. "Paradigmatic Analysis" nevertheless has considerable promise for clarifying and codifying styles of analysis in political science. Each of the paradigms stated here can be represented rigorously in mathematical terms. For example, Model I lends itself to mathematical formulation along the lines of Herbert Simon's "Behavioral Theory of Rationality," *Models of Man* (New York, 1957). But this does not solve the most difficult problem of "measurement and estimation."

government.[26] Governments select the action that will maximize strategic goals and objectives. These "solutions" to strategic problems are the fundamental categories in terms of which the analyst perceives what is to be explained.

II. Organizing Concepts

A. *National Actor.* The nation or government, conceived as a rational, unitary decision-maker, is the agent. This actor has one set of specified goals (the equivalent of a consistent utility function), one set of perceived options, and a single estimate of the consequences that follow from each alternative.

B. *The Problem.* Action is chosen in response to the strategic problem which the nation faces. Threats and opportunities arising in the "international strategic market place" move the nation to act.

C. *Static Selection.* The sum of activity of representatives of the government relevant to a problem constitutes what the nation has chosen as its "solution." Thus the action is conceived as a steady-state choice among alternative outcomes (rather than, for example, a large number of partial choices in a dynamic stream).

D. *Action as Rational Choice.* The components include:

1. *Goals and Objectives.* National security and national interests are the principal categories in which strategic goals are conceived. Nations seek security and a range of further objectives. (Analysts rarely translate strategic goals and objectives into an explicit utility function; nevertheless, analysts do focus on major goals and objectives and trade off side effects in an intuitive fashion.)

2. *Options.* Various courses of action relevant to a strategic problem provide the spectrum of options.

3. *Consequences.* Enactment of each alternative course of action will produce a series of consequences. The relevant consequences constitute benefits and costs in terms of strategic goals and objectives.

4. *Choice.* Rational choice is value-maximizing. The rational agent selects the alternative whose consequences rank highest in terms of his goals and objectives.

III. Dominant Inference Pattern

This paradigm leads analysts to rely on the following pattern of inference: if a nation performed a particular action, that nation must have had ends towards which the action constituted an optimal means. The rational policy model's explanatory power stems from this inference pattern. Puzzlement is relieved by revealing the purposive pattern within which the occurrence can be located as a value-maximizing means.

IV. General Propositions

The disgrace of political science is the infrequency with which propositions of any generality are formulated and tested. "Paradigmatic analysis" argues for explicitness about the terms in which analysis proceeds, and seriousness about the logic of explanation. Simply to illustrate the kind of propositions on which analysts who employ this model rely, the formulation includes several.

The basic assumption of value-maximizing behavior produces propositions central to most explanations. The general principle can be formulated as follows: the likelihood of any particular action results from a combination of the nation's (1) relevant values and objectives, (2) perceived alternative courses of action, (3) estimates of various sets of consequences (which will follow from each alternative), and (4) net valuation of each set of consequences. This yields two propositions.

A. An increase in the cost of an alternative, i.e., a reduction in the value of the set of consequences which will follow from that action, or a reduction in the probability of attaining fixed consequences, reduces the likelihood of that alternative being chosen.

B. A decrease in the costs of an alternative, i.e., an increase in the value of the set of consequences which will follow from that alternative, or an increase in the probability of attaining fixed consequences, increases the likelihood of that action being chosen.[27]

[26] Though a variant of this model could easily be stochastic, this paradigm is stated in non-probabilistic terms. In contemporary strategy, a stochastic version of this model is sometimes used for predictions; but it is almost impossible to find an explanation of an occurrence in foreign affairs that is consistently probabilistic.

Analogies between Model I and the concept of explanation developed by R. G. Collingwood, William Dray, and other "revisionists" among philosophers concerned with the critical philosophy of history are not accidental. For a summary of the "revisionist position" see Maurice Mandelbaum, "Historical Explanation: The Problem of Covering Laws," *History and Theory* (1960).

[27] This model is an analogue of the theory of the rational entrepreneur which has been developed extensively in economic theories of the firm and the consumer. These two propositions specify the "substitution effect." Refinement of this model and

V. Specific Propositions

A. *Deterrence.* The likelihood of any particular attack results from the factors specified in the general proposition. Combined with factual assertions, this general proposition yields the propositions of the sub-theory of deterrence.

(1) A stable nuclear balance reduces the likelihood of nuclear attack. This proposition is derived from the general proposition plus the asserted fact that a second-strike capability affects the potential attacker's calculations by increasing the likelihood and the costs of one particular set of consequences which might follow from attack—namely, retaliation.

(2) A stable nuclear balance increases the probability of limited war. This proposition is derived from the general proposition plus the asserted fact that though increasing the costs of a nuclear exchange, a stable nuclear balance nevertheless produces a more significant reduction in the probability that such consequences would be chosen in response to a limited war. Thus this set of consequences weighs less heavily in the calculus.

B. *Soviet Force Posture.* The Soviet Union chooses its force posture (i.e., its weapons and their deployment) as a value-maximizing means of implementing Soviet strategic objectives and military doctrine. A proposition of this sort underlies Secretary of Defense Laird's inference from the fact of 200 SS-9s (large intercontinental missiles) to the assertion that, "the Soviets are going for a first-strike capability, and there's no question about it."[28]

VARIANTS OF THE RATIONAL POLICY MODEL

This paradigm exhibits the characteristics of the most refined version of the rational model. The modern literature of strategy employs a model of this sort. Problems and pressures in the "international strategic marketplace" yield probabilities of occurrence. The international actor, which could be any national actor, is simply a value-maximizing mechanism for getting from the strategic problem to the logical solution. But the explanations and predictions produced by most analysts of foreign affairs depend primarily on variants of this "pure" model. The point of each is the same: to place the action within a value-maximizing framework, given certain constraints. Nevertheless, it may be helpful to identify several variants, each of which might be exhibited similarly as a paradigm. The first focuses upon the national actor and his choice in a particular situation, leading analysts to further constrain the goals, alternatives, and consequences considered. Thus, (1) national propensities or personality traits reflected in an "operational code," (2) concern with certain objectives, or (3) special principles of action, narrow the "goals" or "alternatives" or "consequences" of the paradigm. For example, the Soviet deployment of ABMs is sometimes explained by reference to the Soviet's "defense-mindedness." Or a particular Soviet action is explained as an instance of a special rule of action in the Bolshevik operational code.[29] A second, related, cluster of variants focuses on the individual leader or leadership group as the actor whose preference function is maximized and whose personal (or group) characteristics are allowed to modify the alternatives, consequences, and rules of choice. Explanations of the U.S. involvement in Vietnam as a natural consequence of the Kennedy-Johnson Administration's axioms of foreign policy rely on this variant. A third, more complex variant of the basic model recognizes the existence of several actors within a government, for example, Hawks and Doves or military and civilians, but attempts to explain (or predict) an occurrence by reference to the objectives of the victorious actor. Thus, for example, some revisionist histories of the Cold War recognize the forces of light and the forces of darkness within the U.S. government, but explain American actions as a result of goals and perceptions of the victorious forces of darkness.

Each of these forms of the basic paradigm constitutes a formalization of what analysts typically rely upon implicitly. In the transition from implicit conceptual model to explicit paradigm much of the richness of the best employments of this model has been lost. But the purpose in raising loose, implicit conceptual models to an explicit level is to reveal the basic logic of analysts' activity. Perhaps some of the remaining artificiality that surrounds the statement of the paradigm can be erased by noting a number of the standard additions and modifications employed by analysts who proceed *predominantly* within the rational policy model. First, in the course of a document, analysts shift from one variant of the basic model to another, occasionally appropriating in an *ad hoc* fashion aspects of a situation which are logically incompatible with the basic model. Second, in the course of explaining a number of occurrences, analysts sometimes pause over a particular event about which they have a great deal of information and unfold it in such detail that an impression of

specification of additional general propositions by translating from the economic theory is straightforward.

[28] *New York Times,* March 22, 1969.

[29] See Nathan Leites, *A Study of Bolshevism* (Glencoe, Illinois, 1953).

randomness is created. Third, having employed other assumptions and categories in deriving an explanation or prediction, analysts will present their product in a neat, convincing rational policy model package. (This accommodation is a favorite of members of the intelligence community whose association with the details of a process is considerable, but who feel that by putting an occurrence in a larger rational framework, it will be more comprehensible to their audience.) Fourth, in attempting to offer an explanation—particularly in cases where a prediction derived from the basic model has failed—the notion of a "mistake" is invoked. Thus, the failure in the prediction of a "missile gap" is written off as a Soviet mistake in not taking advantage of their opportunity. Both these and other modifications permit Model I analysts considerably more variety than the paradigm might suggest. But such accommodations are essentially appendages to the basic logic of these analyses.

THE U.S. BLOCKADE OF CUBA: A FIRST CUT[30]

The U.S. response to the Soviet Union's emplacement of missiles in Cuba must be understood in strategic terms as simple value-maximizing escalation. American nuclear superiority could be counted on to paralyze Soviet nuclear power; Soviet transgression of the nuclear threshold in response to an American use of lower levels of violence would be wildly irrational since it would mean virtual destruction of the Soviet Communist system and Russian nation. American local superiority was overwhelming: it could be initiated at a low level while threatening with high credibility an ascending sequence of steps short of the nuclear threshold. All that was required was for the United States to bring to bear its strategic and local superiority in such a way that American determination to see the missiles removed would be demonstrated, while at the same time allowing Moscow time and room to retreat without humiliation. The naval blackade—euphemistically named a "quarantine" in order to circumvent the niceties of international law—did just that.

The U.S. government's selection of the blockade followed this logic. Apprised of the presence of Soviet missiles in Cuba, the President assembled an Executive Committee (ExCom) of the National Security Council and directed them to "set aside all other tasks to make a prompt and intense survey of the dangers and all possible courses of action."[31] This group functioned as "fifteen individuals on our own, representing the President and not different departments."[32] As one of the participants recalls, "The remarkable aspect of those meetings was a sense of complete equality."[33] Most of the time during the week that followed was spent canvassing all the possible tracks and weighing the arguments for and against each. Six major categories of action were considered.

1. Do nothing. U.S. vulnerability to Soviet missiles was no new thing. Since the U.S. already lived under the gun of missiles based in Russia, a Soviet capability to strike from Cuba too made little real difference. The real danger stemmed from the possibility of U.S. over-reaction. The U.S. should announce the Soviet action in a calm, casual manner thereby deflating whatever political capital Khrushchev hoped to make of the missiles.

This argument fails on two counts. First, it grossly underestimates the military importance of the Soviet move. Not only would the Soviet Union's missile capability be doubled and the U.S. early warning system outflanked. The Soviet Union would have an opportunity to reverse the strategic balance by further installations, and indeed, in the longer run, to invest in cheaper, shorter-range rather than more expensive longer-range missiles. Second, the political importance of this move was undeniable. The Soviet Union's act challenged the American President's most solemn warning. If the U.S. failed to respond, no American commitment would be credible.

2. Diplomatic pressures. Several forms were considered: an appeal to the U.N. or O.A.S. for an inspection team, a secret approach to Khrushchev, and a direct approach to Khrushchev, perhaps at a summit meeting. The United States would demand that the missiles be removed, but the final settlement might include neutralization of Cuba, U.S. withdrawal from the Guantanamo base, and withdrawal of U.S. Jupiter missiles from Turkey or Italy.

Each form of the diplomatic approach had its own drawbacks. To arraign the Soviet Union before the U.N. Security Council held little promise since the Russians could veto any proposed action. While the diplomats argued, the missiles would become operational. To send a secret emissary to Khrushchev demanding that

[30] As stated in the introduction, this "case snapshot" presents, without editorial commentary, a Model I analyst's explanation of the U.S. blockade. The purpose is to illustrate a strong, characteristic rational policy model account. This account is (roughly) consistent with prevailing explanations of these events.

[31] Theodore Sorensen, *op. cit.*, p. 675.

[32] *Ibid.*, p. 679.

[33] *Ibid.*, p. 679.

the missiles be withdrawn would be to pose untenable alternatives. On the one hand, this would invite Khrushchev to seize the diplomatic initiative, perhaps committing himself to strategic retaliation in response to an attack on Cuba. On the other hand, this would tender an ultimatum that no great power could accept. To confront Khrushchev at a summit would guarantee demands for U.S. concessions, and the analogy between U.S. missiles in Turkey and Russian missiles in Cuba could not be erased.

But why not trade U.S. Jupiters in Turkey and Italy, which the President had previously ordered withdrawn, for the missiles in Cuba? The U.S. had chosen to withdraw these missiles in order to replace them with superior, less vulnerable Mediterranean Polaris submarines. But the middle of the crisis was no time for concessions. The offer of such a deal might suggest to the Soviets that the West would yield and thus tempt them to demand more. It would certainly confirm European suspicions about American willingness to sacrifice European interests when the chips were down. Finally, the basic issue should be kept clear. As the President stated in reply to Bertrand Russell, "I think your attention might well be directed to the burglars rather than to those who have caught the burglars."[34]

3. A secret approach to Castro. The crisis provided an opportunity to separate Cuba and Soviet Communism by offering Castro the alternatives, "split or fall." But Soviet troops transported, constructed, guarded, and controlled the missiles. Their removal would thus depend on a Soviet decision.

4. Invasion. The United States could take this occasion not only to remove the missiles but also to rid itself of Castro. A Navy exercise had long been scheduled in which Marines, ferried from Florida in naval vessels, would liberate the imaginary island of Vieques.[35] Why not simply shift the point of disembarkment? (The Pentagon's foresight in planning this operation would be an appropriate antidote to the CIA's Bay of Pigs!)

Preparations were made for an invasion, but as a last resort. American troops would be forced to confront 20,000 Soviets in the first Cold War case of direct contact between the troops of the super powers. Such brinksmanship courted nuclear disaster, practically guaranteeing an equivalent Soviet move against Berlin.

5. Surgical air strike. The missile sites should be removed by a clean, swift conventional attack. This was the effective counter-action which the attempted deception deserved. A surgical strike would remove the missiles and thus eliminate both the danger that the missiles might become operational and the fear that the Soviets would discover the American discovery and act first.

The initial attractiveness of this alternative was dulled by several difficulties. First, could the strike really be "surgical"? The Air Force could not guarantee destruction of all the missiles.[36] Some might be fired during the attack; some might not have been identified. In order to assure destruction of Soviet and Cuban means of retaliating, what was required was not a surgical but rather a massive attack—of at least 500 sorties. Second, a surprise air attack would of course kill Russians at the missile sites. Pressures on the Soviet Union to retaliate would be so strong that an attack on Berlin or Turkey was highly probable. Third, the key problem with this program was that of advance warning. Could the President of the United States, with his memory of Pearl Harbor and his vision of future U.S. responsibility, order a "Pearl Harbor in reverse"? For 175 years, unannounced Sunday morning attacks had been an anathema to our tradition.[37]

6. Blockade. Indirect military action in the form of a blockade became more attractive as the ExCom dissected the other alternatives. An embargo on military shipments to Cuba enforced by a naval blockade was not without flaws, however. Could the U.S. blockade Cuba without inviting Soviet reprisal in Berlin? The likely solution to joint blockades would be the lifting of both blockades, restoring the new *status quo,* and allowing the Soviets additional time to complete the missiles. Second, the possible consequences of the blockade resembled the drawbacks which disqualified the air strike. If Soviet ships did not stop, the United States would be forced to fire the first shot, inviting retaliation. Third, a blockade would deny the traditional freedom of the seas demanded by several of our close allies and might be held illegal, in violation of the U.N. Charter and international law, unless the United States could obtain a two-thirds vote in the O.A.S. Finally, how

[34] Elie Abel, *The Missile Crisis* (New York, 1966), p. 144.

[35] *Ibid.,* p. 102.

[36] Sorensen, *op. cit.,* p. 684.

[37] *Ibid.,* p. 685. Though this was the formulation of the argument, the facts are not strictly accurate. Our tradition against surprise attack was rather younger than 175 years. For example President Theodore Roosevelt applauded Japan's attack on Russia in 1904.

could a blockade be related to the problem, namely, some 75 missiles on the island of Cuba, approaching operational readiness daily? A blockade offered the Soviets a spectrum of delaying tactics with which to buy time to complete the missile installations. Was a *fait accompli* not required?

In spite of these enormous difficulties the blockade had comparative advantages: (1) It was a middle course between inaction and attack, aggressive enough to communicate firmness of intention, but nevertheless not so precipitous as a strike. (2) It placed on Khrushchev the burden of choice concerning the next step. He could avoid a direct military clash by keeping his ships away. His was the last clear chance. (3) No possible military confrontation could be more acceptable to the U.S. than a naval engagement in the Caribbean. (4) This move permitted the U.S., by flexing its conventional muscle, to exploit the threat of subsequent non-nuclear steps in each of which the U.S. would have significant superiority.

Particular arguments about advantages and disadvantages were powerful. The explanation of the American choice of the blockade lies in a more general principle, however. As President Kennedy stated in drawing the moral of the crisis:

> Above all, while defending our own vital interests, nuclear powers must avert those confrontations which bring an adversary to a choice of either a humiliating retreat or a nuclear war. To adopt that kind of course in the nuclear age would be evidence only of the bankruptcy of our policy—of a collective death wish for the world.[38]

The blockade was the United States' only real option.

MODEL II: ORGANIZATIONAL PROCESS

For some purposes, governmental behavior can be usefully summarized as action chosen by a unitary, rational decisionmaker: centrally controlled, completely informed, and value maximizing. But this simplification must not be allowed to conceal the fact that a "government" consists of a conglomerate of semi-feudal, loosely allied organizations, each with a substantial life of its own. Government leaders do sit formally, and to some extent in fact, on top of this conglomerate. But governments perceive problems through organizational sensors. Governments define alternatives and estimate consequences as organizations process information. Governments act as these organizations enact routines. Government behavior can therefore be understood according to a second conceptual model, less as deliberate choices of leaders and more as *outputs* of large organizations functioning according to standard patterns of behavior.

To be responsive to a broad spectrum of problems, governments consist of large organizations among which primary responsibility for particular areas is divided. Each organization attends to a special set of problems and acts in quasi-independence on these problems. But few important problems fall exclusively within the domain of a single organization. Thus government behavior relevant to any important problem reflects the independent output of several organizations, partially coordinated by government leaders. Government leaders can substantially disturb, but not substantially control, the behavior of these organizations.

To perform complex routines, the behavior of large numbers of individuals must be coordinated. Coordination requires standard operating procedures: rules according to which things are done. Assured capability for reliable performance of action that depends upon the behavior of hundreds of persons requires established "programs." Indeed, if the eleven members of a football team are to perform adequately on any particular down, each player must not "do what he thinks needs to be done" or "do what the quarterback tells him to do." Rather, each player must perform the maneuvers specified by a previously established play which the quarterback has simply called in this situation.

At any given time, a government consists of *existing* organizations, each with a *fixed* set of standard operating procedures and programs. The behavior of these organizations—and consequently of the government—relevant to an issue in any particular instance is, therefore, determined primarily by routines established in these organizations prior to that instance. But organizations do change. Learning occurs gradually, over time. Dramatic organizational change occurs in response to major crises. Both learning and change are influenced by existing organizational capabilities.

Borrowed from studies of organizations, these loosely formulated propositions amount simply to *tendencies*. Each must be hedged by modifiers like "other things being equal" and "under certain conditions." In particular instances, tendencies hold—more or less. In specific situations, the relevant question is: more or less? But this is as it should be. For, on the one hand, "organizations" are no more homogeneous a class than "solids." When scientists tried to generalize about "solids," they achieved similar results. Solids tend to expand when heated, but some do and some don't. More adequate categorization

[38] *New York Times*, June, 1963.

of the various elements now lumped under the rubric "organizations" is thus required. On the other hand, the behavior of particular organizations seems considerably more complex than the behavior of solids. Additional information about a particular organization is required for further specification of the tendency statements. In spite of these two caveats, the characterization of government action as organizational output differs distinctly from Model I. Attempts to understand problems of foreign affairs in terms of this frame of reference should produce quite different explanations.[39]

ORGANIZATIONAL PROCESS PARADIGM[40]

I. Basic Unit of Analysis: Policy as Organizational Output

The happenings of international politics are, in three critical senses, outputs of organizational processes. First, the actual occurrences are organizational outputs. For example, Chinese entry into the Korean War—that is, the fact that Chinese soldiers were firing at U.N. soldiers south of the Yalu in 1950—is an organizational action: the action of men who are soldiers in platoons which are in companies, which in turn are in armies, responding as privates to lieutenants who are responsible to captains and so on to the commander, moving into Korea, advancing against enemy troops, and firing according to fixed routines of the Chinese Army. Government leaders' decisions trigger organizational routines. Government leaders can trim the edges of this output and exercise some choice in combining outputs. But the mass of behavior is determined by previously established procedures. Second, existing organizational routines for employing present physical capabilities constitute the effective options open to government leaders confronted with any problem. Only the existence of men, equipped and trained as armies and capable of being transported to North Korea, made entry into the Korean War a live option for the Chinese leaders. The fact that fixed programs (equipment, men, and routines which exist at the particular time) exhaust the range of buttons that leaders can push is not always perceived by these leaders. But in every case it is critical for an understanding of what is actually done. Third, organizational outputs structure the situation within the narrow constraints of which leaders must contribute their "decision" concerning an issue. Outputs raise the problem, provide the information, and make the initial moves that color the face of the issue that is turned to the leaders. As Theodore Sorensen has observed: "Presidents rarely, if ever, make decisions—particularly in foreign affairs—in the sense of writing their conclusions on a clean slate . . . The basic decisions, which confine their choices, have all too often been previously made."[41] If one understands the structure of the situation and the face of the issue—which are determined by the organizational outputs—the formal choice of the leaders is frequently anti-climactic.

II. Organizing Concepts

A. *Organizational Actors.* The actor is not a monolithic "nation" or "government" but rather a constellation of loosely allied organizations on top of which government leaders sit. This constellation acts only as component organizations perform routines.[42]

B. *Factored Problems and Fractionated Power.* Surveillance of the multiple facets of for-

[39] The influence of organizational studies upon the present literature of foreign affairs is minimal. Specialists in international politics are not students of organization theory. Organization theory has only recently begun to study organizations as decisionmakers and has not yet produced behavioral studies of national security organizations from a decision-making perspective. It seems unlikely, however, that these gaps will remain unfilled much longer. Considerable progress has been made in the study of the business firm as an organization. Scholars have begun applying these insights to government organizations, and interest in an organizational perspective is spreading among institutions and individuals concerned with actual government operations. The "decisionmaking" approach represented by Richard Snyder, R. Bruck, and B. Sapin, *Foreign Policy Decision-Making* (Glencoe, Illinois, 1962), incorporates a number of insights from organization theory.

[40] The formulation of this paradigm is indebted both to the orientation and insights of Herbert Simon and to the behavioral model of the firm stated by Richard Cyert and James March, *A Behavioral Theory of the Firm* (Englewood Cliffs, 1963). Here, however, one is forced to grapple with the less routine, less quantified functions of the less differentiated elements in government organizations.

[41] Theodore Sorensen, "You Get to Walk to Work," *New York Times Magazine,* March 19, 1967.

[42] Organizations are not monolithic. The proper level of disaggregation depends upon the objectives of a piece of analysis. This paradigm is formulated with reference to the major organizations that constitute the U.S. government. Generalization to the major components of each department and agency should be relatively straightforward.

eign affairs requires that problems be cut up and parcelled out to various organizations. To avoid paralysis, primary power must accompany primary responsibility. But if organizations are permitted to do anything, a large part of what they do will be determined within the organization. Thus each organization perceives problems, processes information, and performs a range of actions in quasi-independence (within broad guidelines of national policy). Factored problems and fractionated power are two edges of the same sword. Factoring permits more specialized attention to particular facets of problems than would be possible if government leaders tried to cope with these problems by themselves. But this additional attention must be paid for in the coin of discretion for *what* an organization attends to, and *how* organizational responses are programmed.

C. *Parochial Priorities, Perceptions, and Issues.* Primary responsibility for a narrow set of problems encourages organizational parochialism. These tendencies are enhanced by a number of additional factors: (1) selective information available to the organization, (2) recruitment of personnel into the organization, (3) tenure of individuals in the organization, (4) small group pressures within the organization, and (5) distribution of rewards by the organization. Clients (e.g., interest groups), government allies (e.g., Congressional committees), and extra-national counterparts (e.g., the British Ministry of Defense for the Department of Defense, ISA, or the British Foreign Office for the Department of State, EUR) galvanize this parochialism. Thus organizations develop relatively stable propensities concerning operational priorities, perceptions, and issues.

D. *Action as Organizational Output.* The preeminent feature of organizational activity is its programmed character: the extent to which behavior in any particular case is an enactment of preestablished routines. In producing outputs, the activity of each organization is characterized by:

1. *Goals: Constraints Defining Acceptable Performance.* The operational goals of an organization are seldom revealed by formal mandates. Rather, each organization's operational goals emerge as a set of constraints defining acceptable performance. Central among these constraints is organizational health, defined usually in terms of bodies assigned and dollars appropriated. The set of constraints emerges from a mix of expectations and demands of other organizations in the government, statutory authority, demands from citizens and special interest groups, and bargaining within the organization. These constraints represent a quasi-resolution of conflict—the constraints are relatively stable, so there is some resolution. But conflict among alternative goals is always latent; hence, it is a quasi-resolution. Typically, the constraints are formulated as imperatives to avoid roughly specified discomforts and disasters.[43]

2. *Sequential Attention to Goals.* The existence of conflict among operational constraints is resolved by the device of sequential attention. As a problem arises, the subunits of the organization most concerned with that problem deal with it in terms of the constraints they take to be most important. When the next problem arises, another cluster of subunits deals with it, focusing on a different set of constraints.

3. *Standard Operating Procedures.* Organizations perform their "higher" functions, such as attending to problem areas, monitoring information, and preparing relevant responses for likely contingencies, by doing "lower" tasks, for example, preparing budgets, producing reports, and developing hardware. Reliable performance of these tasks requires standard operating procedures (hereafter SOPs). Since procedures are "standard" they do not change quickly or easily. Without these standard procedures, it would not be possible to perform certain concerted tasks. But because of standard procedures, organizational behavior in particular instances often appears unduly formalized, sluggish, or inappropriate.

4. *Programs and Repertoires.* Organizations must be capable of performing actions in which the behavior of large numbers of individuals is carefully coordinated. Assured performance requires clusters of rehearsed SOPs for producing specific actions, e.g., fighting enemy units or answering an embassy's cable. Each cluster comprises a "program" (in the terms both of drama and computers) which the organization has available for dealing with a situation. The list of programs relevant to a type of activity, e.g., fighting, constitutes an organizational repertoire. The number of programs in a repertoire is always quite limited. When properly triggered, organizations execute programs; programs cannot be substantially changed in a particular situation. The more complex the action and the greater the number of individuals involved, the more important are programs and repertoires as determinants of organizational behavior.

5. *Uncertainty Avoidance.* Organizations do not attempt to estimate the probability distribution of future occurrences. Rather, organizations

[43] The stability of these constraints is dependent on such factors as rules for promotion and reward, budgeting and accounting procedures, and mundane operating procedures.

avoid uncertainty. By arranging a *negotiated environment,* organizations regularize the reactions of other actors with whom they have to deal. The primary environment, relations with other organizations that comprise the government, is stabilized by such arrangements as agreed budgetary splits, accepted areas of responsibility, and established conventional practices. The secondary environment, relations with the international world, is stabilized between allies by the establishment of contracts (alliances) and "club relations" (U.S. State and U.K. Foreign Office or U.S. Treasury and U.K. Treasury). Between enemies, contracts and accepted conventional practices perform a similar function, for example, the rules of the "precarious status quo" which President Kennedy referred to in the missile crisis. Where the international environment cannot be negotiated, organizations deal with remaining uncertainties by establishing a set of *standard scenarios* that constitute the contingencies for which they prepare. For example, the standard scenario for Tactical Air Command of the U.S. Air Force involves combat with enemy aircraft. Planes are designed and pilots trained to meet this problem. That these preparations are less relevant to more probable contingencies, e.g., provision of close-in ground support in limited wars like Vietnam, has had little impact on the scenario.

6. *Problem-directed Search.* Where situations cannot be construed as standard, organizations engage in search. The style of search and the solution are largely determined by existing routines. Organizational search for alternative courses of action is problem-oriented: it focuses on the atypical discomfort that must be avoided. It is simple-minded: the neighborhood of the symptom is searched first; then, the neighborhood of the current alternative. Patterns of search reveal biases which in turn reflect such factors as specialized training or experience and patterns of communication.

7. *Organizational Learning and Change.* The parameters of organizational behavior mostly persist. In response to non-standard problems, organizations search and routines evolve, assimilating new situations. Thus learning and change follow in large part from existing procedures. But marked changes in organizations do sometimes occur. Conditions in which dramatic changes are more likely include: (1) Periods of budgetary feast. Typically, organizations devour budgetary feasts by purchasing additional items on the existing shopping list. Nevertheles[illegible] if committed to change, leaders who contro[illegible] udget can use extra funds to effect [illegible]ng[illegible] Periods of prolonged budgetary [illegible]in[illegible] a single year's famine typically results in few changes in organizational structure but a loss of effectiveness in performing some programs, prolonged famine forces major retrenchment. (3) Dramatic performance failures. Dramatic change occurs (mostly) in response to major disasters. Confronted with an undeniable failure of procedures and repertoires, authorities outside the organization demand change, existing personnel are less resistant to change, and critical members of the organization are replaced by individuals committed to change.

E. *Central Coordination and Control.* Action requires decentralization of responsibility and power. But problems lap over the jurisdictions of several organizations. Thus the necessity for decentralization runs headlong into the requirement for coordination. (Advocates of one horn or the other of this dilemma—responsive action entails decentralized power vs. coordinated action requires central control—account for a considerable part of the persistent demand for government reorganization.) Both the necessity for coordination and the centrality of foreign policy to national welfare guarantee the involvement of government leaders in the procedures of the organizations among which problems are divided and power shared. Each organization's propensities and routines can be disturbed by government leaders' intervention. Central direction and persistent control of organizational activity, however, is not possible. The relation among organizations, and between organizations and the government leaders depends critically on a number of structural variables including: (1) the nature of the job, (2) the measures and information available to government leaders, (3) the system of rewards and punishments for organizational members, and (4) the procedures by which human and material resources get committed. For example, to the extent that rewards and punishments for the members of an organization are distributed by higher authorities, these authorities can exercise some control by specifying criteria in terms of which organizational output is to be evaluated. These criteria become constraints within which organizational activity proceeds. But constraint is a crude instrument of control.

Intervention by government leaders does sometimes change the activity of an organization in an intended direction. But instances are fewer than might be expected. As Franklin Roosevelt, the master manipulator of government organizations, remarked:

> The Treasury is so large and far-flung and ingrained in its practices that I find it is almost impossible to get the action and results I want. . . .

But the Treasury is not to be compared with the State Department. You should go through the experience of trying to get any changes in the thinking, policy, and action of the career diplomats and then you'd know what a real problem was. But the Treasury and the State Department put together are nothing compared with the Na-a-vy . . . To change anything in the Na-a-vy is like punching a feather bed. You punch it with your right and you punch it with your left until you are finally exhausted, and then you find the damn bed just as it was before you started punching.[44]

John Kennedy's experience seems to have been similar: "The State Department," he asserted, "is a bowl full of jelly."[45] And lest the McNamara revolution in the Defense Department seem too striking a counter-example, the Navy's recent rejection of McNamara's major intervention in Naval weapons procurement, the F-111B, should be studied as an antidote.

F. *Decisions of Government Leaders.* Organizational persistence does not exclude shifts in governmental behavior. For government leaders sit atop the conglomerate of organizations. Many important issues of governmental action require that these leaders decide what organizations will play out which programs where. Thus stability in the parochialisms and SOPs of individual organizations is consistent with some important shifts in the behavior of governments. The range of these shifts is defined by existing organizational programs.

III. Dominant Inference Pattern

If a nation performs an action of this type today, its organizational components must yesterday have been performing (or have had established routines for performing) an action only marginally different from this action. At any specific point in time, a government consists of an established conglomerate of organizations, each with existing goals, programs, and repertoires. The characteristics of a government's action in any instance follows from those established routines, and from the choice of government leaders —on the basis of information and estimates provided by existing routines—among existing programs. The best explanation of an organization's behavior at t is $t-1$; the prediction of $t+1$ is t. Model II's explanatory power is achieved by uncovering the organizational routines and repertoires that produced the outputs that comprise the puzzling occurrence.

[44] Marriner Eccles, *Beckoning Frontiers* (New York, 1951), p. 336.

[45] Arthur Schlesinger, *A Thousand Days* (Boston, 1965), p. 406.

IV. General Propositions

A number of general propositions have been stated above. In order to illustrate clearly the type of proposition employed by Model II analysts, this section formulates several more precisely.

A. *Organizational Action.* Activity according to SOPs and programs does not constitute far-sighted, flexible adaptation to "the issue" (as it is conceived by the analyst). Detail and nuance of actions by organizations are determined predominantly by organizational routines, not government leaders' directions.

1. SOPs constitute routines for dealing with *standard* situations. Routines allow large numbers of ordinary individuals to deal with numerous instances, day after day, without considerable thought, by responding to basic stimuli. But this regularized capability for adequate performance is purchased at the price of standardization. If the SOPs are appropriate, average performance, i.e., performance averaged over the range of cases, is better than it would be if each instance were approached individually (given fixed talent, timing, and resource constraints). But specific instances, particularly critical instances that typically do not have "standard" characteristics, are often handled sluggishly or inappropriately.

2. A program, i.e., a complex action chosen from a short list of programs in a repertoire, is rarely tailored to the specific situation in which it is executed. Rather, the program is (at best) the most appropriate of the programs in a previously developed repertoire.

3. Since repertoires are developed by parochial organizations for standard scenarios defined by that organization, programs available for dealing with a particular situation are often ill-suited.

B. *Limited Flexibility and Incremental Change.* Major lines of organizational action are straight, i.e., behavior at one time is marginally different from that behavior at $t-1$. Simple-minded predictions work best: Behavior at $t+1$ will be marginally different from behavior at the present time.

1. Organizational budgets change incrementally—both with respect to totals and with respect to intra-organizational splits. Though organizations could divide the money available each year by carving up the pie anew (in the light of changes in objectives or environment), in practice, organizations take last year's budget as a base and adjust incrementally. Predictions that require large budgetary shifts in a single year between organizations or between units within an organization should be hedged.

2. Once undertaken, an organizational in-

vestment is not dropped at the point where "objective" costs outweigh benefits. Organizational stakes in adopted projects carry them quite beyond the loss point.

C. *Administrative Feasibility.* Adequate explanation, analysis, and prediction must include administrative feasibility as a major dimension. A considerable gap separates what leaders choose (or might rationally have chosen) and what organizations implement.

1. Organizations are blunt instruments. Projects that require several organizations to act with high degrees of precision and coordination are not likely to succeed.

2. Projects that demand that existing organizational units depart from their accustomed functions and perform previously unprogrammed tasks are rarely accomplished in their designed form.

3. Government leaders can expect that each organization will do its "part" in terms of what the organization knows how to do.

4. Government leaders can expect incomplete and distorted information from each organization concerning its part of the problem.

5. Where an assigned piece of a problem is contrary to the existing goals of an organization, resistance to implementation of that piece will be encountered.

V. *Specific Propositions.*

1. *Deterrence.* The probability of nuclear attack is less sensitive to balance and imbalance, or stability and instability (as these concepts are employed by Model I strategists) than it is to a number of organizational factors. Except for the special case in which the Soviet Union acquires a credible capability to destroy the U.S. with a disarming blow, U.S. superiority or inferiority affects the probability of a nuclear attack less than do a number of organizational factors.

First, if a nuclear attack occurs, it will result from organizational activity: the firing of rockets by members of a missile group. The enemy's *control system,* i.e., physical mechanisms and standard procedures which determine who can launch rockets when, is critical. Second, the enemy's programs for bringing his strategic forces to *alert status* determine probabilities of accidental firing and momentum. At the outbreak of World War I, if the Russian Tsar had understood the organizational processes which his order of full mobilization triggered, he would have realized that he had chosen war. Third, organizational repertoires fix the range of effective choice open to enemy leaders. The menu available to Tsar Nicholas in 1914 has two entrees: full mobilization and no mobilization. Partial mobilization was not an organizational option. Fourth, since organizational routines set the chessboard, the training and deployment of troops and nuclear weapons is crucial. Given that the outbreak of hostilities in Berlin is more probable than most scenarios for nuclear war, facts about deployment, training, and tactical nuclear equipment of Soviet troops stationed in East Germany—which will influence the face of the issue seen by Soviet leaders at the outbreak of hostilities and the manner in which choice is implemented—are as critical as the question of "balance."

2. *Soviet Force Posture.* Soviet force posture, i.e., the fact that certain weapons rather than others are procured and deployed, is determined by organizational factors such as the goals and procedures of existing military services and the goals and processes of research and design labs, within budgetary constraints that emerge from the government leader's choices. The frailty of the Soviet Air Force within the Soviet military establishment seems to have been a crucial element in the Soviet failure to acquire a large bomber force in the 1950s (thereby faulting American intelligence predictions of a "bomber gap"). The fact that missiles were controlled until 1960 in the Soviet Union by the Soviet Ground Forces, whose goals and procedures reflected no interest in an intercontinental mission, was not irrelevant to the slow Soviet buildup of ICBMs (thereby faulting U.S. intelligence predictions of a "missile gap"). These organizational factors (Soviet Ground Forces' control of missiles and that service's fixation with European scenarios) make the Soviet deployment of so many MRBMs that European targets could be destroyed three times over, more understandable. Recent weapon developments, e.g., the testing of a Fractional Orbital Bombardment System (FOBS) and multiple warheads for the SS-9, very likely reflect the activity and interests of a cluster of Soviet research and development organizations, rather than a decision by Soviet leaders to acquire a first strike weapon system. Careful attention to the organizational components of the Soviet military establishment (Strategic Rocket Forces, Navy, Air Force, Ground Forces, and National Air Defense), the missions and weapons systems to which each component is wedded (an independent weapon system assists survival as an independent service), and existing budgetary splits (which probably are relatively stable in the Soviet Union as they tend to be everywhere) offer potential improvements in medium and longer term predictions.

THE U.S. BLOCKADE OF CUBA: A SECOND CUT

Organizational Intelligence. At 7:00 P.M. on

October 22, 1962, President Kennedy disclosed the American discovery of the presence of Soviet strategic missiles in Cuba, declared a "strict quarantine on all offensive military equipment under shipment to Cuba," and demanded that "Chairman Khrushchev halt and eliminate this clandestine, reckless, and provocative threat to world peace."[46] This decision was reached at the pinnacle of the U.S. Government after a critical week of deliberation. What initiated that precious week were photographs of Soviet missile sites in Cuba taken on October 14. These pictures might not have been taken until a week later. In that case, the President speculated, "I don't think probably we would have chosen as prudently as we finally did."[47] U.S. leaders might have received this information three weeks earlier—if a U-2 had flown over San Cristobal in the last week of September.[48] What determined the context in which American leaders came to choose the blockade was the discovery of missiles on October 14.

There has been considerable debate over alleged American "intelligence failures" in the Cuban missile crisis.[49] But what both critics and defenders have neglected is the fact that the discovery took place on October 14, rather than three weeks earlier or a week later, as a consequence of the established routines and procedures of the organizations which constitute the U.S. intelligence community. These organizations were neither more nor less successful than they had been the previous month or were to be in the months to follow.[50]

The notorious "September estimate," approved by the United States Intelligence Board (USIB) on September 19, concluded that the Soviet Union would not introduce offensive missiles into Cuba.[51] No U-2 flight was directed over the western end of Cuba (after September 5) before October 4.[52] No U-2 flew over the western end of Cuba until the flight that discovered the Soviet missiles on October 14.[53] Can these "failures" be accounted for in organizational terms?

On September 19 when USIB met to consider the question of Cuba, the "system" contained the following information: (1) shipping intelligence had noted the arrival in Cuba of two large-hatch Soviet lumber ships, which were riding high in the water; (2) refugee reports of countless sightings of missiles, but also a report that Castro's private pilot, after a night of drinking in Havana, had boasted: "We will fight to the death and perhaps we can win because we have everything, including atomic weapons"; (3) a sighting by a CIA agent of the rear profile of a strategic missile; (4) U-2 photos produced by flights of August 29, September 5 and 17 showing the construction of a number of SAM sites and other defensive missiles.[54] Not all of this information was on the desk of the estimators, however. Shipping intelligence experts noted the fact that large-hatch ships were riding high in the water and spelled out the inference: the ships must be carrying "space consuming" cargo.[55] These facts were carefully included in the catalogue of intelligence concerning shipping. For experts sensitive to the Soviets' shortage of ships, however, these facts carried no special signal. The refugee report of Castro's private pilot's remark had been received at Opa Locka, Florida, along with vast reams of inaccurate reports generated by the refugee community. This report and a thousand others had to be checked and compared before being sent to Washington. The two weeks required for initial processing could have been shortened by a large increase in resources, but the yield of this source was already quite marginal. The CIA agent's sighting of the rear profile of a strategic missile had oc-

[46] U.S. Department of State, *Bulletin*, XLVII, pp. 715–720.

[47] Schlesinger, *op. cit.*, p. 803.

[48] Theodore Sorensen, *Kennedy*, p. 675.

[49] See U.S. Congress, Senate, Committee on Armed Services, Preparedness Investigation Subcommittee, *Interim Report on Cuban Military Build-up*, 88th Congress, 1st Session, 1963, p. 2; Hanson Baldwin, "Growing Risks of Bureaucratic Intelligence," *The Reporter* (August 15, 1963), 48–50; Roberta Wohlstetter, "Cuba and Pearl Harbor," *Foreign Affairs* (July, 1965), 706.

[50] U.S. Congress, House of Representatives, Committee on Appropriations, Subcommittee on Department of Defense Appropriations, *Hearings*, 88th Congress, 1st Session, 1963, 25 ff.

[51] R. Hilsman, *To Move a Nation* (New York, 1967), pp. 172–173.

[52] Department of Defense Appropriations, *Hearings*, p. 67.

[53] *Ibid.*, pp. 66–67.

[54] For (1) Hilsman, *op. cit.*, p. 186; (2) Abel, *op. cit.*, p. 24; (3) Department of Defense Appropriations, *Hearings*, p. 64; Abel, *op. cit.*, p. 24; (4) Department of Defense Appropriations, *Hearings*, pp. 1–30.

[55] The facts here are not entirely clear. This assertion is based on information from (1) "Department of Defense Briefing by the Honorable R. S. McNamara, Secretary of Defense, State Department Auditorium, 5:00 p.m., February 6, 1963." A verbatim transcript of a presentation actually made by General Carroll's assistant, John Hughes; and (2) Hilsman's statement, *op. cit.*, p. 186. But see R. Wohlstetter's interpretation, "Cuba and Pearl Harbor," 700.

curred on September 12; transmission time from agent sighting to arrival in Washington typically took 9 to 12 days. Shortening this transmission time would impose severe cost in terms of danger to sub-agents, agents, and communication networks.

On the information available, the intelligence chiefs who predicted that the Soviet Union would not introduce offensive missiles into Cuba made a reasonable and defensible judgment.[56] Moreover, in the light of the fact that these organizations were gathering intelligence not only about Cuba but about potential occurrences in all parts of the world, the informational base available to the estimators involved nothing out of the ordinary. Nor, from an organizational perspective, is there anything startling about the gradual accumulation of evidence that led to the formulation of the hypothesis that the Soviets were installing missiles in Cuba and the decision on October 4 to direct a special flight over western Cuba.

The ten-day delay between that decision and the flight is another organizational story.[57] At the October 4 meeting, the Defense Department took the opportunity to raise an issue important to its concerns. Given the increased danger that a U-2 would be downed, it would be better if the pilot were an officer in uniform rather than a CIA agent. Thus the Air Force should assume responsibility for U-2 flights over Cuba. To the contrary, the CIA argued that this was an intelligence operation and thus within the CIA's jurisdiction. Moreover, CIA U-2's had been modified in certain ways which gave them advantages over Air Force U-2's in averting Soviet SAM's. Five days passed while the State Department pressed for less risky alternatives such as drones and the Air Force (in Department of Defense guise) and CIA engaged in territorial disputes. On October 9 a flight plan over San Cristobal was approved by COMOR, but to the CIA's dismay, Air Force pilots rather than CIA agents would take charge of the mission. At this point details become sketchy, but several members of the intelligence community have speculated that an Air Force pilot in an Air Force U-2 attempted a high altitude overflight on October 9 that "flamed out", i.e., lost power, and thus had to descend in order to restart its engine. A second round between Air Force and CIA followed, as a result of which Air Force pilots were trained to fly CIA U-2's. A successful overflight took place on October 14.

This ten-day delay constitutes some form of "failure." In the face of well-founded suspicions concerning offensive Soviet missiles in Cuba that posed a critical threat to the United States' most vital interest, squabbling between organizations whose job it is to produce this information seems entirely inappropriate. But for each of these organizations, the question involved the issue: "*Whose* job was it to be?" Moreover, the issue was not simply, which organization would control U-2 flights over Cuba, but rather the broader issue of ownership of U-2 intelligence activities—a very long standing territorial dispute. Thus though this delay was in one sense a "failure," it was also a nearly inevitable consequence of two facts: many jobs do not fall neatly into precisely defined organizational jurisdictions; and vigorous organizations are imperialistic.

Organizational Options. Deliberations of leaders in ExCom meetings produced broad outlines of alternatives. Details of these alternatives and blueprints for their implementation had to be specified by the organizations that would perform these tasks. These organizational outputs answered the question: What, specifically, *could* be done?

Discussion in the ExCom quickly narrowed the live options to two: an air strike and a blockade. The choice of the blockade instead of the air strike turned on two points: (1) the argument from morality and tradition that the United States could not perpetrate a "Pearl Harbor in reverse"; (2) the belief that a "surgical" air strike was impossible.[58] Whether the United States *might* strike first was a question not of capability but of morality. Whether the United States *could* perform the surgical strike was a factual question concerning capabilities. The majority of the members of the ExCom, including the President, initially preferred the air strike.[59] What effectively foreclosed this option, however, was the fact that the air strike they wanted could not be chosen with high confidence of success.[60] After having tentatively chosen the course of prudence—given that the surgical air strike was not an option—Kennedy reconsidered. On Sunday morning, October 21, he called the Air Force experts to a special meeting in his living quarters where he probed once more for the option of a "*surgical*" air strike.[61] General

[56] See Hilsman, *op. cit.*, pp. 172–174.

[57] Abel, *op. cit.*, pp. 26 ff; Weintal and Bartlett, *Facing the Brink* (New York, 1967), pp. 62 ff; *Cuban Military Build-up;* J. Daniel and J. Hubbell, *Strike in the West* (New York, 1963), pp. 15 ff.

[58] Schlesinger, *op. cit.*, p. 804.

[59] Sorensen, *Kennedy*, p. 684.

[60] *Ibid.*, pp. 684 ff.

[61] *Ibid.*, pp. 694–697.

Walter C. Sweeny, Commander of Tactical Air Forces, asserted again that the Air Force could guarantee no higher than ninety percent effectiveness in a surgical air strike.[62] That "fact" was false.

The air strike alternative provides a classic case of military estimates. One of the alternatives outlined by the ExCom was named "air strike." Specification of the details of this alternative was delegated to the Air Force. Starting from an existing plan for massive U.S. military action against Cuba (prepared for contingencies like a response to a Soviet Berlin grab), Air Force estimators produced an attack to guarantee success.[63] This plan called for extensive bombardment of all missile sites, storage depots, airports, and, in deference to the Navy, the artillery batteries opposite the naval base at Guantanamo.[64] Members of the ExCom repeatedly expressed bewilderment at military estimates of the number of sorties required, likely casualties, and collateral damage. But the "surgical" air strike that the political leaders had in mind was never carefully examined during the first week of the crisis. Rather, this option was simply excluded on the grounds that since the Soviet MRBM's in Cuba were classified "mobile" in U.S. manuals, extensive bombing was required. During the second week of the crisis, careful examination revealed that the missiles were mobile, in the sense that small houses are mobile: that is, they could be moved and reassembled in 6 days. After the missiles were reclassified "movable" and detailed plans for surgical air strikes specified, this action was added to the list of live options for the end of the second week.

Organizational Implementation. Ex-Com members separated several types of blockade: offensive weapons only, all armaments, and all strategic goods including POL (petroleum, oil, and lubricants). But the *"details"* of the operation were left to the Navy. Before the President announced the blockade on Monday evening, the first stage of the Navy's blueprint was in motion, and a problem loomed on the horizon.[65] The Navy had a detailed plan for the blockade. The President had several less precise but equally determined notions concerning what should be done, when, and how. For the Navy the issue was one of effective implementation of the Navy's blockade—without the meddling and interference of political leaders. For the President, the problem was to pace and manage events in such a way that the Soviet leaders would have time to see, think, and blink.

A careful reading of available sources uncovers an instructive incident. On Tuesday the British Ambassador, Ormsby-Gore, after having attended a briefing on the details of the blockade, suggested to the President that the plan for intercepting Soviet ships far out of reach of Cuban jets did not facilitate Khrushchev's hard decision.[66] Why not make the interception much closer to Cuba and thus give the Russian leader more time? According to the public account and the recollection of a number of individuals involved, Kennedy "agreed immediately, called McNamara, and over emotional Navy protest, issued the appropriate instructions."[67] As Sorensen records, "in a sharp clash with the Navy, he made certain his will prevailed."[68] The Navy's plan for the blockade was thus changed by drawing the blockade much closer to Cuba.

A serious organizational orientation makes one suspicious of this account. More careful examination of the available evidence confirms these suspicions, though alternative accounts must be somewhat speculative. According to the public chronology, a quarantine drawn close to Cuba became effective on Wednesday morning, the first Soviet ship was contacted on Thursday morning, and the first boarding of a ship occurred on Friday. According to the statement by the Department of Defense, boarding of the *Marcula* by a party from the *John R. Pierce* "took place at 7:50 A.M., E.D.T., 180 miles northeast of Nassau."[69] The *Marcula* had been trailed since about 10:30 the previous evening.[70] Simple calculations suggest that the *Pierce* must have been stationed along the Navy's original arc which extended 500 miles out to sea from Cape Magsi, Cuba's eastern most tip.[71] The blockade line was *not* moved as the President ordered, and the accounts report.

What happened is not entirely clear. One can be certain, however, that Soviet ships passed through the line along which American destroyers had posted themselves before the official "first contact" with the Soviet ship. On October 26 a Soviet tanker arrived in Havana and was honored by a dockside rally for "running the blockade." Photographs of this vessel show the name *Vinnitsa* on the side of the vessel in

[62] *Ibid.*, p. 697; Abel. *op. cit.*, pp. 100–101.
[63] Sorensen, *Kennedy*, p. 669.
[64] Hilsman, *op. cit.*, p. 204.
[65] See Abel, *op. cit.*, pp. 97 ff.
[66] Schlesinger, *op. cit.*, p. 818.
[67] *Ibid.*
[68] Sorensen, *Kennedy*, p. 710.
[69] *New York Times*, October 27, 1962.
[70] Abel, *op. cit.*, p. 171.
[71] For the location of the original arc see Abel, *op. cit.*, p. 141.

Cyrillic letters.[72] But according to the official U.S. position, the first tanker to pass through the blockade was the *Bucharest*, which was hailed by the Navy on the morning of October 25. Again simple mathematical calculation excludes the possibility that the *Bucharest* and the *Vinnitsa* were the same ship. It seems probable that the Navy's resistance to the President's order that the blockade be drawn in closer to Cuba forced him to allow one or several Soviet ships to pass through the blockade after it was officially operative.[73]

This attempt to leash the Navy's blockade had a price. On Wednesday morning, October 24, what the President had been awaiting occurred. The 18 dry cargo ships heading towards the quarantine stopped dead in the water. This was the occasion of Dean Rusk's remark, "We are eyeball to eyeball and I think the other fellow just blinked."[74] But the Navy had another interpretation. The ships had simply stopped to pick up Soviet submarine escorts. The President became quite concerned lest the Navy—already riled because of Presidential meddling in its affairs—blunder into an incident. Sensing the President's fears, McNamara became suspicious of the Navy's procedures and routines for making the first interception. Calling on the Chief of Naval Operations in the Navy's inner sanctum, the Navy Flag Plot, McNamara put his questions harshly.[75] Who would make the first interception? Were Russian-speaking officers on board? How would submarines be dealt with? At one point McNamara asked Anderson what he would do if a Soviet ship's captain refused to answer questions about his cargo. Picking up the Manual of Navy Regulations the Navy man waved it in McNamara's face and shouted, "It's all in there." To which McNamara replied, "I don't give a damn what John Paul Jones would have done; I want to know what you are going to do, now."[76] The encounter ended on Anderson's remark: "Now, Mr. Secretary, if you and your Deputy will go back to your office the Navy will run the blockade."[77]

MODEL III: BUREAUCRATIC POLITICS

The leaders who sit on top of organizations are not a monolithic group. Rather, each is, in his own right, a player in a central, competitive game. The name of the game is bureaucratic politics: bargaining along regularized channels among players positioned hierarchically within the government. Government behavior can thus be understood according to a third conceptual model not as organizational outputs, but as outcomes of bargaining games. In contrast with Model I, the bureaucratic politics model sees no unitary actor but rather many actors as players, who focus not on a single strategic issue but on many diverse intra-national problems as well, in terms of no consistent set of strategic objectives but rather according to various conceptions of national, organizational, and personal goals, making government decisions not by rational choice but by the pulling and hauling that is politics.

The apparatus of each national government constitutes a complex arena for the intra-national game. Political leaders at the top of this apparatus plus the men who occupy positions on top of the critical organizations form the circle of central players. Ascendancy to this circle assures some independent standing. The necessary decentralization of decisions required for action on the broad range of foreign policy problems guarantees that each player has considerable discretion. Thus power is shared.

The nature of problems of foreign policy permits fundamental disagreement among reasonable men concerning what ought to be done. Analyses yield conflicting recommendations. Separate responsibilities laid on the shoulders of individual personalities encourage differences in perceptions and priorities. But the issues are of first order importance. What the nation does really matters. A wrong choice could mean irreparable damage. Thus responsible men are obliged to fight for what they are convinced is right.

Men share power. Men differ concerning what must be done. The differences matter. This milieu necessitates that policy be resolved by politics. What the nation does is sometimes the result of the triumph of one group over others. More often, however, different groups pulling in different directions yield a resultant distinct from what anyone intended. What moves the chess pieces is not simply the reasons which support a course of action, nor the routines of organizations which enact an alternative, but the power and skill of proponents and opponents of the action in question.

This characterization captures the thrust of the bureaucratic politics orientation. If problems of foreign policy arose as discreet issues, and decisions were determined one game at a time, this

[72] *Facts on File*, Vol. XXII, 1962, p. 376, published by Facts on File, Inc., New York, yearly.

[73] This hypothesis would account for the mystery surrounding Kennedy's explosion at the leak of the stopping of the *Bucharest*. See Hilsman, *op. cit.*, p. 45.

[74] Abel, *op. cit.*, p. 153.

[75] See *ibid.*, pp. 154 ff.

[76] *Ibid.*, p. 156.

[77] *Ibid.*

account would suffice. But most "issues," e.g., Vietnam or the proliferation of nuclear weapons, emerge piecemeal, over time, one lump in one context, a second in another. Hundreds of issues compete for players' attention every day. Each player is forced to fix upon his issues for that day, fight them on their own terms, and rush on to the next. Thus the character of emerging issues and the pace at which the game is played converge to yield government "decisions" and "actions" as collages. Choices by one player, outcomes of minor games, outcomes of central games, and "foul-ups"—these pieces, when stuck to the same canvas, constitute government behavior relevant to an issue.

The concept of national security policy as political outcome contradicts both public imagery and academic orthodoxy. Issues vital to national security, it is said, are too important to be settled by political games. They must be "above" politics. To accuse someone of "playing politics with national security" is a most serious charge. What public conviction demands, the academic penchant for intellectual elegance reinforces. Internal politics is messy; moreover, according to prevailing doctrine, politicking lacks intellectual content. As such, it constitutes gossip for journalists rather than a subject for serious investigation. Occasional memoirs, anecdotes in historical accounts, and several detailed case studies to the contrary, most of the literature of foreign policy avoids bureaucratic politics. The gap between academic literature and the experience of participants in government is nowhere wider than at this point.

BUREAUCRATIC POLITICS PARADIGM[78]

I. Basic Unit of Analysis: Policy as Political Outcome

The decisions and actions of governments are essentially intra-national political outcomes: outcomes in the sense that what happens is not chosen as a solution to a problem but rather results from compromise, coalition, competition, and confusion among government officials who see different faces of an issue; political in the sense that the activity from which the outcomes emerge is best characterized as bargaining. Following Wittgenstein's use of the concept of a "game," national behavior in international affairs can be conceived as outcomes of intricate and subtle, simultaneous, overlapping games among players located in positions, the hierarchical arrangement of which constitutes the government.[79] These games proceed neither at random nor at leisure. Regular channels structure the game. Deadlines force issues to the attention of busy players. The moves in the chess game are thus to be explained in terms of the bargaining among players with separate and unequal power over particular pieces and with separable objectives in distinguishable subgames.

II. Organizing Concepts

A. *Players in Positions.* The actor is neither a unitary nation, nor a conglomerate of organizations, but rather a number of individual play-

[78] This paradigm relies upon the small group of analysts who have begun to fill the gap. My primary source is the model implicit in the work of Richard E. Neustadt, though his concentration on presidential action has been generalized to a concern with policy as the outcome of political bargaining among a number of independent players, the President amounting to no more than a "superpower" among many lesser but considerable powers. As Warner Schilling argues, the substantive problems are of such inordinate difficulty that uncertainties and differences with regard to goals, alternatives, and consequences are inevitable. This necessitates what Roger Hilsman describes as the process of conflict and consensus building. The techniques employed in this process often resemble those used in legislative assemblies, though Samuel Huntington's characterization of the process as "legislative" overemphasizes the equality of participants as opposed to the hierarchy which structures the game. Moreover, whereas for Huntington, foreign policy (in contrast to military policy) is set by the executive, this paradigm maintains that the activities which he describes as legislative are characteristic of the process by which foreign policy is made.

[79] The theatrical metaphor of stage, roles, and actors is more common than this metaphor of games, positions, and players. Nevertheless, the rigidity connotated by the concept of "role" both in the theatrical sense of actors reciting fixed lines and in the sociological sense of fixed responses to specified social situations makes the concept of games, positions, and players more useful for this analysis of active participants in the determination of national policy. Objections to the terminology on the grounds that "game" connotes non-serious play overlook the concept's application to most serious problems both in Wittgenstein's philosophy and in contemporary game theory. Game theory typically treats more precisely structured games, but Wittgenstein's examination of the "language game" wherein men use words to communicate is quite analogous to this analysis of the less specified game of bureaucratic politics. See Ludwig Wittgenstein, *Philosophical Investigations,* and Thomas Schelling, "What is Game Theory?" in James Charlesworth, *Contemporary Political Analysis.*

ers. Groups of these players constitute the agent for particular government decisions and actions. Players are men in jobs.

Individuals become players in the national security policy game by occupying a critical position in an administration. For example, in the U.S. government the players include "Chiefs": the President, Secretaries of State, Defense, and Treasury, Director of the CIA, Joint Chiefs of Staff, and, since 1961, the Special Assistant for National Security Affairs;[80] "Staffers": the immediate staff of each Chief; "Indians": the political appointees and permanent government officials within each of the departments and agencies; and *"Ad Hoc* Players": actors in the wider government game (especially "Congressional Influentials"), members of the press, spokesmen for important interest groups (especially the "bipartisan foreign policy establishment" in and out of Congress), and surrogates for each of these groups. Other members of the Congress, press, interest groups, and public form concentric circles around the central arena —circles which demarcate the permissive limits within which the game is played.

Positions define what players both may and must do. The advantages and handicaps with which each player can enter and play in various games stems from his position. So does a cluster of obligations for the performance of certain tasks. The two sides of this coin are illustrated by the position of the modern Secretary of State. First, in form and usually in fact, he is the primary repository of political judgment on the political-military issues that are the stuff of contemporary foreign policy; consequently, he is a senior personal advisor to the President. Second, he is the colleague of the President's other senior advisers on the problems of foreign policy, the Secretaries of Defense and Treasury, and the Special Assistant for National Security Affairs. Third, he is the ranking U.S. diplomat for serious negotiation. Fourth, he serves as an Administration voice to Congress, the country, and the world. Finally, he is "Mr. State Department" or "Mr. Foreign Office," "leader of officials, spokesman for their causes, guardian of their interests, judge of their disputes, superintendent of their work, master of their careers."[81] But he is not first one, and then the other. All of these obligations are his simultaneously. His performance in one affects his credit and power in the others. The perspective stemming from the daily work which he must oversee—the cable traffic by which his department maintains relations with other foreign offices—conflicts with the President's requirement that he serve as a generalist and coordinator of contrasting perspectives. The necessity that he be close to the President restricts the extent to which, and the force with which, he can front for his department. When he defers to the Secretary of Defense rather than fighting for his department's position—as he often must—he strains the loyalty of his officialdom. The Secretary's resolution of these conflicts depends not only upon the position, but also upon the player who occupies the position.

For players are also people. Men's metabolisms differ. The core of the bureaucratic politics mix is personality. How each man manages to stand the heat in his kitchen, each player's basic operating style, and the complementarity or contradiction among personalities and styles in the inner circles are irreducible pieces of the policy blend. Moreover, each person comes to his position with baggage in tow, including sensitivities to certain issues, commitments to various programs, and personal standing and debts with groups in the society.

B. *Parochial Priorities, Perceptions and Issues.* Answers to the questions: "What is the issue?" and "What must be done?" are colored by the position from which the questions are considered. For the factors which encourage organizational parochialism also influence the players who occupy positions on top of (or within) these organizations. To motivate members of his organization, a player must be sensitive to the organization's orientation. The games into which the player can enter and the advantages with which he plays enhance these pressures. Thus propensities of perception stemming from position permit reliable prediction about a player's stances in many cases. But these propensities are filtered through the baggage which players bring to positions. Sensitivity to both the pressures and the baggage is thus required for many predictions.

[80] Inclusion of the President's Special Assistant for National Security Affairs in the tier of "Chiefs" rather than among the "Staffers" involves a debatable choice. In fact he is both super-staffer and near-chief. His position has no statutory authority. He is especially dependent upon good relations with the President and the Secretaries of Defense and State. Nevertheless, he stands astride a genuine action-channel. The decision to include his position among the Chiefs reflects my judgment that the Bundy function is becoming institutionalized.

[81] Richard E. Neustadt, Testimony, United States Senate, Committee on Government Operations, Subcommittee on National Security Staffing, *Administration of National Security,* March 26, 1963, pp. 82–83.

C. *Interests, Stakes, and Power.* Games are played to determine outcomes. But outcomes advance and impede each player's conception of the national interest, specific programs to which he is committed, the welfare of his friends, and his personal interests. These overlapping interests constitute the stakes for which games are played. Each player's ability to play successfully depends upon his power. Power, i.e., effective influence on policy outcomes, is an elusive blend of at least three elements: bargaining advantages (drawn from formal authority and obligations, institutional backing, constituents, expertise, and status), skill and will in using bargaining advantages, and other players' perceptions of the first two ingredients. Power wisely invested yields an enhanced reputation for effectiveness. Unsuccessful investment depletes both the stock of capital and the reputation. Thus each player must pick the issues on which he can play with a reasonable probability of success. But no player's power is sufficient to guarantee satisfactory outcomes. Each player's needs and fears run to many other players. What ensues is the most intricate and subtle of games known to man.

D. *The Problem and the Problems.* "Solutions" to strategic problems are not derived by detached analysts focusing coolly on *the* problem. Instead, deadlines and events raise issues in games, and demand decisions of busy players in contexts that influence the face the issue wears. The problems for the players are both narrower and broader than *the* strategic problem. For each player focuses not on the total strategic problem but rather on the decision that must be made now. But each decision has critical consequences not only for the strategic problem but for each player's organizational, reputational, and personal stakes. Thus the gap between the problems the player was solving and the problem upon which the analyst focuses is often very wide.

E. *Action-Channels.* Bargaining games do not proceed randomly. Action-channels, i.e., regularized ways of producing action concerning types of issues, structure the game by pre-selecting the major players, determining their points of entrance into the game, and distributing particular advantages and disadvantages for each game. Most critically, channels determine "who's got the action," that is, which department's Indians actually do whatever is chosen. Weapon procurement decisions are made within the annual budgeting process; embassies' demands for action cables are answered according to routines of consultation and clearance from State to Defense and White House; requests for instructions from military groups (concerning assistance all the time, concerning operations during war) are composed by the military in consultation with the Office of the Secretary of Defense, State, and White House; crisis responses are debated among White House, State, Defense, CIA, and Ad Hoc players; major political speeches, especially by the President but also by other Chiefs, are cleared through established channels.

F. *Action as Politics.* Government decisions are made and government actions emerge neither as the calculated choice of a unified group, nor as a formal summary of leaders' preferences. Rather the context of shared power but separate judgments concerning important choices, determines that politics is the mechanism of choice. Note the *environment* in which the game is played: inordinate uncertainty about what must be done, the necessity that something be done, and crucial consequences of whatever is done. These features force responsible men to become active players. The *pace of the game*—hundreds of issues, numerous games, and multiple channels—compels players to fight to "get other's attention," to make them "see the facts," to assure that they "take the time to think seriously about the broader issue." The *structure of the game*—power shared by individuals with separate responsibilities—validates each player's feeling that "others don't see my problem," and "others must be persuaded to look at the issue from a less parochial perspective." The *rules of the game*—he who hesitates loses his chance to play at that point, and he who is uncertain about his recommendation is overpowered by others who are sure—pressures players to come down on one side of a 51–49 issue and play. The *rewards of the game*—effectiveness, i.e., impact on outcomes, as the immediate measure of performance—encourages hard play. Thus, most players come to fight to "make the government do what is right." The strategies and tactics employed are quite similar to those formalized by theorists of international relations.

G. *Streams of Outcomes.* Important government decisions or actions emerge as collages composed of individual acts, outcomes of minor and major games, and foul-ups. Outcomes which could never have been chosen by an actor and would never have emerged from bargaining in a single game over the issue are fabricated piece by piece. Understanding of the outcome requires that it be disaggregated.

III. Dominant Inference Pattern

If a nation performed an action, that action was the *outcome* of bargaining among individuals and groups within the government. That outcome included *results* achieved by groups

committed to a decision or action, *resultants* which emerged from bargaining among groups with quite different positions and *foul-ups*. Model III's explanatory power is achieved by revealing the pulling and hauling of various players, with different perceptions and priorities, focusing on separate problems, which yielded the outcomes that constitute the action in question.

IV. General Propositions

1. *Action and Intention.* Action does not presuppose intention. The sum of behavior of representatives of a government relevant to an issue was rarely intended by any individual or group. Rather separate individuals with different intentions contributed pieces which compose an outcome distinct from what anyone would have chosen.

2. *Where you stand depends on where you sit.*[82] Horizontally, the diverse demands upon each player shape his priorities, perceptions, and issues. For large classes of issues, e.g., budgets and procurement decisions, the stance of a particular player can be predicted with high reliability from information concerning his seat. In the notorious B-36 controversy, no one was surprised by Admiral Radford's testimony that "the B-36 under any theory of war, is a bad gamble with national security," as opposed to Air Force Secretary Symington's claim that "a B-36 with an A-bomb can destroy distant objectives which might require ground armies years to take."[83]

3. *Chiefs and Indians.* The aphorism "where you stand depends on where you sit" has vertical as well as horizontal application. Vertically, the demands upon the President, Chiefs, Staffers, and Indians are quite distinct.

The foreign policy issues with which the President can deal are limited primarily by his crowded schedule: the necessity of dealing first with what comes next. His problem is to probe the special face worn by issues that come to his attention, to preserve his leeway until time has clarified the uncertainties, and to assess the relevant risks.

Foreign policy Chiefs deal most often with the hottest issue *de jour*, though they can get the attention of the President and other members of the government for other issues which they judge important. What they cannot guarantee is that "the President will pay the price" or that "the others will get on board." They must build a coalition of the relevant powers that be. They must "give the President confidence" in the right course of action.

Most problems are framed, alternatives specified, and proposals pushed, however, by Indians. Indians fight with Indians of other departments; for example, struggles between International Security Affairs of the Department of Defense and Political-Military of the State Department are a microcosm of the action at higher levels. But the Indian's major problem is how to get the *attention* of Chiefs, how to get an issue decided, how to get the government "to do what is right."

In policy making then, the issue looking *down* is options: how to preserve my leeway until time clarifies uncertainties. The issue looking *sideways* is commitment: how to get others committed to my coalition. The issue looking *upwards* is confidence: how to give the boss confidence in doing what must be done. To paraphrase one of Neustadt's assertions which can be applied down the length of the ladder, the essence of a responsible official's task is to induce others to see that what needs to be done is what their own appraisal of their own responsibilities requires them to do in their own interests.

V. Specific Propositions

1. *Deterrence.* The probability of nuclear attack depends primarily on the probability of attack emerging as an outcome of the bureaucratic politics of the attacking government. First, which players can decide to launch an attack? Whether the effective power over action is controlled by an individual, a minor game, or the central game is critical. Second, though Model I's confidence in nuclear deterrence stems from an assertion that, in the end, governments will not commit suicide, Model III recalls historical precedents. Admiral Yamamoto, who designed the Japanese attack on Pearl Harbor, estimated accurately: "In the first six months to a year of war against the U.S. and England I will run wild, and I will show you an uninterrupted succession of victories; I must also tell you that, should the war be prolonged for two or three years, I have no confidence in our ultimate victory."[84] But Japan attacked. Thus, three questions might be considered. One: could any member of the government solve his problem by attack? What patterns of bargaining could yield attack as an outcome? The major difference between a stable balance of terror and a questionable balance may simply be that in the first case most members of the government

[82] This aphorism was stated first, I think, by Don K. Price.

[83] Paul Y. Hammond, "Super Carriers and B-36 Bombers," in Harold Stein (ed.), *American Civil-Military Decisions* (Birmingham, 1963).

[84] Roberta Wohlstetter, *Pearl Harbor* (Stanford, 1962), p. 350.

appreciate fully the consequences of attack and are thus on guard against the emergence of this outcome. Two: what stream of outcomes might lead to an attack? At what point in that stream is the potential attacker's politics? If members of the U.S. government had been sensitive to the stream of decisions from which the Japanese attack on Pearl Harbor emerged, they would have been aware of a considerable probability of that attack. Three: how might miscalculation and confusion generate foul-ups that yield attack as an outcome? For example, in a crisis or after the beginning of conventional war, what happens to the information available to, and the effective power of, members of the central game.

THE U.S. BLOCKADE OF CUBA: A THIRD CUT

The Politics of Discovery. A series of overlapping bargaining games determined both the *date* of the discovery of the Soviet missiles and the *impact* of this discovery on the Administration. An explanation of the politics of the discovery is consequently a considerable piece of the explanation of the U.S. blockade.

Cuba was the Kennedy Administration's "political Achilles' heel."[85] The months preceding the crisis were also months before the Congressional elections, and the Republican Senatorial and Congressional Campaign Committee had announced that Cuba would be "the dominant issue of the 1962 campaign."[86] What the administration billed as a "more positive and indirect approach of isolating Castro from developing, democratic Latin America," Senators Keating, Goldwater, Capehart, Thurmond, and others attacked as a "do-nothing" policy.[87] In statements on the floor of the House and Senate, campaign speeches across the country, and interviews and articles carried by national news media, Cuba—particularly the Soviet program of increased arms aid—served as a stick for stirring the domestic political scene.[88]

These attacks drew blood. Prudence demanded a vigorous reaction. The President decided to meet the issue head-on. The Administration mounted a forceful campaign of denial designed to discredit critics' claims. The President himself manned the front line of this offensive, though almost all Administration officials participated. In his news conference on August 19, President Kennedy attacked as "irresponsible" calls for an invasion of Cuba, stressing rather "the totality of our obligations" and promising to "watch what happens in Cuba with the closest attention."[89] On September 4, he issued a strong statement denying any provocative Soviet action in Cuba.[90] On September 13 he lashed out at "loose talk" calling for an invasion of Cuba.[91] The day before the flight of the U-2 which discovered the missiles, he campaigned in Capehart's Indiana against those "self-appointed generals and admirals who want to send someone else's sons to war."[92]

On Sunday, October 14, just as a U-2 was taking the first pictures of Soviet missiles, McGeorge Bundy was asserting:

> I *know* that there is no present evidence, and I think that there is no present likelihood that the Cuban government and the Soviet government would, in combination, attempt to install a major offensive capability.[93]

In this campaign to puncture the critics' charges, the Administration discovered that the public needed positive slogans. Thus, Kennedy fell into a tenuous semantic distinction between "offensive" and "defensive" weapons. This distinction originated in his September 4 statement that there was no evidence of "offensive ground to ground missiles" and warned "were it to be otherwise, the gravest issues would arise."[94] His September 13 statement turned on this distinction between "defensive" and "offensive" weapons and announced a firm commitment to action if the Soviet Union attempted to introduce the latter into Cuba.[95] Congressional committees elicited from administration officials testimony which read this distinction and the President's commitment into the *Congressional Record.*[96]

What the President least wanted to hear, the CIA was most hesitant to say plainly. On August 22 John McCone met privately with the President and voiced suspicions that the Soviets were preparing to introduce offensive missiles into Cuba.[97] Kennedy heard this as what it was: the suspicion of a hawk. McCone left

[85] Sorensen, *Kennedy,* p. 670.

[86] *Ibid.*

[87] *Ibid.,* pp. 670ff.

[88] *New York Times,* August, September, 1962.

[89] *New York Times,* August 20, 1962.

[90] *New York Times,* September 5, 1962.

[91] *New York Times,* September 14, 1962.

[92] *New York Times,* October 14, 1962.

[93] Cited by Abel, *op. cit.,* p. 13.

[94] *New York Times,* September 5, 1962.

[95] *New York Times,* September 14, 1962.

[96] Senate Foreign Relations Committee; Senate Armed Services Committee; House Committee on Appropriation; House Select Committee on Export Control.

[97] Abel, *op. cit.,* pp. 17–18. According to McCone, he told Kennedy, "The only construction I can put on the material going into Cuba is that the Russians are preparing to introduce offensive missiles." See also Weintal and Bartlett, *op. cit.,* pp. 60–61.

Washington for a month's honeymoon on the Riviera. Fretting at Cap Ferrat, he bombarded his deputy, General Marshall Carter, with telegrams, but Carter, knowing that McCone had informed the President of his suspicions and received a cold reception, was reluctant to distribute these telegrams outside the CIA.[98] On September 9 a U-2 "on loan" to the Chinese Nationalists was downed over mainland China.[99] The Committee on Overhead Reconnaissance (COMOR) convened on September 10 with a sense of urgency.[100] Loss of another U-2 might incite world opinion to demand cancellation of U-2 flights. The President's campaign against those who asserted that the Soviets were acting provocatively in Cuba had begun. To risk downing a U-2 over Cuba was to risk chopping off the limb on which the President was sitting. That meeting decided to shy away from the western end of Cuba (where SAMs were becoming operational) and modify the flight pattern of the U-2s in order to reduce the probability that a U-2 would be lost.[101] USIB's unanimous approval of the September estimate reflects similar sensitivities. On September 13 the President had asserted that there were no Soviet offensive missiles in Cuba and committed his Administration to act if offensive missiles were discovered. Before Congressional committees, Administration officials were denying that there was any evidence whatever of offensive missiles in Cuba. The implications of a National Intelligence estimate which concluded that the Soviets were introducing offensive missiles into Cuba were not lost on the men who constituted America's highest intelligence assembly.

The October 4 COMOR decision to direct a flight over the western end of Cuba in effect "overturned" the September estimate, but without officially raising that issue. The decision represented McCone's victory for which he had lobbied with the President before the September 10 decision, in telegrams before the September 19 estimate, and in person after his return to Washington. Though the politics of the intelligence community is closely guarded, several pieces of the story can be told.[102] By September 27, Colonel Wright and others in DIA believed that the Soviet Union was placing missiles in the San Cristobal area.[103] This area was marked suspicious by the CIA on September 29 and certified top priority on October 3. By October 4 McCone had the evidence required to raise the issue officially. The members of COMOR heard McCone's argument, but were reluctant to make the hard decision he demanded. The significant probability that a U-2 would be downed made overflight of western Cuba a matter of real concern.[104]

The Politics of Issues. The U-2 photographs presented incontrovertible evidence of Soviet offensive missiles in Cuba. This revelation fell upon politicized players in a complex context. As one high official recalled, Khrushchev had caught us "with our pants down." What each of the central participants saw, and what each did to cover both his own and the Administration's nakedness, created the spectrum of issues and answers.

At approximately 9:00 A.M., Tuesday morning, October 16, McGeorge Bundy went to the President's living quarters with the message: "Mr. President, there is now hard photographic evidence that the Russians have offensive missiles in Cuba."[105] Much has been made of Kennedy's "expression of surprise,"[106] but "surprise" fails to capture the character of his initial reaction. Rather, it was one of startled anger, most adequately conveyed by the exclamation: "He can't do that to *me!*"[107] In terms of the President's attention and priorities at that moment, Khrushchev had chosen the most unhelpful act of all. Kennedy had staked his full Presidential authority on the assertion that the Soviets would not place offensive weapons in Cuba. Moreover, Khrushchev had assured the President through the most direct and personal channels that he was aware of the President's domestic political problem and that nothing would be done to exacerbate this problem. The Chairman had *lied* to the President. Kennedy's initial reaction entailed action. The missiles must be removed.[108] The alternatives of "doing nothing" or "taking a diplomatic approach" could not have been less relevant to *his* problem.

These two tracks—doing nothing and taking

[98] Abel, *op. cit.*, p. 23.

[99] *New York Times*, September 10, 1962.

[100] See Abel, *op. cit.*, pp. 25–26; and Hilsman, *op. cit.*, p. 174.

[101] Department of Defense Appropriation, *Hearings*, 69.

[102] A basic, but somewhat contradictory, account of parts of this story emerges in the Department of Defense Appropriations, *Hearings*, 1–70.

[103] Department of Defense Appropriations, *Hearings*, 71.

[104] The details of the 10 days between the October 4 decision and the October 14 flight must be held in abeyance.

[105] Abel, *op. cit.*, p. 44.

[106] *Ibid.*, pp. 44ff.

[107] See Richard Neustadt, "Afterword," *Presidential Power* (New York, 1964).

[108] Sorensen, *Kennedy*, p. 676; Schlesinger, *op. cit.*, p. 801.

a diplomatic approach—were the solutions advocated by two of his principal advisors. For Secretary of Defense McNamara, the missiles raised the spectre of nuclear war. He first framed the issue as a straightforward strategic problem. To understand the issue, one had to grasp two obvious but difficult points. First, the missiles represented an inevitable occurrence: narrowing of the missile gap. It simply happened sooner rather than later. Second, the United States could accept this occurrence since its consequences were minor: "seven-to-one missile 'superiority,' one-to-one missile 'equality,' one-to-seven missile 'inferiority'—the three postures are identical." McNamara's statement of this argument at the first meeting of the ExCom was summed up in the phrase, "a missile is a missile."[109] "It makes no great difference," he maintained, "whether you are killed by a missile from the Soviet Union or Cuba."[110] The implication was clear. The United States should not initiate a crisis with the Soviet Union, risking a significant probability of nuclear war over an occurrence which had such small strategic implications.

The perceptions of McGeorge Bundy, the President's Assistant for National Security Affairs, are the most difficult of all to reconstruct. There is no question that he initially argued for a diplomatic track.[111] But was Bundy laboring under his acknowledged burden of responsibility in Cuba I? Or was he playing the role of devil's advocate in order to make the President probe his own initial reaction and consider other options?

The President's brother, Robert Kennedy, saw most clearly the political wall against which Khrushchev had backed the President. But he, like McNamara, saw the prospect of nuclear doom. Was Khrushchev going to force the President to an insane act? At the first meeting of the ExCom, he scribbled a note, "Now I know how Tojo felt when he was planning Pearl Harbor."[112] From the outset he searched for an alternative that would prevent the air strike.

The initial reaction of Theodore Sorensen, the President's Special Counsel and "alter ego," fell somewhere between that of the President and his brother. Like the President, Sorensen felt the poignancy of betrayal. If the President had been the architect of the policy which the missiles punctured, Sorensen was the draftsman. Khrushchev's deceitful move demanded a strong counter-move. But like Robert Kennedy, Sorensen feared lest the shock and disgrace lead to disaster.

To the Joint Chiefs of Staff the issue was clear. *Now* was the time to do the job for which they had prepared contingency plans. Cuba I had been badly done; Cuba II would not be. The missiles provided the *occasion* to deal with the issue: cleansing the Western Hemisphere of Castro's Communism. As the President recalled on the day the crisis ended, "An invasion would have been a mistake—a wrong use of our power. But the military are mad. They wanted to do this. It's lucky for us that we have McNamara over there."[113]

McCone's perceptions flowed from his confirmed prediction. As the Cassandra of the incident, he argued forcefully that the Soviets had installed the missiles in a daring political probe which the United States must meet with force. The time for an air strike was now.[114]

The Politics of Choice. The process by which the blockade emerged is a story of the most subtle and intricate probing, pulling, and hauling; leading, guiding, and spurring. Reconstruction of this process can only be tentative. Initially the President and most of his advisers wanted the clean, surgical air strike. On the first day of the crisis, when informing Stevenson of the missiles, the President mentioned only two alternatives: "I suppose the alternatives are to go in by air and wipe them out, or to take other steps to render them inoperable."[115] At the end of the week a sizeable minority still favored an air strike. As Robert Kennedy recalled: "The fourteen people involved were very significant. . . . If six of them had been President of the U.S., I think that the world might have been blown up."[116] What prevented the air strike was a fortuitous coincidence of a number of factors—the absence of any one of which might have permitted that option to prevail.

First, McNamara's vision of holocaust set him firmly against the air strike. His initial attempt to frame the issue in strategic terms struck Kennedy as particularly inappropriate. Once McNamara realized that the name of the game was a strong response, however, he and his deputy Gilpatric chose the blockade as a fallback. When the Secretary of Defense—whose department had the action, whose reputation in the Cabinet was unequaled, in whom the President demonstrated full confidence—marshalled

[109] Hilsman, *op. cit.*, p. 195.

[110] *Ibid.*

[111] Weintal and Bartlett, *op. cit.*, p. 67; Abel, *op. cit.*, p. 53.

[112] Schlesinger, *op. cit.*, p. 803.

[113] *Ibid.*, p. 831.

[114] Abel, *op. cit.*, p. 186.

[115] *Ibid.*, p. 49.

[116] Interview, quoted by Ronald Steel, *New York Review of Books*, March 13, 1969, p. 22.

the arguments for the blockade and refused to be moved, the blockade became a formidable alternative.

Second, Robert Kennedy—the President's closest confidant—was unwilling to see his brother become a "Tojo." His arguments against the air strike on moral grounds struck a chord in the President. Moreover, once his brother had stated these arguments so forcefully, the President could not have chosen his initially preferred course without, in effect, agreeing to become what RFK had condemned.

The President learned of the missiles on Tuesday morning. On Wednesday morning, in order to mask our discovery from the Russians, the President flew to Connecticut to keep a campaign commitment, leaving RFK as the unofficial chairman of the group. By the time the President returned on Wednesday evening, a critical third piece had been added to the picture. McNamara had presented his argument for the blockade. Robert Kennedy and Sorensen had joined McNamara. A powerful coalition of the advisers in whom the President had the greatest confidence, and with whom his style was most compatible, had emerged.

Fourth, the coalition that had formed behind the President's initial preference gave him reason to pause. *Who* supported the air strike—the Chiefs, McCone, Rusk, Nitze, and Acheson—as much as *how* they supported it, counted. Fifth, a piece of inaccurate information, which no one probed, permitted the blockade advocates to fuel (potential) uncertainties in the President's mind. When the President returned to Washington Wednesday evening, RFK and Sorensen met him at the airport. Sorensen gave the President a four-page memorandum outlining the areas of agreement and disagreement. The strongest argument was that the air strike simply could not be surgical.[117] After a day of prodding and questioning, the Air Force had asserted that it could not guarantee the success of a surgical air strike limited to the missiles alone.

Thursday evening, the President convened the ExCom at the White House. He declared his tentative choice of the blockade and directed that preparations be made to put it into effect by Monday morning.[118] Though he raised a question about the possibility of a surgical air strike subsequently, he seems to have accepted the experts' opinion that this was no live option.[119] (Acceptance of this estimate suggests that he may have learned the lesson of the Bay of Pigs—"Never rely on experts"—less well than he supposed.)[120] But this information was incorrect. That no one probed this estimate during the first week of the crisis poses an interesting question for further investigation.

A coalition, including the President, thus emerged from the President's initial decision that something had to be done; McNamara, Robert Kennedy, and Sorensen's resistance to the air strike; incompatibility between the President and the air strike advocates; and an inaccurate piece of information.[121]

CONCLUSION

This essay has obviously bitten off more than it has chewed. For further developments and synthesis of these arguments the reader is referred to the larger study.[122] In spite of the limits of space, however, it would be inappropriate to stop without spelling out several implications of the argument and addressing the question of relations among the models and extensions of them to activity beyond explanation.

At a minimum, the intended implications of the argument presented here are four. First, formulation of alternative frames of reference and demonstration that different analysts, relying predominantly on different models, produce quite different explanations should encourage the analyst's self-consciousness about the nets he employs. The effect of these "spectacles" in sensitizing him to particular aspects of what is going on—framing the puzzle in one way rather than another, encouraging him to examine the problem in terms of certain categories rather than others, directing him to particular kinds of evidence, and relieving puzzlement by one procedure rather than another—must be recognized and explored.

Second, the argument implies a position on the problem of "the state of the art." While accepting the commonplace characterization of the present condition of foreign policy analysis—personalistic, non-cumulative, and sometimes insightful—this essay rejects both the counsel of despair's justification of this condition as a consequence of the character of the enterprise, and the "new frontiersmen's" demand for *a priori* theorizing on the frontiers and *ad hoc* appropriation of "new techniques."[123] What is re-

[117] Sorensen, *Kennedy*, p. 686.

[118] *Ibid.*, p. 691.

[119] *Ibid.*, pp. 691–692.

[120] Schlesinger, *op. cit.*, p. 296.

[121] Space will not permit an account of the path from this coalition to the formal government decision on Saturday and action on Monday.

[122] *Bureaucracy and Policy* (forthcoming, 1969).

[123] Thus my position is quite distinct from both poles in the recent "great debate" about international relations. While many "traditionalists" of the sort Kaplan attacks adopt the first posture and

quired as a first step is non-casual examination of the present product: inspection of existing explanations, articulation of the conceptual models employed in producing them, formulation of the propositions relied upon, specification of the logic of the various intellectual enterprises, and reflection on the questions being asked. Though it is difficult to overemphasize the need for more systematic processing of more data, these preliminary matters of formulating questions with clarity and sensitivity to categories and assumptions so that fruitful acquisition of large quantities of data is possible are still a major hurdle in considering most important problems.

Third, the preliminary, partial paradigms presented here provide a basis for serious reexamination of many problems of foreign and military policy. Model II and Model III cuts at problems typically treated in Model I terms can permit significant improvements in explanation and prediction.[124] Full Model II and III analyses require large amounts of information. But even in cases where the information base is severely limited, improvements are possible. Consider the problem of predicting Soviet strategic forces. In the mid-1950s, Model I style calculations led to predictions that the Soviets would rapidly deploy large numbers of long-range bombers. From a Model II perspective, both the frailty of the Air Force within the Soviet military establishment and the budgetary implications of such a buildup, would have led analysts to hedge this prediction. Moreover, Model II would have pointed to a sure, visible indicator of such a buildup: noisy struggles among the Services over major budgetary shifts. In the late 1950s and early 1960s, Model I calculations led to the prediction of immediate, massive Soviet deployment of ICBMs. Again, a Model II cut would have reduced this number because, in the earlier period, strategic rockets were controlled by the Soviet Ground Forces rather than an independent Service, and in the later period, this would have necessitated massive shifts in budgetary splits. Today, Model I considerations lead many analysts both to recommend that an agreement not to deploy ABMs be a major American objective in upcoming strategic negotiations with the USSR, and to predict success. From a Model II vantage point, the existence of an ongoing Soviet ABM program, the strength of the organization (National Air Defense) that controls ABMs, and the fact that an agreement to stop ABM deployment would force the virtual dismantling of this organization, make a viable agreement of this sort much less likely. A Model III cut suggests that (a) there must be significant differences among perceptions and priorities of Soviet leaders over strategic negotiations, (b) any agreement will affect some players' power bases, and (c) agreements that do not require extensive cuts in the sources of some major players' power will prove easier to negotiate and more viable.

Fourth, the present formulation of paradigms is simply an initial step. As such it leaves a long list of critical questions unanswered. Given any action, an imaginative analyst should always be able to construct some rationale for the government's choice. By imposing, and relaxing, constraints on the parameters of rational choice (as in variants of Model I) analysts can construct a large number of accounts of any act as a rational choice. But does a statement of reasons why a rational actor would choose an action constitute an explanation of the *occurrence* of that action? How can Model I analysis be forced to make more systematic contributions to the question of the determinants of occurrences? Model II's explanation of t in terms of $t - 1$ is explanation. The world is contiguous. But governments sometimes make sharp departures. Can an organizational process model be modified to suggest where change is likely? Attention to organizational change should afford greater understanding of why particular programs and SOPs are maintained by identifiable types of organizations and also how a manager can improve organizational performance. Model III tells a fascinating "story." But its complexity is enormous, the information requirements are often overwhelming, and many of the details of the bargaining may be superfluous. How can such a model be made parsimonious? The three models are obviously not exclusive alternatives. Indeed, the paradigms highlight the partial emphasis of the framework—what each emphasizes and what it leaves out. Each concentrates on one class of variables, in effect, relegating other important factors to a *ceteris paribus* clause. Model I concentrates on "market factors:" pressures and incentives created by the "international strategic marketplace." Mod-

many "scientists" of the sort attacked by Bull adopt the second, this third posture is relatively neutral with respect to whatever is in substantive dispute. See Hedly Bull, "International Theory: The Case for a Classical Approach," *World Politics* (April, 1966); and Morton Kaplan, "The New Great Debate: Traditionalism vs. Science in International Relations," *World Politics* (October, 1966).

[124] A number of problems are now being examined in these terms both in the Bureaucracy Study Group on Bureaucracy and Policy of the Institute of Politics at Harvard University and at the Rand Corporation.

els II and III focus on the internal mechanism of the government that chooses in this environment. But can these relations be more fully specified? Adequate synthesis would require a typology of decisions and actions, some of which are more amenable to treatment in terms of one model and some to another. Government behavior is but one cluster of factors relevant to occurrences in foreign affairs. Most students of foreign policy adopt this focus (at least when explaining and predicting). Nevertheless, the dimensions of the chess board, the character of the pieces, and the rules of the game—factors considered by international systems theorists—constitute the context in which the pieces are moved. Can the major variables in the full function of determinants of foreign policy outcomes be identified?

Both the outline of a partial, *ad hoc* working synthesis of the models, and a sketch of their uses in activities other than explanation can be suggested by generating predictions in terms of each. Strategic surrender is an important problem of international relations and diplomatic history. War termination is a new, developing area of the strategic literature. Both of these interests lead scholars to address a central question: *Why* do nations surrender *when?* Whether implicit in explanations or more explicit in analysis, diplomatic historians and strategists rely upon propositions which can be turned forward to produce predictions. Thus at the risk of being timely—and in error—the present situation (August, 1968) offers an interesting test case: Why will North Vietnam surrender when?[125]

In a nutshell, analysis according to Model I asserts: nations quit when costs outweigh the benefits. North Vietnam will surrender when she realizes "that continued fighting can only generate additional costs without hope of compensating gains, this expectation being largely the consequence of the previous application of force by the dominant side."[126] U.S. actions can increase or decrease Hanoi's strategic costs. Bombing North Vietnam increases the pain and thus increases the probability of surrender. This proposition and prediction are not without meaning. That—"other things being equal"—nations are more likely to surrender when the strategic cost-benefit balance is negative, is true. Nations rarely surrender when they are winning. The proposition specifies a range within which nations surrender. But over this broad range, the relevant question is: why do nations surrender?

Models II and III focus upon the government machine through which this fact about the international strategic marketplace must be filtered to produce a surrender. These analysts are considerably less sanguine about the possibility of surrender *at the point* that the cost-benefit calculus turns negative. Never in history (i.e., in none of the five cases I have examined) have nations surrendered at that point. Surrender occurs sometime thereafter. *When* depends on process of organizations and politics of players within these governments—as they are affected by the opposing government. Moreover, the effects of the victorious power's action upon the surrendering nation cannot be adequately summarized as increasing or decreasing strategic costs. Imposing additional costs by bombing a nation may increase the probability of surrender. But it also may reduce it. An appreciation of the impact of the acts of one nation upon another thus requires some understanding of the machine which is being influenced. For more precise prediction, Models II and III require considerably more information about the organizations and politics of North Vietnam than is publicly available. On the basis of the limited public information, however, these models can be suggestive.

Model II examines two sub-problems. First, to have lost is not sufficient. The government must know that the strategic cost-benefit calculus is negative. But neither the categories, nor the indicators, of strategic costs and benefits are clear. And the sources of information about both are organizations whose parochial priorities and perceptions do not facilitate accurate information or estimation. Military evaluation of military performance, military estimates of factors like "enemy morale," and military predictions concerning when "the tide will turn" or "the corner will have been turned" are typically distorted. In cases of highly decentralized guerrilla operations, like Vietnam, these problems are exacerbated. Thus strategic costs will be underestimated. Only highly *visible* costs can have direct impact on leaders without being filtered through organizational channels. Second, since organizations define the details of options and execute actions, surrender (and negotiation) is likely to entail considerable bungling in the early stages. No organization can define options or prepare programs for this treasonous act. Thus, early overtures will be uncoordinated with

[125] In response to several readers' recommendations, what follows is reproduced *verbatim* from the paper delivered at the September, 1968 Association meetings (Rand P-3919). The discussion is heavily indebted to Ernest R. May.

[126] Richard Snyder, *Deterrence and Defense* (Princeton, 1961), p. 11. For a more general presentation of this position see Paul Kecskemeti, *Strategic Surrender* (New York, 1964).

the acts of other organizations, e.g., the fighting forces, creating contradictory "signals" to the victor.

Model III suggests that surrender will not come at the point that strategic costs outweigh benefits, but that it will not wait until the leadership group concludes that the war is lost. Rather the problem is better understood in terms of four additional propositions. First, strong advocates of the war effort, whose careers are closely identified with the war, rarely come to the conclusion that costs outweigh benefits. Second, quite often from the outset of a war, a number of members of the government (particularly those whose responsibilities sensitize them to problems other than war, e.g., economic planners or intelligence experts) are convinced that the war effort is futile. Third, surrender is likely to come as the result of a political shift that enhances the effective power of the latter group (and adds swing members to it). Fourth, the course of the war, particularly actions of the victor, can influence the advantages and disadvantages of players in the loser's government. Thus, North Vietnam will surrender not when its leaders have a change of heart, but when Hanoi has a change of leaders (or a change of effective power within the central circle). How U.S. bombing (or pause), threats, promises, or action in the South affect the game in Hanoi is subtle but nonetheless crucial.

That these three models could be applied to the surrender of governments other than North Vietnam should be obvious. But that exercise is left for the reader.

A TRANSACTIONAL THEORY OF POLITICAL INTEGRATION AND ARMS CONTROL*

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Nominally, the English became a nation in the eighth century but did not achieve political integration until the seventeenth century, a thousand years later. During the millennium, the English "nation" was the scene of recurring internal wars, the last ending with the acceptance of the Bill of Rights by William III and Mary in 1688. What was the process leading to cessation in the use of armed conflict as a technique of domestic politics in England?

Nominally, Mexico was an independent nation in 1821 but did not see the end of its internal wars until the 1940's. What political process led to domestic "arms control" in Mexico?

Although taking place in different centuries and in nations with distinct political cultures, were there common elements in the two transitions to internal arms control? What were critical factors in the integrative process? May the same factors, or analogous ones, be identified and controlled in contemporary efforts related to regional and international arms control? What may be learned from the English, the Mexican, and other national cases that is generalizable to the problem of international political integration and arms control?

The present theory sketch views arms control as an aspect of the integration of political organizations. Political integration, in turn, is the consequence of a process of political transactions among principal political actors over time. In keeping with numerous contemporary conceptions of social transaction, political "things" of one kind or another are exchanged between actors when *both* (or more) parties perceive or anticipate a "profit," that is, applying their own individual criteria, see a net gain when measuring what is received against what is given up in the transaction. In order to quantify and relate these transactional events to the process leading to political integration, three types of "currency" are postulated: positional, decisional, and materiel currencies. These types are employed in the analysis of the content of each political transaction. The following general hypotheses are then proposed:

1. Successive transactions perceived as profitable by all parties tend to reinforce attitudes of trust among political transactors.
2. Trust among political transactors is positively correlated with sharp increases in the inclusion of decisional currencies among the content of transactions.
3. Increased trust is positively associated with the decline in political exchanges involving armaments (a sub-type of materiel currency), that is, increasing trust and decreasing violent warfare are positively correlated.
4. Transactional sequences that follow a trust-inducing pattern tend to be associated with successful transition from unintegrated to integrated states of political organization.[1]

[1] The distinction between "disarmament" and "arms control" is significant. Neither England nor Mexico, for example, is a domestically disarmed nation. Rather, as a consequence of the processes to be examined with this theory, each nation—as others that are politically integrated—has established a monopoly of its major means of internal violence. Domestic peace, that is, the cessation of internal wars, seems to be associated with conditions of arms *control* (involving decisional currency exchanges) rather than disarmament.

"Political development" and "political integration" are also troublesome terms that should not delay us long. Since World War II, the rubric "political development" has tended to be employed mainly to describe comparative studies of national modernization, particularly with respect to the growth of economic organization and governmental bureaucracies. Overviews of current usage are reported in Samuel P. Huntington, "Political Development and Political Decay," *World Politics*, 17 (April, 1965), 386–430; Fred W. Riggs, "The Theory of Political Development," in J. C. Charlesworth (ed.), *Contemporary Political Analysis* (New York: The Free Press 1967), pp. 317–349; Robert T. Holt and John E. Turner, *The Political Basis of Economic Development* (Princeton, N.J.: D. Van Nostrand, 1966). The approach described in this report is "developmental" simply in its search for recurring tendencies.

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I. TRANSACTIONS, TRUST, AND ORGANIZATIONAL INTEGRATION

Warfare—whether international or domestic—is a "costly" activity. It is costly to be armed. It is costly to expend armaments in military conflicts. The cost may be measured in the usual economic sense; thus, the annual defense budget of the United States during the Korean War rose from $12 billions to $41 billions. The cost may be measured in lives; e.g., President Lyndon B. Johnson's comment, as he signed orders raising the troop commitments to Vietnam: ". . . If I retreated from Vietnam, I believe it would cost us more boys than it would to stay."[2] But history is replete with references to non-pecuniary, non-personnel costs of warfare. Reputedly, Joan of Arc feared bringing her military campaigns to an end lest it cost her the grace of God. Patrick Henry and other libertarians invariably perceive the loss of liberty as costlier than death during resistance; the contemporary version of "give me liberty or give me death" is presumably "better dead than Red."

Evidently cost units other than dollars and lives are critically relevant to political judgments about engaging in or avoiding warfare. One question may be put as follows: Under what conditions do political decision-makers consider the expenditure of military resources, in arms races or actual warfare, to be *too costly* for the political objectives they seek? Deterrence theorists will recognize the variation on a familiar problem: "How much power—military, economic, etc.—do *we* need to make an adversary see the risks and probable costs of *his* war-like acts?" The focus in deterrence theory is on the other decision-maker as *he* perceives and as *he* measures costs.

If cost judgments are at the heart of the choice between warfare and non-warfare, then the problem may be a political-economic one.[3] This is the view taken here. Political integration, and its concomitant arms control and cessation of violence, are viewed hypothetically as consequences of historic series of "political deals," or transactions, wherein adversary political decision-makers pass through a transitional stage beyond which they believe they may regularly get what they want from their dealings with each other by trading "political currencies" less costly than military resources and less risky than warfare. Certain characteristics of these series of political deals, it appears, are importantly related to attitudes of trust, another condition antecedent to the transition to arms control.

Specifically, a theory sketch about political transaction is set forth, in which a typology of political currencies is offered for *ex post facto* analysis of historic exchanges associated with processes of national political integration. Preliminary findings in English and Mexican case studies suggest that internal wars recur until a particular pattern of political transactions, to be described as "Pattern T" below, takes place.[4]

[2] Interview in *Newsweek*, August 2, 1965, p. 21.

[3] Current interest in political economy is surveyed by William C. Mitchell in "The Shape of Political Theory to Come; From Political Sociology to Political Economy," *American Behavioral Scientist*, 11 (November, 1967), 8–37, and in "The New Political Economy," *Social Research*, 35 (Spring, 1968), 76–110. See also, R. L. Curry, Jr., *A Theory of Political Exchange* (Englewood Cliffs, New Jersey: Prentice-Hall, 1969). Particular impetus has been provided by such works as Robert A. Dahl and Charles E. Lindblom, *Politics, Economics and Welfare* (New York: Harper, 1953), Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957), and James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: University of Michigan Press, 1962).

[4] Some theoretical difficulties of the case study method of political inquiry are described in Harold Stein (ed.), *Public Administration and Policy Development; A Case Book* (New York: Harcourt, Brace & World, 1952), pp. xx-xxx; also, Bernard Berelson and G. A. Steiner, *Human Behavior* (New York: Harcourt, Brace & World, 1964), p. 27. The logical and empirical pitfalls of *ex post facto* research are succinctly stated in Fred N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt, Rinehart and Winston, 1966), pp. 360 ff., whereas the more general requirements of scientific historiography are discussed in Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace & World, 1961), Chap. 15; Carl G. Hempel, "The Functions of General Laws in History," in Herbert Feigl and Wilfred Sellars (eds.), *Readings in Philosophical Analysis* (New York: Appleton-Century-Crofts, 1949), pp. 459–471; and Patrick Gardiner, *The Nature of Historical Explanation* (New York: Oxford University Press, 1952). To cope with some of the limitations of *ex post facto* tests of hypotheses, this project anticipates developing a simulation experiment which will presumably permit measurement of player behavior as related to currency transactions, attitudes of trust and suspicion, and other variables of the present theory. For an important attempt to replicate historical events through an experimental simulation, see Charles F. Hermann and Margaret G. Hermann,

The "political currencies" exchanged in the course of this transactional pattern tend to promote attitudes of trust among political transactors. These attitudes in turn facilitate a "critical transitional phase" in national evolution toward domestic arms control, disappearance of internal wars, and political integration.

Transaction Theory

Exchange, or transactional, explanations of political events and behavior have great antiquity. Plato, in discussing the essential activities of inhabitants of a state, observed that "they exchange with one another, and one gives and another receives, each under the idea that the exchange will be for his own good." Aristotle spoke of a conception of "distributive justice" according to which the offices of the state and the rights adhering to these offices are distributed to each citizen in proportion to his contribution to the ends of the state, that is, to a good quality of life. As persons A and B give to the state, in personal merit and personal contribution to its well-being, so A and B receive from the state, in office and honor. After describing this political exchange between citizen and state, Aristotle acknowledged the difficulties arising from different criteria for judging what constitutes a meritorious contribution. Each social class, he observed, tries to have its particular criterion, whether wealth, noble birth, personal goodness, greater numbers, or some other, prevail, thus leading to different forms of state and to the revolutions that change states from one form to another.[5]

A transaction between the citizen and his government is implied in Locke's "social contract." Adam Smith, David Hume, Jeremy Bentham, Thomas Malthus, and the later Utilitarians emphasized the place of each citizen's personal valuational yardstick in the political economy created by the state. James Madison applied a market-place perspective to his analysis of group and governmental relations. Edmund Burke declared:

> . . . All government, indeed every human benefit and enjoyment, every virtue, and every prudent act, is founded on compromise and barter. We balance inconveniences; we give and take; we remit some rights, that we may enjoy others. . . . In every arduous enterprise we consider what we are to lose, as well as what we are to gain. . . . Man acts from adequate motives relative to his interest and not on metaphysical speculation.[6]

The "broker" aspect of the politician's role was alluded to by Sir Henry Maine in his critique of democracy:

> . . . There is no doubt then, in popular government resting on a wide suffrage . . . the leader . . . will be the Wire-puller. The process of cutting up political power into petty fragments has in him its most remarkable product. The morsels of power are so small that men, if left to themselves, would not care to employ them . . . But the political chiffonnier who collects and utilizes the fragments is the Wire-puller.[7]

Lowell carried forward the notion that the politician is primarily a "broker" among group interests; democracy "requires a species of brokerage and one of the functions of politicians is that of brokers." Bentley, Lasswell, Dahl and Lindblom, Downs, and Buchanan and Tullock follow this tradition.[8]

For economists, the exchange concept is fairly well-defined. Together with production and consumption, exchange of goods is a core concern of economics. "Exchange" has been defined in economic theory as "a situation in which each of two parties provides some good(s) to the other." In the more traditional usage, the giving up of goods by one party is contingent upon receipt of goods from the other. A "transaction," then, is an exchange, along with the attendant bargaining or negotiations. There may be two or more parties involved in a transaction, and the parties may be either individuals or organizations.[9]

George C. Homans incorporated elements of classical economics and behavioral psychology

"An Attempt to Simulate the Outbreak of World War I," this REVIEW, 61 (June, 1967), 400–416.

[5] Plato's *Republic*, Book Two; Ernest Barker, *The Politics of Aristotle* (London: Oxford University Press, 1946), pp. 115–137.

[6] John Locke, *Second Treatise on Civil Government* (1689); James Madison's *Federalist Papers*, Number 10; Edmund Burke, *Speech on the Conciliation of the Colonies* (Boston: Houghton Mifflin, 1896), pp. 67–68.

[7] *Popular Government* (New York: Holt, 1886), p. 30.

[8] A. Lawrence Lowell, *Public Opinion and Popular Government* (New York: Longmans, Green, 1913), pp. 61–62; Arthur F. Bentley, *The Process of Government* (Evanston, Illinois: Principia Press, 1949 re-issue); Harold D. Lasswell, *Politics: Who Gets What, When, How* (New York: McGraw-Hill, 1936); Dahl and Lindblom, *op. cit.*, Downs, *op. cit.*, and Buchanan and Tullock, *op. cit.*

[9] One of the best behavioral formulations is found in Alfred Kuhn, *The Study of Society* (Homewood, Illinois: Irwin-Dorsey Press, 1963).

into his theory of social behavior as a form of exchange or transaction. Homans examines face-to-face contacts between individuals, viewing them as "an exchange of activity, tangible or intangible, and more or less rewarding or costly" to each individual.[10]

"Activity," according to Homans, is a comprehensively descriptive term referring not only to the transfer of objects and materials from one individual to another but also the emission of "sentiments" by one to the other. One person gives advice in exchange for "sentiments" of deference or affection from the other. Neighbors exchange attentions and favors. Members of a seminar exchange ideas. An activity emitted by one is rewarded or punished by an activity emitted by the other, regardless of the kinds of activity each emits.

Consistent with economic theory, Homans views a "cost" as a value foregone. The person who emits an activity must, by doing so, have foregone a *potentially* rewarding alternative activity; the activity foregone is a "cost." A friend who takes time to give advice instead of pursuing some other potential use of his time is incurring a "cost," that is, the foregone time. In exchange, he may, for example, expect affection or deference. If he receives derision or abuse instead, he is likely to consider further transactions with the other person "unprofitable." Further exchanges are likely to decline in frequency or cease.

In Homans' terms, the more costly an activity is to a person, the less often will he emit it. Psychic profit, therefore, is reward less cost. Again, as in traditional economic theory, no exchange of activity continues unless *both* parties believe they are profiting.

Homans illustrates his theory by drawing upon the findings of Blau's study of interpersonal behavior in a bureaucracy. Sixteen agents of a Federal law-enforcement agency were the subjects observed. These men held the same job title, but they varied in competence. The more skilled received more requests for advice and assistance from their co-workers. The recipients of the advice and assistance benefited, but nonetheless at a cost. The agent requesting help usually was rewarded by being enabled to do a better job. He paid the price of implicitly admitting his inferiority to a colleague who, by title, was supposedly his equal. On the other hand, the consultant gained prestige, at the same time incurring the cost of time taken from his own work.[11]

Each agent—advisee and adviser—netted "social profit" if his perceived reward from the interaction exceeded the personal costs. In other words, "social justice" was realized in this transaction if the rewards of both participants were proportional to their respective costs. The criterion of proportionality rests upon each participant's *own* normative expectation, derived though it may be from what he had learned about the nature of "fairness" and "justice" from his social environment.

Kuhn adds the concept "effective preference." The behavior of *each* party to a transaction must be analyzed in order to understand the combined behaviors of *both*. For this purpose, "effective preference" (EP) is "the degree of capacity to get some desired thing plus the will to get it rather than to satisfy conflicting desires." The EP of an individual is determined not only by his subjective desires, but also by his objective opportunities (availability of the goods, his resources for exchange, his capacity for deciding, etc.)[12]

Assume that person A desires good Y and has the resources to enable him to acquire Y, that is, Y lies with A's opportunity system. Whatever A must give up or forego in order to get Y will be called X; that is, X is the objective cost of good Y as far as A is concerned. The objective cost, however, is a poor basis for predicting whether A will incur X in order to obtain Y. His decision depends on the *relative valuation* that A places on X and Y. Let us, then, label his valuation of X as AX and of Y as AY. If, in A's value or preference system, AY is greater than AX, he will incur the cost X to get Good Y. If AY equals AX, he will be indifferent to the transaction, that is, he is equally likely to exchange as not to exchange. If AY is less than AX, person A will keep X and forget about obtaining good Y. In general, A's effective preference (EP) for Y is AY-AX.

Each transactor has his own subjective yardstick for measuring "profit" or "loss," although his membership in various groups may have some cultural influence upon the "units" he employs. Conceivably, it is possible to design an "Equity Scale" to help measure a decision-maker's own valuation of a transaction. An Equity Scale may be based upon an evaluative scale with polar values such as "very profitable for me" and "very costly to me" and a middle interval for "even exchange" or "fair deal." If what he receives (R) in a transaction is worth exactly as much to him as what he gives (G), the ratio between the two (R/G) equals one, or unity. An Equity Scale score of one could be

[10] *Social Behavior: Its Elementary Forms* (Boston: Harcourt, Brace & World, 1961).

[11] Peter M. Blau, *The Dynamics of Bureaucracy* (Chicago: University of Chicago Press, 1955.)

[12] Kuhn, *op. cit.*, pp. 319–20, Chap. 17 ff.

said to describe "a sense of equity." If R is worth more to him than G, then R/G is greater than one, and he presumably experiences a psychic "sense of profit." On the other hand, if R is worth less to him than G, then R/G is less than one, producing "a sense of loss." Such an Equity Scale as suggested here would be a simple ordinal scale that, in some respects, operationalizes Homan's notion of "distributive justice" and Kuhn's "effective preference." As far as the transactor is concerned, he will probably consummate an exchange if he has a sense of profit but refuse to exchange if he experiences a sense of loss. A sense of equity is likely to produce a 50–50 prospect of transaction, the kind of "satisfaction" that adversaries experience when they are about to compromise on each other's minimum terms. "Half a loaf is better than none" perhaps best describes "sense of equity."

However, a transaction is a decision process involving more than one person. Person B must therefore be added to the basic model. Assume that B possesses good Y and desires X for it. The general statement of B's EP is BX-BY. A transaction requires two decisions: A's decision to give up X for Y, and B's decision to give up Y for X.

The preference orderings of two persons must now be examined simultaneously. It will of course be necessary for both X and Y to be valued *somewhat* by A and B, otherwise neither transactor would be interested in keeping the good presently in his possession nor would he seek the good of the other.[13] On the other hand, for a transaction to ensue, some degree of opposite or divergent preference ordering must exist between A and B. Each must want the other's good at least a little more than his own. The possibilities are as follows:

(1) If A and B both prefer Y to X, then A will be willing to offer X for Y, but B will refuse. No transaction. (2) If both prefer X to Y, B will be willing to give Y for X, but A will refuse. No transaction. (3) If A prefers Y to X, and B prefers X to Y, then an exchange is possible. (4) Both A and B prefer Y over X, that is, have the same preference *ordering*, but A places a greater value, that is, would give up more, on Y than does B. Such a difference in relative valuation, even with the same preference orderings, may be sufficient to permit a transaction to take place.

In a bargainable transaction, each person's power to acquire what he desires depends, therefore, not only on his own but also on the other's EP. Only if there is *some minimal overlap* of transactor EPs can a transaction take place. This means that the parties must know about each other, want each other's goods, have information about the respective EPs. With all this having to transpire, very few social interactions would seem likely to end up as successful transactions. On the other hand, the notion of overlap of EPs helps make explicit the important function of marketplaces in bringing potential transactors together in some convenient and rational manner. If a prospective transactor wants to trade in wheat or bonds, for example, he knows exactly where to find persons with prospectively overlapping EPs, that is, in some appropriate marketplace in the United States.

Trust and Suspicion

Time is an important aspect of transactions.[14] The simplest transactions are those in which two parties transfer goods between themselves simultaneously. When the transfers take place at different times, anticipation and trust become significant elements in the situation. The local grocer will extend non-interest credit to a customer until pay-day, that is, he will exhibit an attitude of trust toward the customer in anticipation of a completion of the transaction at some fixed future time. A more complicated economic transaction occurs with the purchase of a house in which the buyer signs a mortgage contract to pay a price, say, $30,000, plus interest, in specific installments over a period of years. In this case, the mortgage company extends trust at an economically acceptable price (the going interest rate) if its investigations establish the trustworthiness (credit standing) of the buyer.

Social and political exchanges, as Blau points out, have somewhat different trust conditions in that they usually entail unspecified obligations. Business transactors can bargain over price and interest rate, but friends can hardly bargain over the expectation that an invitation to dinner will be reciprocated at some future time.[15] Another difference relates to the ambiguity or the

[13] A may of course be willing to exchange X for B's Y in order to facilitate getting C's Z later, but this is an elaboration of the basic model rather than a new model.

[14] ". . . Evolution of trust goes through phases brought on by a time-dependent cognitive reappraisal of the relationship," in the words of Marc Pilisuk, Paul Skolnick, Kenneth Thomas, and Reuben Chapman, "Boredom vs. Cognitive Reappraisal in the Development of Cooperative Strategy," *Journal of Conflict Resolution*, 9 (March, 1967), p. 116. Cf. processes taking place during "critical transition" in the present theory.

[15] Peter M. Blau, *Exchange and Power in Social Life* (New York: Wiley, 1964).

unquantified character of currency units usually involved in social and political exchanges. In Blau's study of Federal investigators, for example, what "units" can measure (1) the advice given by the more competent investigators and (2) the deference returned by the less competent?

Social and political exchanges, therefore, involve transfers that create diffuse rather than precisely specified future obligations and a return that cannot readily be bargained about but must be left to the discretion of the one who makes it. Since there is no way to assure an appropriate return for a social favor, social exchange particularly requires trusting others to discharge their obligations. Typically, according to Blau, exchange relations evolve as a slow process, usually starting with minor transactions requiring little trust as well as little risk. By discharging such obligations for goods or services received, individuals demonstrate their trustworthiness, leading to gradual expansion of mutually equitable exchanges, in turn leading to further growth of mutual trust. Examples readily come to mind; e.g., the borrower who repays a small loan promptly in order to establish his credit standing for a larger loan.

What if a transactor receiving goods fails to keep his promise to pay or leaves unfulfilled the arranged return of goods? In the usual economic transaction, "foreclosure" is a familiar response by the giving transactor. Non-payment of a mortgage at the scheduled rate leads to a forfeiture of "the stake," namely the house and all previous payments on it. Termination of a credit standing is a kind of foreclosure, that is, loss of the opportunity to possess goods in advance of cash payment. In social and political exchanges, unkept promises and unreturned obligations lead to the loss of such "stakes" as friendship, reputation, peace, honor, credibility, opportunities for affiliation, etc. Transaction relationships may become less trusting, transactors less attractive to each other, and grounds for cooperation or coalition less tenable. If enough suspicion is created, one of the transactors may, usually at some risk, try to take back the goods transferred in the first phase of the exchange. Thus, for political transactions in which transfers of currencies are spread over time rather than completed simultaneously, attitudes of trust or suspicion by the transactors toward each other become operative and influential.

A familiar example from American history may be drawn from the events following the Compromise of 1850. In the Congressional actions based upon the Compromise, the North succeeded in having California admitted as a non-slave state and having slave trade abolished in the District of Columbia. The South won a promise of enactment of stricter Federal statutes dealing with the return of runaway slave property. Although the promise was technically implemented by passage of a Fugitive Slave Act, the spirit of the Act, from the Southern point of view, was undermined by the Northern "underground railroad" for helping slaves escape and by other local interferences with Federal enforcement. The South perceived the latter activities as breach-of-promise and grounds for distrust of Northern intentions. All through the 1850's, of course, event after event broadened the distrust between the two sections.[16]

Organizational Integration

A nation, like any other group or organization, may become politically "integrated," a state of affairs whose parameters at best continue to be difficult to operationalize. Ake, for example, suggests several possible empirical indicators of political integration: (1) the extent to which the citizens of a state identify with the state as an embodiment of their interest and therefore concede that it deserves their loyalty and the authority to exercise certain powers

[16] Political scientists are becoming increasingly aware of the central importance of trust attitudes in political and social relations. Most of the work, however, continues to be done by sociologists and psychologists. The trust-suspicion factor is expounded and experimented with by the following: William A. Gamson, *Power and Discontent* (Homewood, Ill.: Dorsey Press, 1968), Chap. 3; M. Deutsch, "Trust and Suspicion," *Journal of Conflict Resolution,* 2 (December, 1958), 265–279, and "Some Considerations Relevant to National Policy," *Journal of Social Issues,* 17 (1961), 57–68; M. Deutsch and R. M. Krauss, "Studies in Interpersonal Bargaining," *Journal of Conflict Resolution,* 6 (March, 1962), 52–76; Harold Garfinkel, "A conception of, and Experiments with, 'Trust' as a Condition of Stable Concerted Actions," in O. J. Harvey (ed.), *Motivation and Social Interaction* (New York: Ronald Press, 1963), pp. 187–238; E. D. Hoedemaker, "Distrust and Aggression: An Interpersonal-International Analogy," *Journal of Conflict Resolution,* 12 (March, 1968), 69–81; B. Lieberman, "i-Trust: A Notion of Trust in Three-Person Games and International Affairs," *Journal of Conflict Resolution,* 8 (September, 1964), 271–280; Marc Pilisuk *et al.*, "Honesty, Deceit, and Timing in the Display of Intentions," *Behavioral Science,* 12 (May, 1967), 205–215; G. H. Shure and R. J. Meeker, "A Personality/Attitude Schedule for Use in Experimental Studies," *Journal of Psychology,* 65 (1967), 233–252.

over them; (2) the degree of preference political actors have for extraconstitutional rather than constitutional actions; (3) political violence as a special type of extraconstitutional behavior which, if frequent, suggests that the normative political culture is still in an embryonic stage; (4) the number of groups wishing to secede from the political system; (5) the extent to which major political groups, such as political parties, competing for control of the government draw their support from a diversity of regions and groups within the country; (6) the extent to which loyalty of members of the political system is focused on office rather than on charismatic appeal of the holder of an office; and (7) the availability of the state's coercive resources.[17]

Others write of "political integration" as a process involving such factors as: shifting loyalties and expectations; jurisdiction; a participant community; progressive reduction of cultural and regional tensions and discontinuities; and value consensus.[18]

Another conception of political integration could undoubtedly be derived from group and organization theory. According to most students of the subject, a human "group" comes into being when two or more persons engage in a minimum frequency of interaction in response to some shared attitude (or shared characteristic about which they have a common attitude) which produces among them specific uniformities of behavior. An "organization" has been defined as a system of consciously coordinated personal activities or forces that comes into being when (1) there are two or more persons able to communicate with each other (2) who are willing to contribute action (3) to accomplish a common purpose.[19]

All social transactions are a concomitant of group or organizational life. By definition, an exchange must be engaged in by two or more persons or groups. For each person to perceive a prospective exchange as profitable (a necessary condition for consummating the transaction), he, along with the other transactor, must attach some value to the thing or currency being negotiated. Each person's valuation of the currency may be different in degree, but each must to some degree value it. The more valuable a goal to the individual, the more effort he will put into group activities designed to achieve the goal. In transactional terms, a group is "integrated" when most of its members think it more costly to contribute effort and resources elsewhere than to the group. Affiliation and effort by a member continue so long as he thinks it is "worth doing business" with the particular group, according to whatever subjective valuational yardstick he may apply. When the members of a group prefer its goals, remain in communication with other members, contribute effort to implement these goals, and participate in group decision-making, the group may be described as "integrated." Conversely, "disintegration" is characterized by manifestations of goal-dissatisfaction, low morale, membership inertia, non-communication, or disaffiliation of members.

The durability of "group integration" is invariably a probabilistic condition; in the life of human groups, disintegration and demise are always a possibility. However, if most group members experience (a) a sense of profit during most transactions with the group, (b) reinforced attitudes of trust toward group leaders and fellow-members, and (c) increased transactional opportunity at declining subjective cost, then intra-group transactions may become "irrevocable."

The department store "clearance sale" is a familiar commercial example of an "irrevocable" transaction. The merchant interested in disposing of an inventory of goods, at the same time covering all costs, will announce a "clearance sale" in which prices are irreducibly low and all sales are declared "final." The latter requirement reflects the fact that the reduced prices leave him no margin for either risks or clerical overhead associated with returns of goods. On the other hand, buyers, just as interested in minimizing expenditures, will take special pains to examine goods prior to purchases, unless, of

[17] Claude Ake, *A Theory of Political Integration* (Homewood, Ill.: Dorsey Press, 1967), pp. 8–11.

[18] An excellent empirical study of political integration, employing a content analysis of the American colonial press, is Richard L. Merritt's *Symbols of American Community, 1735–1775* (New Haven: Yale University Press, 1966). For an outstanding attempt to cope with the logical and empirical issues of defining "integration," see Johan Galtung, "A Structural Theory of Integration," *Journal of Peace Research* (1968), No. 4, pp. 375–95. According to Galtung, integration is "the process whereby two or more actors form a new actor."

[19] For development of these definitions, David B. Truman, *The Governmental Process* (New York: Knopf, 1951); Chester Barnard, *The Functions of the Executive* (Cambridge: Harvard University Press, 1938); James G. March and Herbert A. Simon, *Organizations* (New York: Wiley, 1958); and Bertram M. Gross, *The Managing of Organizations* (New York: Free Press, 1964).

course, the merchant is a trusted seller in the community. Any attempt to reverse the transaction, that is, take back or return goods, would involve unwanted costs to both transactors. The transaction, in this cost-analysis sense, is then "irrevocable." The greater the number and significance of irrevocable transactions among members of a group, the more integrated the group presumably is.

"Irrevocable transaction," then, is another feature of organizational integration, including the integration of a nation or a world organization. In view of the constant search for "irrevocable" arms control arrangements in international politics, data about irrevocable transactions would undoubtedly be an important part of an *ex post facto* search into the transactional histories of nations.

II. BEHAVIORAL AND POLITICAL CURRENCIES: A TYPOLOGY

What is exchanged in a transaction? The economists, who have made the most extensive use of the "transaction" concept, have had perhaps the easiest task of identifying the objects exchanged, for they have been relatively material, observable, and quantifiable. Human commerce over the centuries has led to a specific identification of things transacted: goods, services, currencies, etc. For social exchanges, Homans added "sentiments" and "activities." For the purposes of the present theory, "transactable things" are called "currencies." A "currency" is any empirically observable object, action, or condition perceived as valuable by a particular set of interacting persons (or groups) and capable of being transferred or transmitted by one to the other in a transaction.

As suggested earlier, all social transactions are a concomitant of group and organizational life. Shared values are an essential element in the formation of human cooperative systems. Groups come into being when two or more persons hold in common at least one attitude about some goal. Each member of the group contributes effort toward the implementation of the goal in direct ratio to the worth he attaches to the goal. Among the principal behaviors generated in groups and organizations are those relating to (1) the performance of tasks intended to implement either the group goal or the group's response to a shared attitude; (2) the accomplishment of collective decisions, and (3) the utilization of material resources. These principal types of behavior, respectively, are theoretically exchangeable as *positional, decisional,* and *materiel* currencies. Thus, the present theory offers a general typology of behavioral and political "currencies" derived from theoretical characteristics of all groups and organizations.

Positional Currencies

To help in their immediate decisions and actions, the members of a group, or their leaders, will analyze the goal into specific tasks whose accomplishment is then delegated to particular individuals. These are "task expectations."[20] Sets of task-expectations comprise the "positions" in an organized group. The more a group believes that a particular set of tasks or skills will be instrumental in moving it to its goal, the more will that group esteem the position and reward its incumbents. A group position or incumbency therein, therefore, may be a valuable good or currency amenable to exchange in social transactions. The job patronage given by party leaders in exchange for party service is a well known example.

When organizations, including governments, are created, positions are also brought into being. Such positions are usually given (a) a name, (b) a set of tasks to perform, (c) conditions of investiture, according to which the group places into incumbency in the position some particular person, and (d) conditions of divestiture, according to which the group may remove an incumbent from a position. Each of these four aspects of *incumbency* may become negotiable and exchangeable sub-types, or "denominations," of currency in political transactions.

(a) *Position name.* If a position is created and named by a group, the naming may be part of a significant transaction. Should the executive head of the new Federal Union (in 1787) be called "king" or "president"? Should the chief executive office of the United Nations be called "president" or "secretary-general"? These naming questions may carry a number of implications for valued political symbolisms. "President" implies a republican government; "king" implies a monarchy; "chairman" suggests a committee form of executive leadership; etc.

(b) *Task specification.* The creation, assignment, and withdrawal of duties or task-expectations associated with an office may be another sub-type of incumbency currency. Task specification requires information and judgments about the following kinds of issues: What are the odds that particular tasks will produce particular events leading to the goal? What member talent is available that is most relevant to an adequate

[20] Defined in this author's "A Theory of Conflict Processes and Organizational Offices," *Journal of Conflict Resolution,* 10 (September, 1966), 331-334.

performance of the tasks expected? How are conflicting estimates of the success of particular instrumental activities—the fight over "means"—to be reconciled with job descriptions? How may the group compensate for inadequate performance by an irremovable incumbent? On each issue, the specification and allocation of tasks may become negotiable currency. For example: "If you let my group (because it knows so much about farming) influence the policies that tell the Secretary of Agriculture what he should be doing, we'll agree that your group (because it knows so much about business) influence what the Secretary of Commerce should be doing."

(c) *Legitimizing conditions.* The relationship between a person and a position when the former "fills" the latter is an "incumbency." When an organization prescribes the legitimizing conditions that signal a conclusive choice of incumbent for a position, the elements of that prescription become negotiable currency. The design of the Electoral College method for electing a president, for example, was the result of transactions in legitimizing conditions.

(d) *Divesting conditions.* To create a vacancy in a position is, in effect, to increase the number of incumbencies available for political transaction. For example, in the heyday of Federal post office patronage (1850–1890), tens of thousands of postmasters customarily resigned or failed to have their appointments renewed each time a different political party captured the Presidency. This made way for appointment of the loyal rank-and-file of the winning party. Sometimes, if divesting conditions are not specified or operable, the creation of a vacancy may become quite costly—as in the case of wars, revolutions, assassinations, or *coups d'état* aimed at removing incumbents from office.

Decisional Currencies

Groups, like individuals, must make choices. A group may delegate, by fiat or silent acquiescence, all of its decisional activity to a single individual, a familiar arrangement in dictatorships. At the opposite pole, a group may require the explicit and unanimous consent of all its members before adopting an alternative. Between these extremes are the countless other collective decision-making procedures invented by mankind, including such familiar types as the Quaker "sense of the meeting" and the "majority rule" voting arrangements of democratic representative assemblies.

Whether the group consists of only two persons, e.g., husband and wife, or millions of persons, e.g., the American presidential electorate, its decision-making is normally a matter of selecting one of two or more perceived and available alternatives of action. What is presumably decided is the action alternative most likely, in the minds of group members, to be instrumental in moving the group toward its goal. The greater a member's weight or *share* in the collective decision process, the more likely will *his* valuation of group goals and *his* interpretation of the best means for achieving those goals prevail. "Power" is generally viewed as a matter of sharing in or influencing collective decisions. It follows, then, that decisional units (*shares* in collective decision) may be a valuable currency.

Invariably, decision-making is an individual act or event. Group or collective decision-making, therefore, requires some "method" for summarizing relevant individual decisions in order to discover and act upon the group's choice of alternatives.[21] As a consequence, any arrangement for distributing or sharing collective decision-making prerogative requires a system of participation that may be analyzed into the following components: *Who* takes *what part* in deciding *what kinds of matters* and on *what occasions*? Each of the underscored components may be considered a sub-type, or denomination, of decisional or *shares* currency.

Politicians are conspicuously concerned about trading with shares of prerogative in the collective decisions of the community. To distribute shares of participation in collective decision-making is to establish an organized structure of power dispersion. This leads politicians to analyze and transact solutions to the question above. Political inventors over the centuries have produced a variety of arrangements dealing with each of the components of this question: (a) the attributes that identify "who"; (b) the units of participation that prescribe "what party"; and (d) the situational conditions that identify "occasions." *Prima facie* evidence of a formally arranged system of participation may usually be found in constitutions, statutes, treaties, and similar documents.

(a) *Personal attributes.* The identification of those who shall participate is a major negotiable shares currency. Historically, participation in collective decision-making was first extended to groups rather than to individuals. For example, the House of Lords and House of Commons, as organized groups in the early English Parliament, negotiated with the king for prerogatives in certain public decisions.

In designating different classes of participants, many political communities often indicate who shall be *excluded* from participation as well

[21] Buchanan and Tullock, *op. cit.*

as who shall be *included*. A partial list of the criteria of eligibility for candidacies or for suffrage privileges shows the range of categories that has developed: citizen status; a minimum age; residence or other geographical restrictions; registered party membership; nomination by a party; educational achievements and literacy skills; real or personal property ownership; membership in one or the other sex; payments of taxes or special fees; religious qualifications; mental or penal disqualifications; race, nationality, or belief requirements; oaths of loyalty; etc.

(b) *Units of participation*. What part a participant shall have in choice-making usually refers to his "weight" in voting units. Shall the vote of each participant weight equally with that of every other? Shall the "say" of certain participants weigh more heavily than others in the distribution of votes? These questions run through the history of voting and apportionment systems. Every plan of apportionment is controversial because some group inescapably gets more currency than others. One standard of equality among voting units may create a bias under some other standard, e.g., the equal representation of small and large states in the U. S. Senate.

(c) *Substantive topics*. Types of issues are another basis for parceling out participation. Usually, certain topics or issues may be included within the scope of decision of certain types of participants acting on behalf of the entire community. The Constitution, for example, has many such specifications: only Congress may declare war; only the House of Representatives may initiate tax measures; only the President may grant pardons.

Sometimes certain issues are *excluded* entirely from the decision prerogative of the collectivity, to be left entirely to the choice of individuals. Bills of rights are the most common place for such constraining specifications. Religious affiliation, for example, is subject only to individual, never collective, decision in the United States.

(d) *Situational conditions*. Kinds of occasions are a fourth denomination in the shares currency. These are certain types of situations, frequently identified in law or procedure, whose existence may trigger the exercise of shares. Specification of such situations may place important limits upon power dispersion. For example, in a parliamentary system, a legislative vote of "no confidence" in the incumbent ministry may trigger a general election, whereupon voter participation becomes operative. In war or national emergency situations, usually carefully described in law, the President is allowed to become a "constitutional dictator," with unusual prerogatives for deciding certain matters.

Materiel Currencies

This is perhaps the most familiar of the types of currencies. In economics it is usually referred to as "merchandise," wares," "commodities," "services," etc. The term "materiel," as employed here, pertains to things of physical matter: iron ore, houses, foodstuff, bombs, clothing, typewriters, etc. In ordinary commercial practice, most materiel currencies are assigned a price in monetary units, so that an automobile may be worth $2,000, a typewriter $200, or a loaf of bread 20 cents. Money has itself become a materiel commodity with fluctuating value; as every loan company knows, money is worth much more just before pay-day than just after.

A substantial portion of human activity is devoted to the acquisition, conversion, and utilization of the resources found in the environment, for example, into food to satisfy hunger, clothes to provide warmth, housing to give protection from the elements, vehicles to help move people and goods about, and so on. Civilization has, of course, elaborated these basic activities with advanced technologies.

Materiel currencies, like other currencies, are valued to the extent that they are perceived as instruments for the achievement of desired goals. Cultural influences are often crucial in establishing the valuableness of materiel; the aborigine is hardly a "ready" audience for televised toothpaste commercials.

An overwhelming proportion of what may be called governmental business is usually conducted with materiel, or *commodity*, currencies; so much so that Karl Marx and other political economists came to believe that economic considerations determine all operations of government. Even less ideological documents, such as party platforms, textbooks on public policy-making, etc., are largely discussions of demands for and allocations of government funds, services, goods, credit, and armaments.

(a) *Government funds*. Government funds are those monies collected into the public treasury through tax receipts and other revenue sources, to be re-allocated by public officials. A legislature may appropriate public funds for highway construction, but a public official must let the contract. For this reason, a political party may expend great resources to put its candidates into public office, for this office may determine which "friendly" contractors shall receive the construction work.

(b) *Government services*. Government services rendered by personnel under the direction of public officers and supported by government funds are frequently used as a barter type of

substitute for materiel or money e.g., the use of the services of military personnel in a disaster area to help rebuild a community. A Federal agency, the Coast Guard, for example, may render valuable service to port communities by providing emergency aid to private craft sailing in these areas for pleasure or business; for port communities this service is as good as money. Some government services are difficult to classify or measure as materiel currency. What, in dollar or materiel units, is an efficient traffic control service worth to the members of a community in "units" of physical safety, hours saved in transit, and orderly flow of daily business?

(c) *Government goods.* Many public agencies are producers of economic goods. There are "government goods" because of some special circumstance under which they are produced, processed, or sold. Government operated or controlled dams produce electricity. Government agencies purchase surplus agricultural products for resale or gift to foreign nations. Prisons are public agencies whose inmates may produce saleable goods.

(d) *Government credit.* A fourth denomination of materiel currencies is government credit. This has become increasingly prominent in American political transactions during recent decades. Low-interest loans to farmers or homeowners are now a well-established form of aid to these important contributers to the Nation's prosperity. Government credit is also extended in the granting of veteran loans and small business loans.

(e) *Armaments.* A somewhat special sub-type of material currency are weapons, or armaments. The dictionary definition of "armament" is a convenient point of departure: "equipment for hostile action." A hammer is a piece of materiel used as equipment in construction, that is, until it happens to be used as a piece of equipment for committing murder. In the latter situation, a "construction tool" is identified as a "murder weapon." Obviously, classification as "weapon" is contingent upon *purposive* and *situational* factors: the intention of the user; the weapon; the moral category into which the purpose is placed by society; perhaps the name assigned in conversation, e.g., "This hammer would make quite a *murder weapon,*" or "This hunting rifle can be used as a *revolutionary weapon.*"

A weapon, therefore, is a physical device intended to destroy, damage, or otherwise disrupt life and property. Such devices become weapons only among persons who (a) recognize the destructive potentiality of the device under circumstances of conflict and (b) place value upon particular lives and property. The aborigine who has never seen a gun can hardly be threatened with it until its effects are demonstrated and become important to him.

One distinctive characteristic of armaments is the significance of their *anticipated* use; that is, a gun may be as important as an unfired threat as it is when fired even though never used in violent exchanges. Also, armaments may *deny* rather than *give* the transactor things of value (life, property), in effect, reduce the negotiable and bargainable currencies available to the other transactor. To "trade shots" is the essence of a transaction involving armament currency.

Some may prefer to call armaments a "negative commodity," that is, materiel manufactured to destroy and be destroyed, in effect, wasted. But this is not entirely an accurate characterization. A tax dollar spent on guns is a tax dollar less for schools, but a railroad built for military transport may also serve as civilian transport. A pilot trained to fly bombers may also be employed by civilian airlines. In this sense, armaments are government commodities, sometimes totally expendable, sometimes convertible to non-destructive uses. It is the latter convertibility that produces the surprising affluence often associated with war-time economies; not all armament spending need be an economic "waste."

III. POLITICAL TRANSACTIONS RELEVANT TO ARMS CONTROL

The typology of general "behavioral currencies" proposed in this paradigm or sketch of a transactional theory, then, consists of three classes: *positional, decisional,* and *materiel.* The "political currencies" identified as counterparts or derivatives of these more comprehensive behavioral currencies consist of *incumbencies* (positional), *shares* (decisional), and *commodities* (materiel). While the definitions of the political currencies fall short of being well-operationalized at this time, the requirement is made that their referents be valuable, observable, and transferable in political transactions.

In an investigation reported elsewhere[22] this typology of political currencies was employed to code the content of political transactions occurring in seventeenth century England and twentieth century Mexico, more specifically, the period 1603–1708 in England and the period 1910–1940 in Mexico. In each case, long periods of recurrent internal war were followed by a cessation of internal wars, national political inte-

[22] See cited Final Technical Report entitled "A Transactional Theory of Political Integration and Arms Control" (November 1968), Office of Naval Research Project NR 177258, Contral Nonr-4722 (00).

gration, and domestic arms control. In the two cases, a common pattern of transacted political currencies was identified.

This pattern suggests the existence of three distinct stages in the development of integrated political communities: (1) an Unintegrated Stage, (2) a Critical Transition, and (3) an Integrated Stage, the latter including arms control arrangements. It was also found important to separate out the armament from the other denominations (sub-types) of commodities currency in order to reveal more clearly the trend concluding in arms control. Thus, the frequencies of four, rather than three, currency types were observed. The four included incumbencies, shares, non-armament commodities, and armament commodities.

In the two cases, the Unintegrated Stage saw a high frequency of *armament* exchanges and very few transacted *incumbencies*. This seemed to reflect the recurrence of internal wars, usually wars about who should sit on the throne or in the presidency; that is, incumbencies as such were not traded, but instead captured by force of arms. In contrast, the Integrated Stage was marked by highly frequent transactions in *incumbencies* and eventually none in *armaments*, the consequence, undoubtedly, of arrangements making *incumbencies* more available for political "deals" in the national marketplace.

The pattern through the three stages shows an over-all rise in transactions involving *shares* currencies. This rise goes up sharply during the Critical Transition, suggesting that *transactions in shares (decisional) currencies are the most important taking place during the Critical Transition*. This would seem to reflect intensive efforts to disperse, share, or otherwise re-arrange the collective decision prerogatives within the national organization. The pattern also suggests that, if shares transacted during the Critical Transition prove to be unprofitable to the major transactors, transactional activity reverts or "loops back" to the frequencies typifying the Unintegrated Stage. In the English and Mexican cases, the deals turned out to be "profitable," and the Critical Transition was successfully navigated into the Integrated Stage. Thus, in passing through the Critical Transition to national integration, *a rising frequency in shares transactions is associated with a sharp decline in violent exchanges of armament commodities.*

Finally, the Integrated Stage experiences a rising rate of incumbency and non-armament commodity transactions. This seems to reflect a regularization of non-violent means for gaining office, the practice of only temporarily controlling the government, as well as growth in relative numbers of governmental positions available for trading. The rising non-armament commodity transactions are, of course, a familiar indicator of peace. Prosperity tends to be growth of economic activity and resources under conditions of peace. This aspect of the transactional pattern may be seen as further confirmation of the proposition that, contrary to Marxist assumptions, peace, not war, is "good for business."

Impressionistic evidence derived from the two case studies indicates another factor appearing to correlate with the integrating pattern, namely, a trust-suspicion factor. Even though transactors reach a level of frequent transaction of shares currencies, previous violent armament exchanges between them are not likely to be conducive to attitudes of trust toward each other. In fact, the contrary has the greater probability; suspicion is the more probable attitude under the usual historical circumstances. *Creation of an environment of transactor trust appears to be the most important consequence of shares exchanges during the Critical Transition period.* Once trust is engendered, transactional activity involving non-armament currencies seems also to be facilitated.

Put another way, observation of transactional histories seems to isolate specific exchange conditions under which attitudes of trust or suspicion may be generated. Conditions leading to trust between transactors may facilitate the surmounting of bargaining hurdles during a Critical Transition period in national political integration. Conditions leading to suspicion, on the other hand, may heighten the negotiating and transactional hurdles to an insurmountable degree, returning the transactors to disintegrative patterns, particularly to armaments exchanges.

These findings may also be interpreted as describing part of the dynamics of evolving representative institutions, suggesting a theory of the development of systems of representation. Representation presumably disperses power (decisional currency) so as to assure comprehensive participation in collective decisions, which in turn promotes trust among transactors, commitment to the political organization, and reduction in the perceived need for armament expenditures. As Charles A. Beard and John D. Lewis note in their survey of "Representative Government in Evolution":

> . . . During the thousands of years which we call antiquity, monarchies, despotisms, dictatorships, tyrannies, democracies, and aristocracies, with numerous variants, rose, flourished, and fell; but nowhere, at no time, did representative institutions appear, at least on any impressive scale. . . .

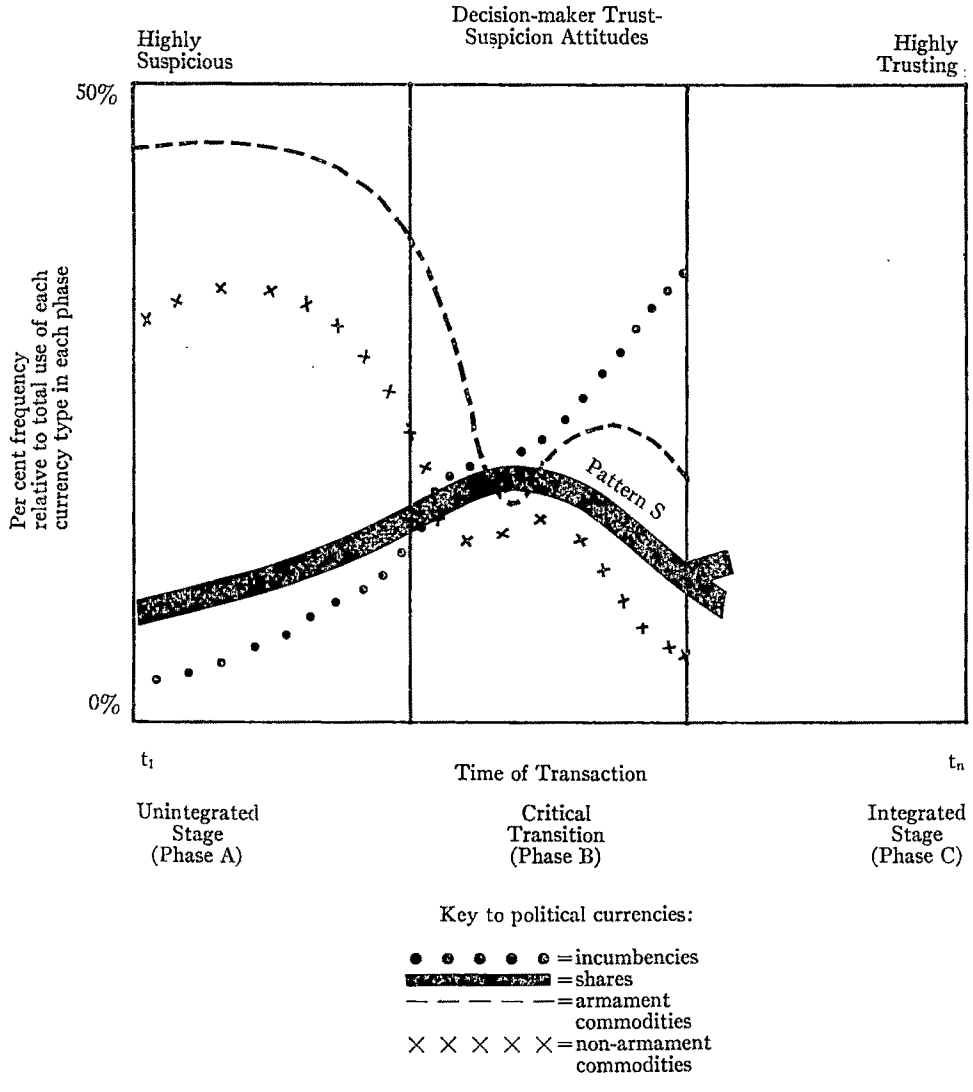


FIGURE 1. Political Currencies Transacted Over Time, Showing Hypothesized Suspicion-inducing Pattern (Pattern S).

The starting point of representative government on a national scale, then, is the Middle Ages in time, and the place is Europe. . . . The monarchs who first called representatives of communities or estates [shares] to grant money [commodities] were not thinking of democracy; they were concerned primarily with the conservation of the peace, the administration of lucrative justice, and the replenishment of their royal treasurier. . . .[23]

Insights provided by the English and Mexican case studies, therefore, suggest testing a relatively ancient political proposition: Dispersed and shared power (decisional currency) is a major contingent factor in promoting attitudes of trust associated with cessations in the use of means of violence (armament commodities) in political transactions. In such a test, the consummated political transactions would be observed, the content of these transactions would be classified as political currencies, that is, as incumbencies, shares, or commodities. The specific major hypotheses would probably be:

a. Rises in frequency of transacted shares (decisional) currencies are positively correlated with attitudinal shifts from suspicion to trust among political transactors.

b. Transactional sequences that follow the

[23] This REVIEW, 26 (April, 1932), 223–240.

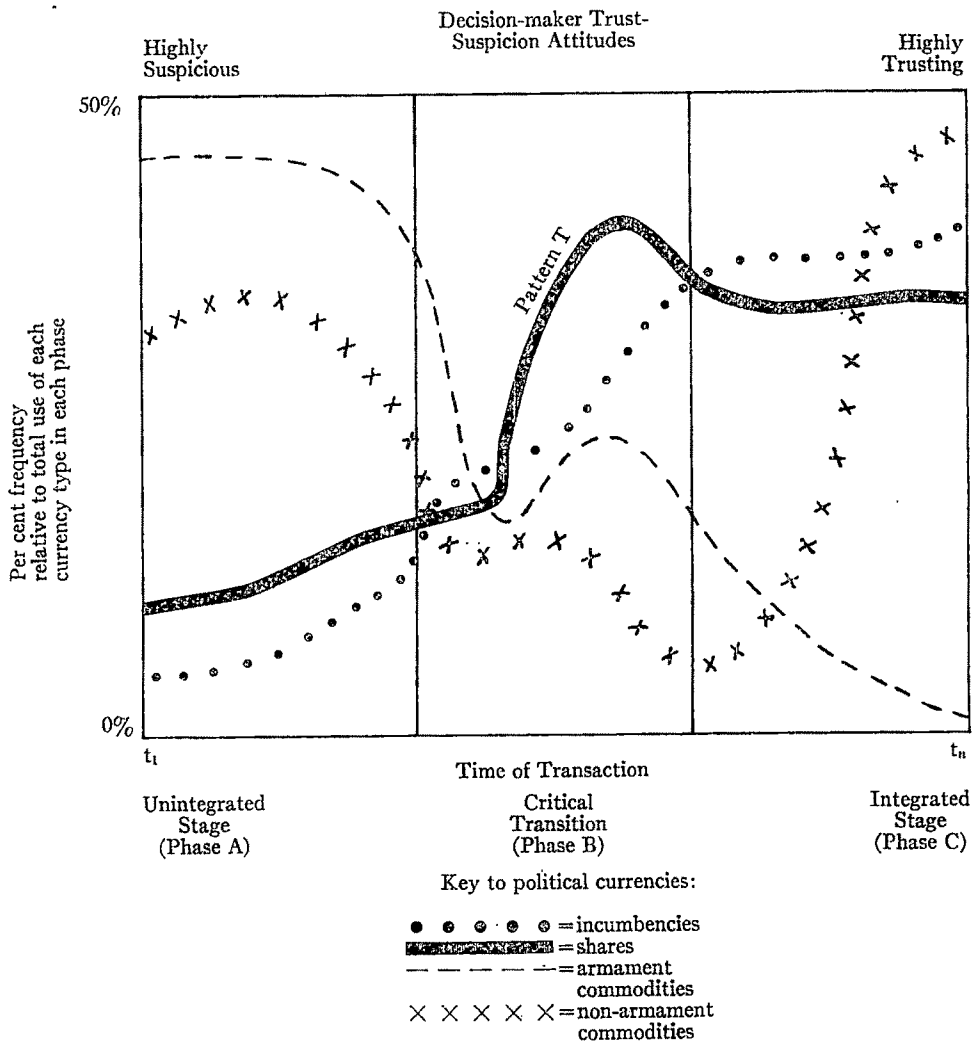


FIGURE 2. Political Currencies Transacted Over Time, Showing Hypothesized Trust-inducing Pattern (Pattern T)

trust-inducing Pattern T (see Figure 2) tend to be associated with successful transitions from unintegrated to integrated stages of political organization.

The transactional time-series that would test the hypothesis are set forth, hypothetically, in Figures 1 and 2.[24]

Briefly, the political currencies transacted during Phase A (Unintegrated Stage) are for the most part armaments. Phase B (Critical Transition) witnesses a rise in armaments transactions, a drop in non-armament commodity transactions (e.g., due to blockades, boycotts, etc.), some increase in incumbency transactions, and *either* Pattern T (sharp rise) *or* Pattern S (decline) in shares transactions. If there is a significant rise in the relative frequency of transactions involving shares currencies (Pattern T), this, hypothetically, will be accompanied by a rise in trusting attitudes and a successful transition to Phase C (Integrated Stage). However, if there is either no rise or a decline in shares transactions (Pattern S), then attitudes of suspicion remain, as in Phase A (as

[24] The mathematical and logical problems of measurement in such a time-series may be considered in the light of the excellent discussion by James S. Coleman, "The Mathematical Study of Change," in Hubert M. Blalock, Jr., and Ann B. Blalock (eds.), *Methodology in Social Research* (New York: McGraw-Hill, 1968), Chap. 11.

the leftward arrow at the end of Pattern S in Figure 1 suggests), and transactional activity returns to the Phase A state.

The theory sketch proposed here requires further formalization, particularly with respect to the operational definition of its principal variables. The predictive statements offered need to be submitted to more rigorous empirical tests than provided in the two cases studies employed for exploratory purposes thus far. Further *ex post facto* historical investigation and simulation experiments seem to hold greatest promise in the design of such tests, and are discussed elsewhere.[25] Either approach has important empirical limitations, yet both types afford challenging opportunities for political scientists wishing to test theoretical generalizations in "the laboratory of history."[26]

If the major hypotheses of this transactional approach are experimentally confirmed, the practical implications may be substantial in a variety of political circumstances, including those involving the design of strategies for advancing international arms control, advancing the political integration of new nations, and even settling violent outbreaks in contemporary urban communities. The strategies would presumably endeavor to promote trust-inducing Pattern T political transactions.

The military capacity to wage war successfully has traditionally been considered an instrument of foreign policy. This view is reflected in Clausewitz's classic definition of war as the continuation of diplomacy by other means. The same view also may be found in most contemporary approaches to "deterrence" of armed aggression through superiority of military forces. Yet, knowledge about the conditions and dynamics of *effective* deterrence continues to be more a matter of conventional wisdom than of validated theory.[27] How "hard" is the political knowledge, after all, that would provide answers to such transactionally stated questions as the following: What are the valuational yardsticks by which particular political transactors judge armament exchanges as "too costly" and transactions in other negotiable political currencies "profitable"? By what series of transactions may adversary decision-makers acquire attitudes of trust sufficient to facilitate arrangements for arms control and political integration? How may changes in valuation yardsticks, political "market-place" arrangements, and political currency denominations raise the probability that world integrating transactions may remain "irrevocable"?

[25] Final Technical Report, ONR Project NR 177258, *op. cit.*

[26] For further pertinent comment, Ole R. Holsti and Robert C. North, "History as a 'Laboratory of Conflict'," in Elton B. McNeil (ed.), *Social Science and Human Conflict* (Englewood Cliffs, N.J.: Prentice-Hall, 1965).

[27] Milton J. Rosenberg, in James N. Rosenau (ed.), *Domestic Sources of Foreign Policy* (New York: Free Press, 1967), for example, attempts to build new ways to reduce distrust between the United States and the Soviet Union from approaches suggested by validated psychological theories. One suggestion is to rely upon systematic "cross-trading," wherein the advantage in one interest conflict is given to one side, while the advantage in another conflict is given to the other side. Thus, adversary leaders may be conditioned away from the view that their relationship is an extended zero-sum game, and attitudes of distrust toward one another may be modified by overt transactional successes.

TOWARD A RATIONAL THEORY OF DECENTRALIZATION: SOME IMPLICATIONS OF A MATHEMATICAL APPROACH

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This paper seeks to open for exploration the field of decentralization in politics and organizational design. As a first approach, it examines conditions under which decentralization is preferable from the viewpoint of rationality or cost-effectiveness. Our normative statements as to what would be best, or what should be done, are formulated first from the viewpoint of the subjects or clients, but they are expected to include the interest of the community in ensuring adequate service at low cost, and they also include the interest of the rulers, insofar as their power in the long run depends on their capacity to respond to the demands made upon them quickly enough and adequately enough to retain their political support.

The political theory underlying our study assumes that modern governments retain "their just powers by the consent of the governed," and hence that both their legitimacy and their power will depend at least in significant part on their ability to respond adequately to the popular demands made upon them. We do not deal in this study with other important criteria of preference, such as the psychological value which some of those who take the role of powerholders may put upon centralized control, or the contrary value which some of those who identify with their subjects may put upon power sharing and decentralization. We do hope to suggest, however, which side to the centralization-decentralization controversy is likely to be favored by the pressure of cost-effectiveness considerations, and under what conditions.

At this stage, we also assume ethnic, linguistic, cultural and social uniformity, as the conditions most likely to favor centralization. According to traditional theory, at least, strong centralization under non-uniform conditions usually would be more difficult.

We consider a situation where several identical and equally spaced facilities for decision making, information processing, or other operations perform services in response to requests from sources that are uniformly distributed in time and across varying distances. Such distances could be measured in terms of intervening decision points, switching points, waiting lines, or simple geographic space. As the simplest first approximation, we have analyzed this last type—service through communication and/or transport along a long thin spatial strip.

In the simplest models, the focus is on *costs*: the number of duplicated facilities is determined by setting the marginal increase in fixed costs equal to the marginal decrease in the costs of communication and transportation due to decentralization. In later models, the utility of *time* gained by more rapid service in response to requests is taken into account, and attention is given to the more dependable availability of service that comes with decentralization involving duplication of facilities. Throughout all these models the total *volume* of requests, and its ratio to the speed of communication and transport, turn out to be crucial.

As far as they go, the results of the present analysis suggest that the social, political and economic changes, which influence the rising volume of requests, may outweigh in many cases the effects of the technological changes that have speeded up communication and transport, and that societies which adopt the technological changes are likely to be societies that increase also—or even more—the volume of requests for service or responses. Where these conditions apply, current and future social, political and technical developments may increase, rather than decrease, conditions favoring decentralization.

More extended mathematical derivations and discussions are presented elsewhere.[1]

The present paper is the first of a projected series which is to investigate problems of centralization and decentralization by means of a series of models intended to be increasingly close to the actual variables and problems encountered in the practice of governments and other organizations. Problems of uneven distributions of messages or service requests, in space and time, questions of hierarchies among service or decision centers, and questions of lateral communications or interference among multiple facilities among several centers at the same level, will have to be investigated in such later papers.

[1] M. Kochen and K. W. Deutsch, "A Mathematical Model for a Study of Political Decentralization," submitted to *Operations Research*, September 1968.

I. DECENTRALIZATION AS A GENERAL PROBLEM

When the director of a research institute in which there are several independent investigators plans to provide computer-based information services, a choice presents itself. Should he provide each independent investigator with his own small computer? Should he install a large computer system everyone must share? Should he provide some mix of these two extremes?

When an architect plans a low-income housing project on a fixed budget, he faces a somewhat analogous choice. Should he provide one bathroom for each family but no swimming pool plus bathhouse for the project (assuming that the budget does not stretch to cover both) or should he provide 50% fewer bathrooms than families, thus making every two families share one, but providing a swimming pool plus bathhouse for use by all as well?

A university library system may comprise over a dozen branches plus departmental and special libraries. In planning the construction of new buildings and ordering library materials, should the university librarian favor the creation of branches and special libraries wherever they are wanted, supplying each with duplicate copies and each with a separate bibliographic control system? Or should he favor the creation of a central custodial unit where responsibility for bibliographic control is concentrated?

A school system may concentrate most of the effective control in the hands of a single board of education for an entire metropolitan area with several million inhabitants, or it may decentralize much or most of this control down to about three dozen neighborhood school boards, each serving a community of between 100,000 and 200,000 people. Apart from group demands and local power contests, which arrangement would be more rational? In what respects and on what grounds?

To what extent should the commander-in-chief delegate various battlefield responsibilities to local commanders and platoon sergeants? What are the consequences of too much or too little autonomy for the junior managers and foremen of a 100,000-employee industrial organization, as distinct from rigid control by a strong chief executive?

Are the customers of an industry better served by a single firm, or a single decision-making unit within a firm, or by several firms or several decision-making units?

Is the population of a territory best served by a single nation-state or government or a single administrative center within a nation, or by several states, governments, or administrative centers?

All these questions have a common core. It is the concept of the "degree of decentralization" of an organization or institution and its relation to the "performance" of the latter. The performance of every human organization includes crucially its *responses* to information received from its environment. Service organization, such as governmental housing or other public or private service agencies, health services, libraries, or computing centers, often appear passive in that they wait for requests from their potential clients or users, to which they then respond. Other public or private agencies appear more active in that they often initiate a sequence or transaction between themselves and some members of the public. Thus tax collection agencies, draft boards, truant officers, public health departments, and private sales organizations often take some initiative in this sense; and some organizations, such as legislatures, general administrative agencies, and police departments play a mixed role, initiating some transactions and also responding to messages from the outside.

Human organizations of all kinds, however, depend critically on the *feedback* of information from their environment in order to discover the results of their actions and to correct their errors.[2] It is in regard to this exchange of information, and often also of things and persons, with their environment, that the performance of more or less centralized or decentralized organizations is to be compared here, in a series of preliminary steps toward a theory of decentralization.

The approach proposed here would go somewhat deeper, therefore, than the familiar view that centralization is a matter of power—that the strong tend to centralize in their own hands as much power as they can, and concede to the weak only as much decentralization as they must. It is quite true that a powerful person, group or office often can concentrate a great many flows of information and of decision making responsibilities in their own hands, but it does not necessarily follow that they will be able to use this information effectively to produce timely and adequate responses.

The relation between power and communi-

[2] On the concept of feedback, see Norbert Wiener, *The Human Use of Human Beings*, (New York: Avon Books, 1967), ch. 1; K. W. Deutsch, *The Nerves of Government*, (2nd ed.; New York: Free Press, 1966), with references; and Robert A. Rosenthal and Robert S. Weiss, "Problems of Organizational Feedback Processes," in *Social Indicators*, Raymond A. Bauer (ed.), (Cambridge: M.I.T. Press, 1966), pp. 302–340.

cation is thus not a one-way process. Power can command communication, but communication and information-processing capacity may increase power or reduce it to the point of paralysis. Where power originally produced centralization, delays or failures in information-processing or responsiveness may then tend to shift both loyalties and power away from the centralized but unresponsive offices or places of command. The operational effectiveness of centralized or decentralized information-processing or decision-making centers, governmental agencies, business organizations and service facilities, is thus in each case a question intellectually distinct from the question of power, and worth considering in its own right.

Much of the well-known political and social theory on centralization and decentralization implies that centralization is preferable on the grounds of efficiency, and that modern technology, with its jet aircraft and electronic communication, has increased the pressures toward centralization.[3] Some theories also assert, however, that decentralization is better suited to protect the values of democracy, liberty, and popular participation in decision-making.[4] Some theorists point our that these different views balance to some extent, and that the optimal point on some more or less continuous centralization-decentralization scale must be discovered in each case; but they give little further indication as to how these discoveries are to be made.

For a start, we shall try to make explicit a few of the basic notions involved, leaving other aspects of the problem for later work. Thus we shall start from the feedbacks and transactions with the outside world, postponing for later analysis elsewhere the internal decision processes of organizations and their possible overloads which play such a large role in much of current organization theory.[5]

We stress in this paper the concern of the person with primary responsibility for the institutional organization rather than the concerns of the members of that organization. Further, we stress what he *should* be concerned with in terms of purposive rationality, rather than what may actually concern him most for personal, psychological, or sociological reasons. By "what he should be concerned with" we mean norms, related to the basic functions and the survival of the institution for which he is responsible; these norms are determined environmentally in terms of feedback processes that are decisive about the survival of the institution. Not that these neglected subjective aspects are unimportant. Far from it. They may well be the salient features of a viable theory of decentralization. So much more reason to treat them thoroughly, but separately, at a later stage.

There is surprisingly little published literature on which to base a formal explication of the decentralization concept.[6] Some of the

[3] See, e.g., Herbert A. Simon, Donald W. Smithburg, and Victor A. Thompson, *Public Administration* (New York: Knopf, 1956), pp. 272–279. For a related argument in international relations, see Albert Wohlstetter, "Illusions of Distance," *Foreign Affairs*, 46 (January 1968), p. 250.

[4] A qualified emphasis on decentralization is found in Joseph A. Litterer, *The Analysis of Organizations* (New York: John Wiley & Sons, 1965), pp. 378–393; and John M. Pfiffner and Frank P. Sherwood, *Administrative Organizations*, (Englewood Cliffs, N.J.: Prentice-Hall, 1960), pp. 440–462.

[5] For some important general approaches, see Herbert Kaufman, "Organization Theory and Political Theory," this REVIEW, 58 (1964), 5–14, esp. 7–8, 13–14; Mason Haire, *Organization Theory in Industrial Practice* (New York: John Wiley & Sons, Inc., 1962), pp. 6–7; Albert H. Rubenstein and Chadwick J. Haberstroh, *Some Theories of Organization* (Homewood, Illinois: Irwin and Dorsey, 1966).

[6] For examples of discussions of the political and social aspects of decentralization and the size of political units or areas, see Bernard H. Baum, *Decentralization in a Democracy*, (Englewood Cliffs, N.J.: Prentice Hall, 1961); Robert A. Dahl, "The City in the Future of Democracy," this REVIEW, 61 (1967), 953–970; James W. Fesler, *Area and Administration* (University, Ala.: University of Alabama Press, 1942); S. W. Hess, *et. al.*, "Non-Partisan Political Redistricting by Computer," *Operations Research*, 13 (1965), 998–1006; Philip Jacob and James V. Toscano (eds.), *The Integration of Political Communities*, (Philadelphia: Lippincott, 1964); Arthur W. Macmahon, *Delegation and Autonomy* (New York: Asia Publications and Taplinger, 1967); E.A.G. Robinson (ed.), *Economic Consequences of the Size of Nations* (New York: St. Martins Press, 1963). General studies of decentralization are relatively rare in political science. *The Cumulative Index to the American Political Science Review* (1906–1963) (ed.), (Evanston, Ill: Northwestern University Press, 1964) lists only one article for the 57 years covered, S. G. Lowrie's "Centralization versus Decentralization (Federalism)", which appeared in the REVIEW in 1922. During the same 57 years, three articles referring to their titles to the delegation of legislative powers appeared in 1908, 1926, and 1947, respectively.

operations research literature dealing with warehouse allocation problems[7] is relevant, as is some of the applied mathematical statistics literature on problems for determining the optimum number of machines or service facilities.[8] Generally, however, customers are assumed to arrive at the service facility, and the time for requests to pass from the customer's site to the facility and for responses to pass back to the customer is not taken into account. The conventional "warehouse-location" problem is to locate a specified number of warehouses and assign customers to each warehouse so that the sum of the squared distances between each customer and the warehouse closest to him is minimized. Actually, distance may not be best represented by its second power;[9] and the present paper will treat the best number of warehouses not as given but as to be computed. It is primarily in the analysis of information systems that such problems are encountered, and relatively little analytical study has been devoted to these.

In this paper we analyze the simplest of these mathematical problems which are, nonetheless, quite generic to a large class of situations in which centralization vs. decentralization is an issue.

II. MOVING PEOPLE, THINGS AND INFORMATION: THE LOGISTICS OF ORGANIZATION

Decentralization depends in large part on logistics, and all social organizations have problems of logistics. All of them must move messages from one place to another; many of them also must move people, such as field representatives, investigators, fire truck crews, riot squads, case workers or other agents of various kinds; and many must also move material objects, ranging from small samples or mail order items to bulky goods in mass production. Yet some organizations mostly depend in their functioning on the inflow and outflow of messages, as do telephone exchanges, while others depend most critically on the movement of material goods, as do factories. Still other organizations depend primarily on receiving information, such as orders or requests, and on responding to these by moving material objects or people, as do mail order houses and at higher rates of speed also fire departments and ambulance services.

The agencies of government and politics overlap these various types. Public school systems depend upon the movement both of people and of information. The long-run recruitment and maintenance of an army depends upon the movement of persons and supplies, but the short-run intervention of some of its forces may occur in response to an order or message. A law court requires the physical presence of the parties or their representatives, and a prison insists on the physical presence of its inmates. Administrative decisions, on the other hand, often are sent out as control signals or messages in response to information that was received at some earlier stage. Legislation consists from this point of view in a special class of messages issued in response not only to earlier messages but also in response to the physical movement of various spokesmen for interest groups at formal hearings, or more informally of lobbyists, and to the bodily movement of legislators and constituents between their respective constituencies and the capital.

A simple overview of such organizations classified into basic logistic types is shown in Table 1.

[7] See Hess, *et. al., op. cit.*; W. J. Baumol and P. Wolfe, "A Warehouse Location Problem," *Operations Research*, 6 (1958), 252–263; L. Hurwicz, *Decentralized Resource Allocation*, Cowles Commission Discussion Paper No. 2112, 1955; T. C. Koopmans and M. J. Beckmann, "Assignment Problems and the Location of Economic Activities," *Econometrica*, 25 (1957), 53–76; A. A. Kuehn and M. J. Hamburger, "A Heuristic Program for Locating Warehouses," *Management Science*, 9 (1963), 643–666; Alan S. Manne, "Plant Location under Economies-of-Scale—Decentralization and Computation," *Management Science*, 11 (1964), 213–235.

[8] J. P. Anderson, *et. al.*, "D825—A Multiple Computer System for Command and Control," in *AFIPS Conference Proceedings*, Volume 22, (Washington, DC: Spartan Books, 1962), pp. 86–96; K. J. Arrow and L. Hurwicz, "Decentralization and Computation in Resource Allocation," in R. W. Pfouts (ed.), *Essays in Economics and Econometrics* (Chapel Hill: University of North Carolina Press, 1960), pp. 34–104; Information Dynamics Corporation, *A Methodology for the Analysis of Information Systems*, Final Report to the National Science Foundation, NSF-C-370, (1965); J. R. Jackson, "Networks of Waiting Lines," *Management Sciences Research Project Research Report No. 53* (Los Angeles: University of California, mimeographed, 1957); Z. S. Wurtele, "A Criterion for Determining the Number of Identical Machines to be Installed at a Machine Center," *Management Sciences Research Project Discussion Paper No. 78* (Los Angeles: University of California, mimeographed, 1961); Edgar M. Hoover, *The Location of Economic Activity*, (New York: McGraw Hill, 1948).

[9] K. W. Deutsch and W. Isard, "A Note on a Generalized Concept of Distance," *Behavioral Science*, 6 (1961), 308–311.

TABLE 1. SOME EXAMPLES OF LOGISTIC TYPES OF ORGANIZATION

		OUTGOING	
		Matter	Information
INCOMING	Matter*	Army Law Court Prison Repair Shop Hospital Medical Examination Factory or Plant Legitimate Theater	Quality Control Stations Analytical Laboratory Movie Studio Live Television Museum, Gallery, Archives
INCOMING	Information	Military Task Force Police Fire Department Ambulance Relief Supplies Mail Order House	Decision-making and Control Centers (governmental and private) Telephone Switching Nets Data Centers Reference Services Computer Facilities

* Material objects and/or human bodies

Each entry in any cell in Table 1 refers to either a task to be performed ("medical examination and treatment") or to an institution which performs some task or tasks (e.g., "army," "hospital"). In some cases, such a task is performed only by relatively large institutions, while other tasks can be decentralized in their performance to the level of individual clients. Government and defense usually are concentrated in such larger institutions, but some physicians still visit patients in their homes. Even governments and armies, however, can decentralize their functions to a greater or lesser degree.

For a functional theory of decentralization, toward which the present paper is directed, the social tasks to be performed are more important than the historical institutions which currently happen to perform them. Though institutions have a great deal of persistence and tend to protect their own survival, they nonetheless do change. Some of these changes tend toward greater or lesser decentralization, and a functional analysis of the level of decentralization most appropriate to their chief task is essential in order to evaluate rationally the actual changes which occur in response to technological or political pressures.

We emphasize the distinction between the movement and storage of persons and materials on the one hand, and the transmission and storage of information on the other hand as most important from the viewpoint of logistics. Messages move more quickly by several orders of magnitude than do persons and materials, and unlike persons and materials they can be duplicated at low cost. All information, of course, depends on some material carrier or marker which bears it, much as a book depends on printer's ink and paper, but the ratio of amount of information to mass of carrier, say in bits per pound per hour, is very high and on a computer display terminal (e.g., teletypewriter or display screen), it is still higher. Our analysis will show that transmission speed is often critical for determining the optimum level of decentralization of a network of facilities and we may expect, therefore, to get different answers for systems depending primarily upon transport as against those depending primarily upon communication.

A simple exercise may illustrate these points. Here we idealize a special but prototypical problem to such an extent that we can analyze it mathematically. Our prototype model will deal with the supply of services from one or several storage facilities. This is not irrelevant. For politics and administration, government, administrative agencies, and law courts all can be thought of as storage places for memories, recorded in their files or carried in the minds of their specialized personnel, and as suppliers of decisions to their clients or constituents. The mathematical analysis of this simplified model may give us insights that a purely verbal analysis does not provide. Building on such a more precise and explicit foundation, we hope then to make some progress toward an edifice

of theory that explicates the core problems and proves helpful in explaining the success of various organizations and possibly also in advising decision-makers.

III. A PROTOTYPE MODEL

Imagine an institution existing to supply objects of value, e.g., books or mail-order items, to customers located in an elongated region from Boston to San Francisco in which the north-south dimension is treated as negligible.

Let D be the east-west distance of this region in miles or in terms of relay stations, traffic intersections, intervening opportunities or other conditions affecting the probability and costs of transmission or transport (Definition 1). Let us make the further assumption that the demand for these valuable objects is spread uniformly over the length of this strip. For example, suppose that ten requests per month originate from each mile along the strip.

Let L denote the total load, in requests per month (Definition 2). The total load, L, resulting from this demand requiring service, then would be $L=10D$ requests per month. Assume that we can control the way the institution is organized, and we wish to explore the consequences of various degrees of decentralization.

By the most centralized organization we mean a single service facility, say a warehouse, which maintains a large enough stock of the valuable objects to clear the demand at all times. (At this first stage of the analysis we assume that the time to forward and process requests is negligible.) It is obvious that the best location for this warehouse is in the middle of our stretched-out region, as shown in Figure 1.

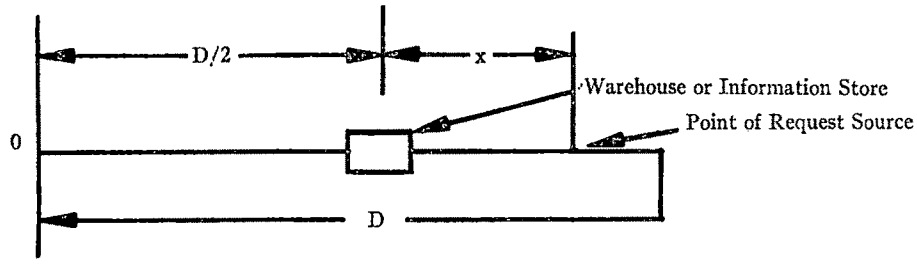


FIG. 1. A Centralized Organization

The manager of the organization is uncertain about the exact distance x between the service facility and the site from which a request (for an object) originates at any moment. When such a request is made, the manager must find the wanted object in the warehouse and transport it to that location.

We assume having a transportation medium which transports at a uniform speed of v miles per hour (Definition 3).

Then the delivery time to a point at a distance of x from the warehouse is x/v. Because the sites of requests are assumed to be uniformly distributed over the length D, the average distance between the site of a randomly selected request and the warehouse at the midpoint is D/4. Hence, the average delivery time for a request is D/4v.

By a slightly less centralized organization we mean two warehouses, with identical inventories (each containing a copy of each valuable object), located at the quarter points of our elongated region (see Figure 2).

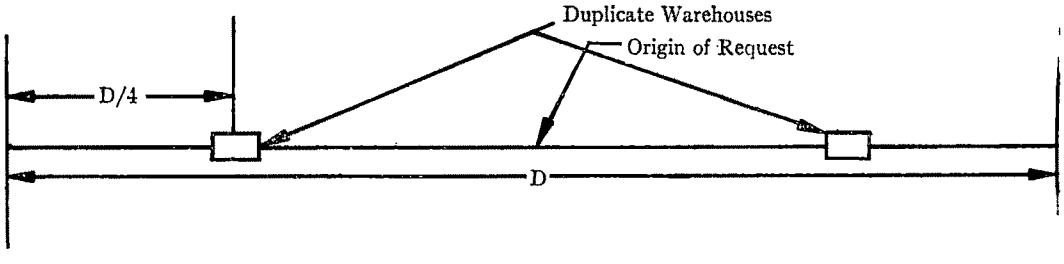


FIG. 2. A Two-Facility Organization

A request originating at any point is serviced by the warehouse nearest to it; requests from points equidistant from two facilities are serviced at random by either.

While for the single facility shown in Figure 1, the delivery time, averaged over many randomly originating requests, was D/4v, it is half of that, or D/8v, for the two facilities shown in Figure 2.

Let the dollar costs of operating a warehouse be \$C per month (Definition 4).

Then the fixed cost of operating the single repository organization of Figure 1 is C (including interest and amortization on its cost of installation) while that of the duplicated service in Figure 1 is 2C.

Let the dollar cost of delivering objects be c dollars per hour (Definition 5).

Then the total demand is L requests per month (with L/2 for each of the two warehouses in the organization of Figure 2), and the average delivery cost is cDL/4v dollars per month for Figure 1 and cDL/8v for Figure 2.

If we used three warehouses, we would arrange them as shown in Figure 3.

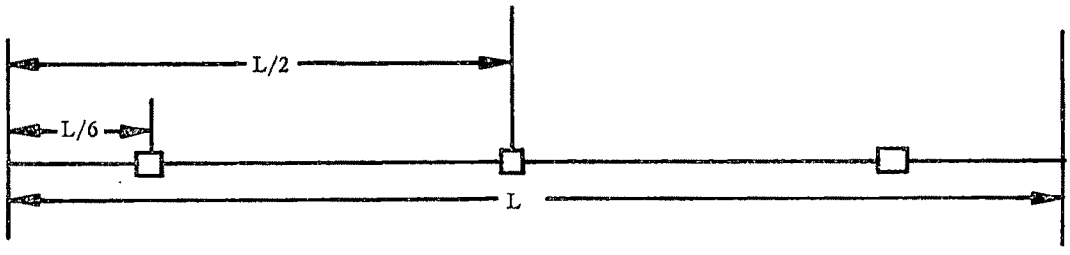


FIG. 3

The most primitive index of decentralization is n, the number of duplicate warehouses (Definition 6).

We would, in the general case, place the left-most warehouse at distance $D/2n$ miles from the left, and space $n-1$ warehouses D/n miles apart, the right-most warehouse being $D/2n$ miles from the right end. Each warehouse meets requests issuing from a distance of D/n or fewer miles to its right or left. Since a request, on the average, should originate $\frac{1}{2}$ $(D/2n)$ miles to the right or left of the warehouse, the average transport distance is $D/4n$. The average delivery time (in hours) is the average distance divided by transport speed (miles per hour) or $D/4vn$.

This first approximation neglects the internal cost of processing and servicing each request within each facility. It also neglects congestion effects and instead assumes *unobstructed* channels of transport and communication. That is, it assumes that the input load L, in requests per unit of time, has no effect on the transmission speed v, and that it has no effect on the throughput rate of the system.

The maximum throughput rate of the system is defined as its capacity, called here M for objects and B for information, respectively (Definition 7).

If the system were partially obstructed, the speed v of transport or transmission would decline with rising input loads; if the system were *overloaded*, its throughput rate would decline with increasing inputs, and it would fall increasingly below the system's capacities M or B. In reality, there often is an extended range of traffic loads L over which the speed v remains substantially unchanged. In the present prototype example, however, we are assuming that this range extends all the way to the maximum capacity point and beyond it, so that v is treated as independent from L at all times. More realistic congestion effects and other complicating possibilities must be left for later treatment.

Suppose time were of no value to users, and only costs mattered to them. Then it would pay to increase the number, n, of facilities until the marginal increase in fixed costs is equal to the marginal decrease in delivery costs.

Let C_n be the cost of operating an organization with n warehouses. Let us assume that $C_n = nC$, namely that it costs simply n times as much to operate an n-warehouse organization as it does to operate a single repository. That is, the marginal increase in fixed costs is $C_n + 1 - C_n$ or just $(n+1-n).C$; that is, it is simply equal to C and remains therefore constant.

We have shown earlier that the average delivery cost per month for a system of n facilities is $cDL/4vn$. An increase by 1 in the number of facilities reduces this cost to $cDL/4v(n+1)$, by a difference of

$$\frac{cDL}{4v}\left(\frac{1}{n}-\frac{1}{n+1}\right)$$

which is

$$\frac{cDL}{4v}\,\frac{1}{n(+1)}.$$

There is a point, therefore, where the savings from lower delivery costs will be just equal to or less than C, the increase in capital costs, and where it will no longer pay to increase n, the number of facilities, any further. We set C equal to

$$\frac{cDL}{4v}\,\frac{1}{n(n+1)}$$

and solve for n. The best number n of decentralized facilities thus would be the integer just larger than:

$$1/2\left(\sqrt{1+\frac{cDL}{Cv}}-1\right) \tag{1}$$

The average service cost per request, S, of this optimal service is given by:

$$s=\frac{nC}{L}+\frac{cD}{4nv} \tag{2}$$

Even these simple formulas have some interesting implications. Let us suppose the following possibilities, as in Table 2:

sensitive over a wide range to proportionate changes in c, D, L, C and v, but is more sensitive to changes in C and v where these values are very small. The table also shows that decentralization may decline as capital costs and/or delivery speeds increase, but that it may increase again at high levels of capital cost and delivery speeds, if the load of service requests and the costs of delivery have risen faster.

This result tends to modify the suggestion of Herbert Simon and his associates, that the introduction of long-distance or intercontinental telephones makes it "unnecessary" for governments "to allow (their) representatives much discretion in the choice of decisional premises." Generally, they stress that "improved methods of communication have made a much greater degree of centralization possible."[10] If we are to judge what is probable or rational, in addition to what is merely possible, we now may consider that a society which produces telephones also produces higher request rates, service demands, and decision loads for public and pri-

TABLE 2. SOME EXAMPLES OF OPTIMAL DECENTRALIZATION LEVELS

	Low Load, Low Delivery Cost		High Load, High Delivery Cost	
	low capital cost, low speed	high capital cost, medium speed	high capital cost, high speed	very high capital cost, very high speed
	A	B	C	D
1. Distance D (miles)	3,000	3,000	3×10^3	3×10^3
2. Load (requests per month) L	3×10^4	3×10^4	3×10^6	3×10^6
3. Costs of delivery c ($ per hour)	0.10	0.10	10.–	10.–
4. Capital cost C ($ per month)	10^2	10^4	10^4	10^5
5. Velocity of transport v (miles per hour)	10	10^2	10^3	10^4
cDL	9×10^6	9×10^8	9×10^{10}	9×10^{10}
Hence Cv	10^3	10^6	10^7	10^9
Then optimum number of facilities N	47	1	47	5
Distance among facilities	64	—	64	600
Cost of service, S, in dollars per request	.32	1.08	.32	.32

This simple exercise demonstrates—as does, of course, the formula

$$\frac{1}{2}\left(\sqrt{1+\frac{cDL}{Cv}}-1\right)$$

given earlier—that this simple model is equally

[10] Simon, Smithburg, & Thompson, *op. cit.*, pp. 275, 279. Simon's thoughtful comments on the various tendencies making for actual, rather than merely possible, centralization are wholly compatible with our analysis. The latter merely adds some emphasis on several no less real tendencies which may work in the opposite direction.

vate agencies, and that they may become more affluent in regard to capital costs, so that the optimum degree of decentralized facilities may increase.

The optimum level of decentralization emerges as a systems property in response to changes, not in one parameter, but in the configuration of several critical parameters. Such a configuration may favor the excellent telephone service and considerable political decentralization of present-day Switzerland rather than the slow messenger service and the much greater political centralization of ancient Egypt or Tokugawa Japan.[11] Even under the simplified assumptions of our prototype model, growth of the capitalization and performance of service organizations of all kinds, including governments, may rationally lead to decentralization or to centralization, depending on which of the relevant parameters grow at a greater rate.

IV. TRANSPORTATION VS. TRANSMISSION: THE BULK OF OBJECTS AND THE LENGTH OF MESSAGES

Whether or not to service requests with a centralized or decentralized deployment of facilities depends to some extent on what is being requested: materials or information. Both are transmitted through channels in which they would move at characteristic primary speeds if the channels were unobstructed. In each case, however, these channels have limited capacities. They can, and usually do, become overloaded and obstructed with a resulting decline in the effective rate of flow. Thus, congested roads may cut the effective speed of traffic flow from 60 to 15 miles per hour; congested airports may cut the effective speed of downtown-to-downtown jet air travel from 600 miles per hour to 300 miles per hour for airport-to-airport speed and 100 m.p.h. for downtown-to-downtown speeds. Similarly, overloaded switchboards and relays reduce the effective speed of electronic transmission from the theoretical limit of the speed of light (186,000 miles per second, or over .67 billion miles per hour, making light from the sun take about nine minutes to reach the earth) to an average effective speed varying from 1,000 to 10,000 miles per hour. Even when delayed in this manner, however, electronically-optically transmitted information will tend to move on the average about 100 times as fast as would bulky material objects. Communication thus differs from transport by several orders of magnitude in regard to the degree of centralization or decentralization which it makes relatively optimal.

At this point it becomes desirable to consider the bulk or weight of objects, measured in pounds or tons, and the length of messages, measured in bits of information. (Actually, we shall be more concerned here with orders of magnitude than with precise measurements.) The cost variable previously introduced measured the cost of "delivering an object" in response to a request, in dollars per hour. But c depends not only on the speed but on the nature of the object as well. It costs more to deliver a massive object than a light one. It costs more to deliver a voluminous message that requires wider and costlier communication channels than to deliver a brief message.

As to the average volume of transmission per request, we shall call it m for material objects and measure it in pounds or tons; and we shall call it b when referring to the transmission of information, which is measured conventionally in "bits." (A bit is a single yes-or-no decision, such as the on-or-off position of a switch, or a dot or dash in the Morse code, or a black or white point on a television screen. A single digit number corresponds to about four bits, a teletype letter six bits, an average English word about thirty bits, a printed page in a book about 10,000 bits, a 300-page book three megabits, i.e., three million bits, and over twenty megabits for one second's picture on a normal television screen.) The capacity of a transportation channel then is the amount of mass M it can move times the velocity v (i.e., distance/time) at which it can do so under the best traffic conditions. The formula for this is Mv for the overall capacity of the channel and mv for its effective capacity in regard to the size and speed of transmitting and/or servicing the average request. Both Mv and mv are formally analogous to the concept of momentum in mechanics. As it takes force to change momentum in mechanics, so it takes innovation—in the form of visible investment in capital equipment and/or invisible investment in new technology—to change capacity in a

[11] On centralization and communication at earlier levels of technology, Lewis Mumford comments: "Action at a distance, through scribes and swift messengers, was one of the identifying marks of the new megamachine (the centralized state) . . . 'The scribe, he directeth every work that is in this land,' an Egyptian New Kingdom composition tells us . . . They made possible the constant 'report to political headquarters' essential for a centralized organization": *The Myth of the Machine* (New York: Harcourt Brace, 1967), p. 192. Cf. also Harold A. Innis, *Empire and Communication*, (Oxford: Oxford University Press, 1950).

transport channel (e.g., to a supersonic transport aircraft). The change in capacity Mv is then a possible measure of the effect (or "force") of innovation in respect to the overall capacity of a channel, and the change in mv similarly would measure its effect in regard to the servicing of average sized requests.

Decentralization often depends critically on the effective service capacities of the available transport and communication channels, and changes in the optimum level of decentralization for a given organization, government or service.

The following table may clarify some of the relations between capacities, rates and the different conditions examined so far.

munication, though, of course, the cost differs. These channels differ mainly in their capacity, in how many bits, or bv, similar to v per second of information, mv for materials. In the communication processes, therefore, the number of duplicated facilities does not depend very much on geography with respect to saving message delivery time. The distribution of overall channel capacities, B (bits per second), and data transmission rates B', in place of bv (i.e., message speed times message length), plays a much more central role in this regard. A low value of B is about 2,000 bits per second for voice-grade telephone lines, and a high value about 200,000 bits per second for wireless transmission, such as radio telephony or tele-

TABLE 3. SUMMARY OF KEY VARIABLES

3a. Basic Variables

D = length of strip, in miles, typically 3,000 for a big country, 300 for little one

L = total load, in requests per month, about 30,000 for heavy demand, 3,000 for low demand

C = fixed cost for one facility, in dollars per month, about 10,000 for an expensive facility and 100 for a cheap one

v = average transport speed under actual traffic conditions, in miles per hour, about 1,000 for jet speeds and 10 to 100 for ground passenger speeds

m = mass or bulk of items to be transported in response to requests, in pounds, about 100 for massive objects to 1 for light ones

b = amount of information in an average message delivered in response to a request, in bits, from 10 megabits for a second's worth of TV to 10,000 for a printed page's worth

B' = rates of information transmission under channel conditions, in bits/second, from 10^7 for video to 2,000 for digital telephone

3b. Parameters	*Unit Transport Cost*	*Average Cost To Service Request*	*Assumption*
Simplest Model ($m=0$)	c_v, dollars per hour, for transport at a speed-capacity of v miles per hour	$c_v \cdot \frac{D}{4nv}$	$v=V$
Bulk Taken Into Account	c_{MV}', dollars per pound per hour, for transport characterized by speed-bulk capacity of MV pound-miles per hour	$c_{MV}' \cdot m \frac{D}{4nv}$	$mv=MV$
Digital	c_B'', dollars per mile per hour, for communication characterized by a capacity of B bits per second	$c_B'' \frac{D}{4n} \frac{1}{3600} \frac{b}{B'}$	$B'=B$

Actual rather than maximum transmission speeds, however, are very nearly the same for a person-to-person call over a distance of thirty miles than over a distance of 3,000 miles and what really counts is not the velocity in miles per hour but the capacity in bits per second. The value of v for telephone, telegraph, wireless via radio or television, etc., is substantially the same for local as for long-distance com-

graphy, and at least one hundred times this amount for television.

These considerations permit us to make our notion of current transport costs somewhat more realistic. Let us begin with the case of bulk movements, such as mail, freight, or personnel. We have previously considered c, or more precisely, c_V, as the transport cost in dollars per hour depending only on speed

capacity, but regardless of the bulk to be moved. Now let us consider, instead, $c'MV$ as the transport cost in dollars per pound transported per hour for a carrier with capacity MV. Here it costs more to move the same bulk of freight more quickly, as airmail and airfreight cost more than do slower services, but it also still costs more to move a larger number of pounds at unchanged speeds. The average transport cost per request is simply $c'MV$ times m times D/v. The optimal number of facilities can therefore be calculated with these refined assumptions by the formula:

$$\frac{1}{2}\left(\sqrt{1+\frac{c'_{MV}DLm}{Cv}}-1\right) \qquad (3)$$

The average cost of service S' per request of average shipment weight m then becomes:

$$S' = \frac{nC}{L} + \frac{c'Dm}{4nv} \qquad (4)$$

and the total cost of service $S'' = S'L$ (Formula 4a).

In order to take into account in these formulas the weight of the average shipment, we had to replace c/C by c'/C and D/v by mD/v, while the total load of requests, L, has remained unchanged. If $c'_{MV} = m$, the cost of transporting the shipments of average weight m, equals c_V, the average transport cost of filling a request, then the weight of the average shipment would make no difference for the optimum level of decentralization, and the results of Table 2 would remain unchanged. If, however, an increase in the average weight of shipment would bring about an increase in the transport cost, then the optimum number of facilities would increase, in proportion to the square root of the change: a 100-fold increase in average shipment weight (and cost) would then require approximately a 10-fold increase in the most efficient number of facilities.

Let us assume that an administrative agency has to consult a file on each case on which it has to make a decision. Servicing a case would then require either transporting, on the average, one pound of documents from the repository to the site of the request, or else transmitting the information in those documents over a long distance computer channel. The file to be transmitted is assumed to contain the equivalent of a 200-page book, that is, about 4 million bits, or 4 megabits.[12] According to formula (3), the optimal number of facilities for electronic transmission under the above assumptions is now sixteen. This is more than three times as many as in the earlier case presented in column D of Table 2, where the transmission costs depended only on speeds and not on the amount of information being transmitted to service the average request.

Had we sent our four megabits by express messenger service at the rate of $c' = \$.04$ per pound-hour, then the optimal number of facilities, according to formula (2), would be three. In other words, *under the assumptions of our example, a slower service based on messengers or mail delivery could be much more highly centralized than a fast service based on a computer-based or satellite-based communication net.*

It might be interesting to compare the total costs for these two kinds of service. Let us recall that each of them would have to cope with 3 million requests per month, each request requiring transmission of four megabits. The cost of servicing requests by digital telephone—called "dataphone"—transmission would be $1.05 per request, compared with $.20 per request for service by mail or express messenger. *The five-fold increase in cost,* however, *here would buy a sixty-fold increase in speed.* The dataphone replies would be available within little more than half an hour, in contrast to the thirty hours response time required for the more highly centralized service based on messengers or mail.

The situation is similar in some respects to the transmission of messages through communication channels. Here c_B'' is the cost of transmission for a channel with a capacity (maximum rate) of B bits per second, in dollars per hour per mile. The average cost of servicing a request would then be $c_B''/3600 \cdot b/B \cdot D/4n$, because $D/4n$ is the average distance from a request to its nearest service facility when there are n such facilities. The actual cost of telephone transmission does not increase in *direct* proportion to distance; it increases by less for large distances than it does for small distances, but this will be neglected for the purpose of this paper. The cost c_B'' for long distance telephone is about $.02 per mile per hour for ordinary long-distance transmission. (This would amount to $3.00 for a three-minute business-hour call from Boston to San Francisco and $1.00 for a similar Boston-

[12] This calculation assumes roughly the usual average of 6 bits per character, 80 characters per line, and 42 lines per page. For further discussion, see Manfred Kochen (ed.), *The Growth of Knowledge* (New York: John Wiley & Sons, Inc., 1967); also Manfred Kochen, "Newer Techniques for Processing Bibliographic Information," in F. Kilgour (ed.), *Proceedings of the Symposium on Data Processing in University Libraries* (Philadelphia: Drexel Institute of Technology, 1968).

Chicago call.) Such a channel is characterized by a capacity of $B=2000$ bits per second.

The formula for computing the optimal number of facilities in this case is therefore

$$\frac{1}{2}\left(\sqrt{1+\frac{c_B''}{C}\cdot\frac{1}{3600}\cdot\frac{b}{B}\cdot D\cdot L}-1\right). \quad (5)$$

Note that we have replaced c/C and D/v in formula (1), or c'/C and Dm/v in formula (2) by c_B''/C and $1/3600\cdot b/B$ respectively.

V. THE VALUE OF TIME: A FIRST APPROACH THROUGH OPTIMIZATION

In the primitive model treated so far, no value was assigned to time. Users were assumed not to care how long they had to wait for service. In fact, however, users find service with smaller delivery time more valuable than service with high delivery time. Similarly, clients or constituents of a governmental agency often request decisions which are to be acceptable to them in substance but which are valued more highly if they are prompt in time. In some other studies, the probability that a government, or a governmental agency, will produce within an acceptable time an acceptable response to the average demand upon it has been called the *responsiveness* of that agency.[13] Responsiveness and mean response time thus may be included among the highly relevant performance indicators of governments and their agencies. This section of our inquiry will attempt to make explicit some possible relationships between response time, the utility of time to the average user, and the degree of decentralization.

As a first step, let us assume a single "representative user" with the properties of both the average and the median users of the service, who are assumed to be identical. (We assume at this point uniform distribution of locations and utilities for all users.)

The net worth, W, of the institution is its utility to the user minus the cost to operate it. Figure 4 shows a possible relation between utility and communication speed, compared with approximate current rates for sending a brief message across 1000 miles in the United States.

It is difficult to infer even very roughly the shape of the demand curve of the American people for speed of transmission of their messages. It is illuminating, however, to plot the rates for the transmission of messages by telephone, telegraph, and mail (air and regular). All these rates have been set by complex processes. These are at least partially responsive to the political demand generated by the population in their roles as interest groups and citizens. Since these rates have remained relatively stable and have not aroused intense opposition, they can serve as indicators of acceptability. Fitting a curve to these data suggests a monotonically decreasing function of rate versus delivery time such as shown in this figure.

Let us first consider the case of a society which can afford to back up its utilities completely with actual purchasing power representing all the required resources. Such a society might then consider increasing the number of decentralized facilities n so long as this increases the net worth to the society. It would stop increasing only when the marginal increase in net worth with n duplicated centers is zero. This enables us to derive a formula for n, which depends on the utility of time. The higher the utility of time, the more we shall be willing to pay for higher speeds of response or of service.

The utility of time will be large in a society that has urgent needs to save time or that puts great cultural value on speed (which some critics of contemporary Western culture might call impatience), or whose members have many attractive alternative opportunities for occupying, so that the opportunity cost of time to them is raised sometimes to an oppressive degree.[14] Our R may reflect a combination of all these pressures. The same cultural factors of urgency and impatience would be reflected to an even greater extent in the shape of a curve describing the variation of utility with response time. The greater the society's concern with speed, the steeper the curve will be.

In the case just presented, the optimal degree of decentralization would be to make n as large as possible if the value of time is larger than the increase in total cost. If the opposite condition

[13] On the concept of responsiveness, see K. W. Deutsch, S. A. Burrell, *et. al.*, *Political Community and the North Atlantic Area*, (Princeton: Princeton University Press, 1957, 1968), pp. 40–41, 199–200; Bruce M. Russett, *Community and Contention: Britain and America in the Twentieth Century*, (Cambridge: M.I.T. Press, 1963), pp. 29–30; Dean Y. Pruitt, "Definition of the Situation as a Determinant of International Action," in Herbert C. Kelman (ed.), *International Behavior: A Social-Psychological Analysis* (New York: Holt, Rinehart and Winston, 1965), pp. 397–399.

[14] On the general problem of the "quality of life," which is connected with our growing lack of and need for time, compare John Kenneth Galbraith, *The Affluent Society* (New York: Houghton-Mifflin, 1958) and *The New Industrial State* (New York: Houghton-Mifflin, 1967).

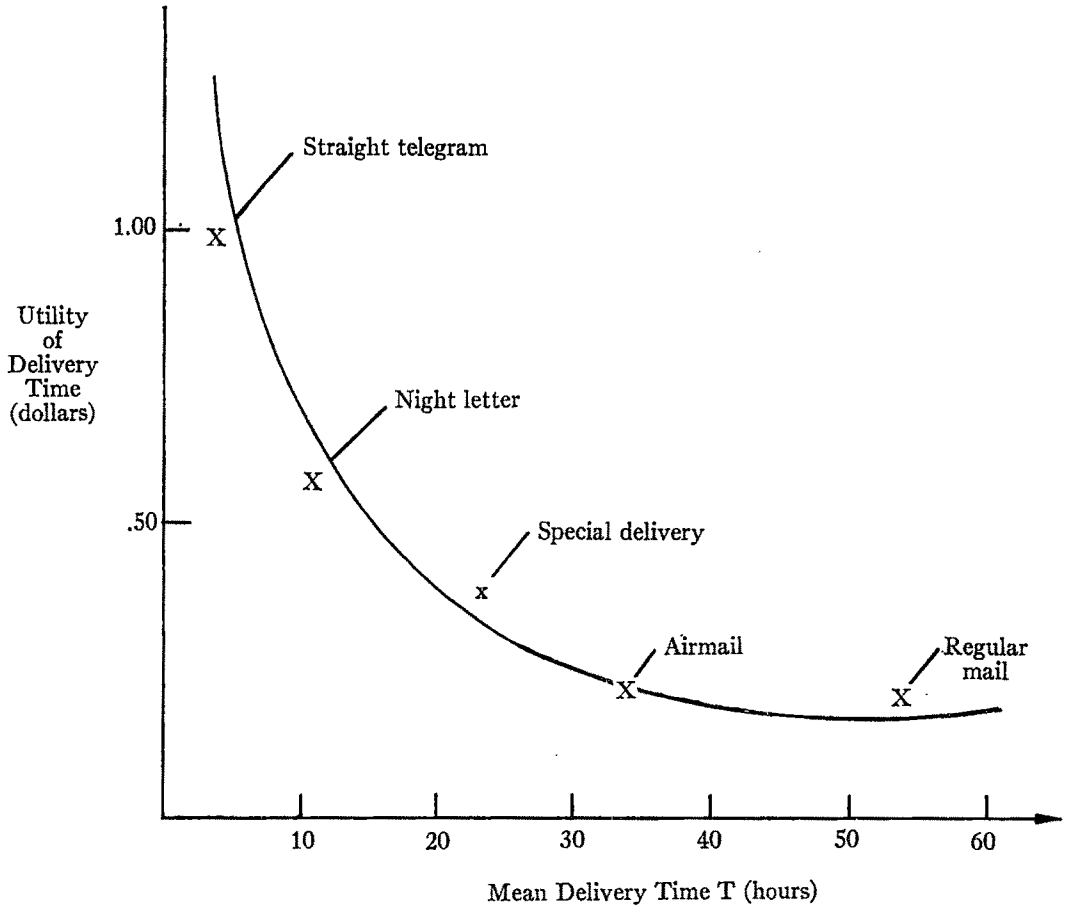


FIGURE 4

holds and increases in the speed of response on service are valued less highly than the increases in cost resulting from increases in decentralization, then the optimum number for n is 1; that is, complete centralization. A society as rich and as pressed for time, therefore, as we assumed it to be in this example would tend to go to one extreme or the other: complete centralization or decentralization. The tendencies toward substituting in many parts of the United States individual transportation by personal automobile instead of public transportation may resemble in some ways the second of these trends—up to the point, at least, where mutual interference among automobiles on congested streets and highways becomes critical.

MORE REFINED MODELS: REDUNDANCY AND FLUCTUATIONS IN DEMAND

Using multiple facilities for more dependable service. In the models analyzed so far, we assumed that if there are n duplicated facilities any one of which could have serviced all the L requests per month, then each facility is used only $1/n$th of the time. Each is to some degree redundant, therefore, for each now services only L/n requests per month. For a decentralized organization, with large n, the utilization percentage is quite low and redundancy is high. If low response time has very high utility —i.e., the value of R is high—and transmission costs are high but fixed costs are not too high, then such low utilization is tolerable. The unused portion of the time, however, has considerable potential value in:

(a) insurance against the effects of breakdown;
(b) smoothing losses due to fluctuating demands and busy local facilities;
(c) control of errors.

Clearly this average delivery time increases with the probability of breakdown in one parti-

cular facility, but in proportion to n. As we decentralize, we gain more side benefits in reduced mean time loss due to failure. The greater the transmission speed v, the greater the effect of this time saving. Technical progress may confront designers with a choice between increasing the speed or the reliability of their equipment. Decentralization and redundancy, at higher cost but at the same level of technology, may permit them to have both.

Total response time and fluctuations in demand. Instead of mean delivery time $\overline{T}_n$, it would be more realistic to consider the total response time Q_n. This is the total time lapse between the epoch the request originates and the epoch the information or item requested is received by the requestor at the site where it originated. It is this turn-around time which matters to the originator of a request. This is the sum of three terms:

$$Q_n = F_n + S_n + T_n \tag{6}$$

Here F_n is the time to transmit and forward the request from its source to the nearest station able to service it.

It is the run-around time, which the customer does not notice, apart from the turn-around time.

Next, we let Sn be the time it takes to service the request at that station, including the time at the other stations which had to decide whether to pass it on and to do so.

Finally, T_n is the time to transmit or transport the results back to the source, as defined before.

Since we can calculate $\overline{T}_n$, we can do likewise for $\overline{F}_n$ and $\overline{S}_n$. The latter term, the internal service time S_n, includes the time needed for decision-making and the possible delays resulting from the overloading of the decision-making and the possible delays resulting from the overloading of the decision-making capabilities of the service organization or bureaucratic agency. This aspect of the problem has received a good deal of attention in the literature of organization theory and management science.[15]

If the total demand is L' requests per hour, then each of n stations has to service L'/n requests per hour under the assumption of uniformity over length D of the strip. If we assume 10×30 or 300 hours per month, then $L = 300\ L'$ where L is the load in requests per month, as previously defined. If one service could handle the total demand, its service time must be $S=1/L'$ hours per request. Thus n replicated facilities offering this service could handle n times this total demand or each be utilized only a fraction $1/n$ of the time. On the other hand, slower services, with service time $S_n=n/L'$, could be used to handle the same demand, and these would be utilized one-hundred-per-cent.

Of course, the cost of such a slower service with service time n/L' hours per request should not be C, the cost of a service with $S=1/L'$ hours per request; it should be lower. Since many identical such services are required, their fixed cost C_n might be lowered still further, at least in regard to those components that could be produced in quantity.

If the demand is not constant in time, but fluctuates randomly, and if the capacity of each facility is only equal to the average demand, then there will be times when a request cannot be serviced by the nearest server because it is busy. In that event, the request is forwarded according to the same pattern used for routing when the nearest server was down. If the request has to be rerouted several times, $\overline{F}_n$ becomes a "run-around time," comparable to the possibly longer or shorter waiting time in a queue or single centralized facility. In either case, the effective waiting time may exceed the waiting capacity or tolerance of the client, as in the case of emergency patients for hospitals, or of emergency responses in military or police systems, or in urgent local or national political decisions.

The formulas and calculations involved even in this simplified model would permit us to derive conditions on R and optimal n in many additional pages of more complex mathematics. For this reason, as well as for reason of the greater flexibility in modeling more realistic systems, it seems desirable for future work to obtain these quantities by computer, either by evaluating complex formulas or by Monte-Carlo simulation.

In this more realistic and complex analysis, we suppose that the service time, s, per server varies with the percentage u "he" is utilized. That is, u is the percentage of time a facility is utilized.

If u is small, $s(u)$ is high. He is slow because he is out of practice, because start-up time causes delays, etc. If u is close to 100%, $s(u)$ is also high. He is slow again, this time because he is overloaded. Some services don't get done at all if the server is too overloaded, and this may be far more costly than under-utilizing him; this is especially true when the value of the service is high, as in cases where the server is a highly paid executive. For intermediate values of u, $s(u)$ is moderate. A curve like $s(u)=s_0$

[15] See references in notes 2–6, above.

$(u-u_0)^2+s_{00}$ might represent the general shape of the curve.

VII. SOME IMPLICATIONS FOR ORGANIZATION THEORY AND POLITICS: SOME SECULAR CHANGES IN KEY VARIABLES

Our crude models suggested that a greater number of multiple service facilities, n, and hence a higher degree of decentralization, becomes rational with an increase in the load or volume of services demanded. Also, a higher degree of decentralization becomes desirable with an increase in the value of time, and in those portions of service costs which are related to time and/or distance.

Our models further suggested that this optimal degree of decentralization increases with a decrease in the proportion of the fixed capital costs of the service among its total costs; and they suggest that the optimum degree of decentralization also increases with any decrease in the margins of error and of breakdown that are considered acceptable in the performance of the service.

All these decentralization-promoting conditions will tend to increase with economic growth and technological development. Let us first consider changes in direct costs. If other things remain equal, then the optimal number of decentralized facilities for a service increases in proportion to the square root of the ratio of its time-distance costs c to its capital costs C. Secular trends toward highly advanced levels of economic and technological development make human labor more expensive, and they make capital goods relatively cheaper. Thus they may tend to increase this ratio, and hence the desirable degree of decentralization. Some economic data suggest that this may have happened, both in Communist and non-Communist countries. An increase in real wages normally represents an increase in the price of labor relative to the price of goods, including capital goods, and hence also an increase in the c/C ratio. In many advanced countries of the world during recent decades, real wages have tended to grow on the average at about three per cent per year, thus doubling every twenty-three years. If real wages should grow fourfold in the course of a half-century, while capital costs would not, then at the end of such a period the optimal number of decentralized facilities for a wide variety of services may well have as much as doubled during the same time. If capital costs should grow somewhat but less than real wages, there would still be a net tendency toward some decentralization, albeit to a lesser extent.

Demographic, economic and technological growth in many countries in recent decades, very roughly speaking, has proceeded at annual rates of about 2 per cent and 3 per cent, respectively, adding up to about annual 5 per cent increase in the demands for many services, and doubling about every 14 years the load upon many of the public or private centers that have to supply them. Such increases in loads include the volume of demands for many political, administrative, judicial and quasi-judicial decisions and responses, as well as for many services and for the communication and transportation they imply.[16] Moreover, as

[16] In the 1950s, the average rate of per capita income growth in 68 countries, comprising 86 per cent of the world's population, was about 3 per cent, while the average rate of population growth in 111 countries, with 97 per cent of mankind was 2.3 per cent. (B. M. Russett, *et. al.*, *World Handbook of Political and Social Indicators*, New Haven: Yale University Press, 1964, pp. 46–48, 160–161.) More recently, in 1960–66, world population increased at an annual rate of 1.9 per cent. During the last two decades, total GNP in many countries may have been rising, therefore, at about 5 per cent per year, and thus doubling every 14 years. Request loads and service loads in many fields, including governments may have grown at about the same rate as GNP. *United Nations Demographic Yearbook*, 1966 (New York: 1967), p. 95. During the same years, 1960–66, the population of the United States increased at the rate of 1.4 per cent, while its gross national product in constant 1958 prices grew at 4.8 per cent. Murders and non-negligent homicides grew at 2.3 per cent, all accidents at 2.8 per cent, domestic mail at 3.5 per cent, and daily telephone conversations at 5.0 per cent, and automobile thefts at about 7 per cent. In contrast to this, the average speed of motor vehicles increased only at 1.4 per cent a year, and about the same low rate held for the 1945–66 period. The average speed of aircraft on scheduled domestic air carrier routes increased at about 3.5 per cent for 1950–67 period, but rose more quickly, at 5.0 per cent, during the years 1960–67, and the speed of aircraft on international flights in 1960–67 increased at about 7 per cent per year. (Approximate rates, computed from data in the *Statistical Abstract of the United States 1967*, pp. 58, 86, 505, 508–509, 561 and 586; and *ibid.*, 1968, pp. 6, 550 and 573; and *Uniform Crime Reports for the United States*, 1960, p. 2, and *ibid.*, 1965, p. 3.) These figures exclude, however, the delays caused by getting to and from the airports, and the delays by traffic congestion on the ground, and in the air above many airports. If these delays are taken into account, it seems plausible that many activities,

technological progress speeds up transport and communication, it increases the opportunity cost of time, since more things now could be done or experienced in each unit of time than had been the case earlier. Finally, highly modernized and automated technologies may require higher levels of accuracy and reliability of service. Thus they may need more redundancy of facilities as an insurance against error and breakdown.

Perhaps most importantly, the growing need for greater precision and adaptability of responses to the increasingly complex, diverse and changing needs of people will require in many cases more frequent dialogs and feedback cycles of back-and-forth communication between service centers and customers, agencies and clients, administrators and constituents, the rulers and the ruled. This will multiply the effective distance D (regardless of whether it is measured in rules, switching points, or intervening obstacles or opportunities); it will multiply it by the average number of feedback cycles of communication, travel or transport required to ensure an acceptable fit between each specific demand and the particular response to it. The current demands for more popular participation, initiative and communication "from below," if heeded, would have similar effects: it would increase the average number of feedback cycles needed, and hence often the importance of distance and proximity. Each of these changes makes for an increase in the degree of desirable decentralization.

Under the assumptions of our models it seems quite possible, therefore, that many of the highly modernized and automated societies and nations which we may find by the end of this century will by then require a much greater degree of decentralization in public and private services than the one to which we are accustomed now. Doubtless some of these assumptions will have to be replaced by more realistic ones. In particular, refining our simple concept of decentralization, introducing the notion of hierarchies of facilities, and revising the assumption of uniform distribution of demand in space and time, all may well modify some of our findings. The present paper already may suggest, however, that the naive images of a future of ever increasing centralization in business organizations, public services, national governments, and the international political systems may have to be revised.

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calling for public service, now may be growing faster than does our effective speed in moving persons and goods in daily practice. Something similar may apply to telephone communications: a telephone call from Washington to San Francisco during business hours does not get through much more quickly to the individual we want to reach in a busy office than it did in 1945.

THE DEVELOPMENT OF POLITICAL IDEOLOGY: A FRAMEWORK FOR THE ANALYSIS OF POLITICAL SOCIALIZATION*

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"Myself, I get confused. The President tells ya that he don't want no war, it's peace. You pick up a paper, they're bombing children. And television, the guys being interviewed, talking about peace, and the picture shows where the women and children are being bombed and slaughtered and murdered. How long if I think that way and I have had a bad feeling, how long will other people that their mentality's not strong enough, to separate the cause of it? Fear. What's gonna happen to our kids, our grandchildren?

"Lotta them are afraid of their jobs, losing their jobs. Because the government's maybe got some contract with some company. For example, we got one fellow here works with the government, with this here carbonic gas or whatever it is. If he opens his mouth up too much, he can lose his job. And the senators or congressmen, they personally don't take interest in their own country, right here, what's going on.

"The colored. We had a tavern on 61st and State, three and a half years, Negro neighborhood. I tell you I never was insulted no place by not a Negro person over there. They respected me highly. It took a white fella to come in and insult me because I wouldn't serve him beer, he was too drunk. And if it wasn't for these poor Negro fellas, I'd a probably killed this man. (Laughs) Because he called me a dirty name."

Eva Barnes, 56, citizen of Chicago.[1]

I. FOUR THEORIES ABOUT THE SOURCES OF IDEOLOGY

Mrs. Barnes is trying to understand the political world, but she suffers from a common problem. She has no intellectual handle on it. She blames the government for acting inconsistently, but she herself is inconsistent. She deplores war and its slaughter; yet she can imagine herself killing a man who has merely called her "a dirty name." She rambles from war to governmental control of employment to the problem of race. There are no explicit intellectual links in this chain. Rather, her choice of topics seems almost randomly martialed. For example, "them" in the first sentence of the second paragraph has no clear referent; therefore, we have no way of knowing the logic that led her from war to the involvement of government in employment. And, of course, she speaks primarily from and about personal experiences, which she uses to illustrate her generalizations. There is the "fellow . . . (who) works with the government," and the incident in the barroom. Each single case must support by itself the general principle. Finally, her remarks include no familiar political concepts by which we can connect one observation to the next. We can understand why Mrs. Barnes yearns for "the cause of it."

Is Mrs. Barnes atypical? Yes, but in an unexpected way. She has been a political activist. She helped unionize the Chicago stockyards under conditions which might well have made her reflective about her political world.[2] The vast majority of her fellow citizens have never done anything as political as union organizing. Consequently, their conversations about politics might be even more fragmented than hers.

Although Mrs. Barnes' discussion of politics evidences a desire to understand, it is uninformed by any basic ideational framework which would permit comprehension. This framework may be called ideology. It is by now a commonplace that the mass of Americans do not have a sophisticated conceptual organization by which politics may be understood. Perhaps the aptest description of this situation is contained in Philip E. Converse's "The Nature of Belief Systems in Mass Publics."[3] Converse defines political ideologies as belief systems characterized by high constraint, great range, and a centrality of political items.[4] These belief systems provide conceptual and terminological canals into which the flood of political events which overwhelms Mrs. Barnes can be diverted and managed.

Political scientists have emphasized different components of ideology. The concept of con-

*I wish to thank Fred Greenstein for his comments on an earlier version of this piece. He is absolved of any responsibility for what follows.

[1] Studs Terkel, *Division Street: America* (New York: Pantheon Books, 1967), p. 65.

[2] *Ibid.*, pp. 61–62.

[3] In *Ideology and Discontent*, ed., David E. Apter (New York: The Free Press of Glencoe, 1964), pp. 206–262. For an interesting attempt to rebut Converse, see Norman R. Luttberg, "The Structure of Beliefs among Leaders and the Public," *Public Opinion Quarterly*, 32 (Fall, 1968), 398–410.

[4] Converse, *loc. cit.*, pp. 207–213.

straint is vital to Converse's analysis. Constraint "may be taken to mean the success we would have in predicting, given initial knowledge that an individual holds a specified attitude, that he holds certain further ideas and attitudes."[5] But ideologies also provide standards by which political events may be evaluated, allowing a person to approve some and deplore others. While Converse's discussion of ideology stresses cognition, Lane's definition of ideology emphasizes evaluation. To Lane, ideologies "are normative, ethical, moral in tone and content."[6]

My understanding of ideology encompasses both panels of this picture. Let us think of ideology as involving: 1) a considerable number of constrained political ideas. By "constraint" let us mean, with Converse, that if one idea changes, those others related to it in the ideology will change as well. 2) An evaluational and prescriptive system. The ideology sets forth a statement of political preferences. 3) Persistence. An ideology must have some arbitrary, but considerable, duration in order for us to distinguish its components from passing whims. 4) Global standards. The judgments applied to any sub-category of political events within the ideology are exhaustive and consistent. 5) Boundaries. Political events which fall into different sub-categories within the ideology are sharply distinguished from each other and are, therefore, judged differently by and within the ideology. 6) Deductive consistency. Given inherently non-logical premises, deductions must occur in accordance with the rules of logic. For example, if a person, because of some views of individual morality, favors a balanced budget, he cannot, at the same time, believe in extra-budgetary social welfare payments to his own group. In short, ideological prescriptions must not produce logical absurdities. 7) Activist directives. Political ideologies do not produce apathy.

According to Converse, no more than 2½% of Americans meet our qualifications as ideologues.[7] There is no clear understanding among political scientists as to why so few Americans think ideologically. After all, our society is characterized by the largest percentages of college students and graduates in the world, great affluence, and instant contact with political events through the mass media. Shouldn't these things help to produce more ideologues? Four attempted explanations of this paradox exist, but, as we shall see, none is successful.

A favorite hypothesis notes a relationship between political activity and the development of political ideologies. More generally, the argument may be formulated as follows: The greater his proximity to politics at any time, the greater is the likelihood that a person will become an ideologue. Few people participate; therefore, few are ideologues.[8] According to this hypothesis, the more active a person is in politics, the more familiar with political life he becomes. His familiarity leads him to forge the informational links between political events which encourage and sustain the growth of a political ideology. Furthermore, the more active he becomes, the more he associates with others who speak in terms of and group themselves around political ideologies, such as liberalism or conservatism. These are only a sample of the supporting arguments for the hypothesis, but they suffice for our purposes.

No one doubts the high correlation between sustained political activity and the existence of political ideologies.[9] But even if all the ideologues so defined by Converse were political "gladiators,"[10] many other political participants would not be included. Nor in reality should we expect that all ideologues engage in sustained political activity. In short, many political gladiators are no doubt able to function without the benefit of tightly knit political ideologies; and some ideologues do not become regular political participants.

The hypothesis may also be attacked even when we take the case most favorable to it. We would certainly expect, under the terms of the argument, that the sons and daughters of the politically active would be themselves especially likely to become both politically active and ideological. Politics becomes proximate to them at particularly early ages, and they, surrounded as they are by political influences and having a set of ready-made contacts, should naturally fall into a political life. However, the process is neither so simple nor so encompassing as it seems, for we can observe many sons and daughters of politicians who resist the political siren. No compelling political ideology pushes them into politics. On the other hand, there are many people from backgrounds of political apathy who, moved by motives divorced from the proximity of

[5] *Ibid.*, p. 207.

[6] Robert E. Lane, *Political Ideology* (New York: The Free Press of Glencoe, 1962), p. 15. The uses of the term "ideology" are legion. Therefore, we have defined the term in our own way. For further elucidation, see *Ibid.*, pp. 13–17.

[7] Converse, *op. cit.*, p. 218.

[8] For a thoroughly tangled statement of the interrelationships of participation and cognition, see Lester W. Milbrath, *Political Participation* (Chicago: Rand-McNally, 1965), pp. 62–66ff.

[9] *Ibid.*

[10] *Ibid.*, p. 21.

political decisions, enter into political careers. Indeed, it is often those from the most unpromising backgrounds who become the most ideologically deviant and politically militant in their society. For all these reasons, it would appear that the proximal politics hypothesis is insufficient. It can not tell us why the proximity of politics is not sufficient for ideological development among all the politically active. It can not tell us why some children of politicians become active themselves and others do not. It can not tell us why some people from backgrounds in which politics is only dimly perceived become political ideologues. Nor can it tell us why some ideologues do not engage in sustained political activity.

A second, weaker hypothesis concentrates on sociological factors. This argument asserts that the development of political ideologies is associated with particular positions in the social structure. For example, as Converse and others stress, ideology is closely related to educational level.[11] It also appears that extremist ideologies often originate in social positions which are exposed to sociological discontinuities, such as, for example, status inconsistency.[12] People experiencing status inconsistency are accepted by society in some respects and rejected in others. A case in point, in terms of European society, involves the early marginal status of the Nazi ideologues, numbers of whom felt themselves worthy of respect as ex-soldiers and German patriots, but could find no place in the Weimar Republic. In addition, many of the early Nazi leaders came from geographically marginal areas of the Reich.[13] Another European case involves the many middle class Jewish ideologues, such as Karl Marx, whose financial standing was more than respectable, but whose religion set them apart.

Unfortunately, these sociological hypotheses suffer from the same difficulties encountered in our earlier discussion of the proximal politics argument. For example, there are many more people in positions of status inconsistency, particularly in an upwardly mobile, highly fluid society such as ours, than there are ideologues. Furthermore, a respectable body of sociological theory suggests that the modal response to status inconsistency and cross-pressures is withdrawal rather than activism.[14] There are, of course, many more educated men than there are political ideologues. We are forced to conclude that while this theory may provide clues to help us discover those positions in society where ideologues are most likely to cluster, it can do little more. It does not explain the existence of ideology any more than the arrival of clouds explains the consequent rain.

A third suggested explanation is also heavily sociological, though it is not so well defined as the previous two arguments examined. It may, therefore, be dealt with more rapidly. Some observers feel that while ideology in the United States and Western Europe once flourished, fed by the class antagonisms of stratified industrial societies, the age of ideology has come to a close.[15] The social basis of ideological thought has simply disintegrated under the impact of widespread affluence and the lowering of class barriers. This position may be faulted on two counts. It assumes that ideological thought was more characteristic of nineteenth century than twentieth century America, an assumption for which it is impossible to gain reliable evidence. But, in addition, it implies that contemporary American politics is not marked by ideological divisions. Yet we are living in a comparatively rich time for the generation of ideology. Black Power, Student Power, New Left Radicalism, and their conservative counterparts fill the news and shape the nation's politics. Furthermore, the segment of our population most indulged by our affluence—college students—has been disproportionately responsible for the development of these new ideologies. For these reasons, the end of ideology hypothesis is unpersuasive.

Finally, we may take brief note of an hypothesis which attributes the absence of large numbers of American ideologues to the American "national character."[16] Americans are alleged to be "pragmatic" people whose instincts do not

[11] Converse, *op. cit.*, p. 213. See also Angus Campbell *et al., The American Voter* (New York: Wiley, 1960), p. 476.

[12] See, for example, the pieces by Seymour Lipset and Richard Hofstader in *The Radical Right*, ed., Daniel Bell (Garden City: Doubleday Anchor Books, 1964); and Joseph R. Gusfield, *Symbolic Crusade* (Urbana: University of Illinois Press, 1963). For an early statement of the theory underlying these formulations, see Gerhard Lenski, "Status Crystallization: A Non-Vertical Dimension of Social Status," *American Sociological Review*, 19 (August, 1954), 405–413.

[13] Daniel Lerner, *The Nazi Elite* (Stanford: Stanford University Press, 1951), p. 85.

[14] For a review and empirical test of the two theories, see Richard M. Merelman, "Intimate Environments and Political Behavior," *Midwest Journal of Political Science*, 12 (August, 1968), 382–400. A case study of the same phenomenon is E. Franklin Frazier, *Black Bourgeoisie* (New York: Collier Books, 1962).

[15] The fullest statement of this position may be found in Daniel Bell, *The End of Ideology* (New York: Collier Books, 1961), pp. 393–407.

[16] For one of the best national character treatments of Americans, see Max Lerner, *America as a*

lead them into the airy realms of ideology. We can see immediately that this position is no explanation; it is, rather, another way of saying the same thing. Even if true, it does not tell us *why* Americans are the way they are. Therefore, the national character hypothesis is of no use for our discussion.

If none of these attempted explanations is sufficient, where can we turn? The answer is simple. The four arguments fail primarily because they are unable to account for the development of an ideology in some people and not in others. The key word, in this formulation, is *development*. A person must be psychologically capable of ideological thought. We must uncover and describe both those developmental patterns which produce ideologues and those which hinder the growth of ideology. Our definition of ideology tells us where to begin the search for these patterns. To become an ideologue, a person must: 1) have cognitive skills which allow him to see linkages between ideas and events. Such linkages determine the amount of constraint in his belief system. 2) Have a developed morality which allows him to evaluate consistently the ethical meanings of political events. In order, therefore, to explain the development of political ideologies, we must explore the course of cognitive and moral development. So far, this developmental process and the literature which studies it is largely uncodified and unintegrated into the study of both political science as a whole and of political socialization in particular. This omission is particularly important for the latter area, because political socialization is the branch of our discipline most concerned with human development. Indeed, as we shall see, knowledge about the development of moral and cognitive skills not only enlightens us about the question of ideology but also has broader import for the entire study of poliitcal socialization. Therefore, let us see if we can understand and describe the growth of those cognitive and moral skills which are prerequisites for ideological thought.

II. A MODEL OF IDEOLOGY FORMATION

We will begin by sketching an ideal-typical psychological development culminating in the ability to think ideologically. The pattern is characterized by a passage through a series of psychological stages. It is true that psychological theories relying upon developmental stages are subject to Bandura and Walter's criticism that "Stage theories have at best specified only vaguely the conditions that lead to changes in behavior from one level to another."[17] However deficient though such theories are, their division of a complex process of growth into distinct phases does provide an unusually clear model against which real-world development may be compared.

Let us first turn to the cognitive side of ideological development. As Adelson and O'Neil put it, ". . . the growth of cognitive capacity allows *the birth of ideology* [italics theirs]. . . . What passes for ideology in . . . younger respondents is a raggle-taggle array of sentiments. . . . When younger subjects are cross-questioned, however gently, they are ready to reverse themselves even on issues they seem to feel strongly about."[18] But what are the specific components of "cognitive capacity" which become the building blocks of ideology?

One of the cognitive skills indispensable for ideological thought is the ability to think causally. In order for a political ideology to grow, the individual must be able not only to see the interrelations of social events and personalities, but also to arrange such events and personalities in meaningful causal sequences. Otherwise, few ideas in his repertory will exhibit constraint. Secondly, the individual must believe that the political world is malleable, for otherwise there is no motivational basis for an activating political ideology. Finally, the person's understanding of events, his scheme of political causation, must be at least partially capable of communication to others. Developed political ideologies, such as liberalism and conservatism, are *shared*. They are based upon principles of causation which have been communicated and understood consensually. To sum up, the individual must be able to reason from cause to effect. The causes of effects in the political world must seem partially and/or potentially under human control, and the principles by which causes and effects are linked must be transmissible.

An examination of the reasoning processes of young children indicates clearly how much learning and maturation must transpire before these requirements can be met. According to Piaget and his followers, the young child is incapable of causal thought as we have described it.[19]

Civilization (New York: Simon and Schuster, 1957).

[17] Albert Bandura and Richard H. Walters, *Social Learning and Personality Development* (New York: Holt, Rinehart, and Winston, 1963), p. 25.

[18] Joseph Adelson and Robert P. O'Neil, "The Growth of Political Ideas in Adolescence: The Sense of Community," unpublished, n. d., 29.

[19] The most important sources for this aspect of Piaget's thought are Barbel Inhelder and Jean Piaget, *The Growth of Logical Thinking*, trans. Anne Parsons and Stanley Milgram (New York: Basic Books, 1958); and Jean Piaget, *The Language and*

Instead, the child reasons precausally until the ages of 10–13. What are the characteristics of precausal thought? Laurendeau and Pinard have isolated and examined four major forms of this childish reasoning: realism, artificialism, dynamism, and animism.[20]

According to Kohlberg, realism is "the confusion of subjective phenomena with objective things."[21] For example, young children who are asked the origins and spatial placement of such subjective phenomena as dreams report that "the events that occur in the dream have an origin external to the dreamer and also take place in front of him. . . ."[22] The child does not understand that he has created the objects he sees in the dream. The implications of this thought process are important. Because every child has a somewhat idiosyncratic subjective world, each child also has a different "real" world. Therefore, children are unable to communicate consensually enough to develop shared ideologies. Furthermore, because all phenomena are viewed as objective, the child finds it difficult to realize that some events need not be imposed upon him. He has no sense of the effects his own efforts or his own ideas might have on the world. Because it is unalterable by human effort, the world has no place for the judgments that human beings might make to change it.

In addition, the child attributes much natural and social phenomena to artifical and supernatural, hence unalterable, forces. For example, the child explains that the coming of night is not a natural, but a supernatural process.[23] The child also states that the clouds are pushed by God.[24] Unseen supernatural forces with human-like personalities manipulate the world. This childish attribution of causation to supernatural forces functions to remove many matters of choice from human control and to render the construction of a political ideology virtually impossible. Furthermore, inevitably, complex interactions of events are reduced to short and distorted sequences. The many subtleties of the transition from day to night are eliminated by invoking a personalized supernatural actor.

But the child is inconsistent. Laurendeau and Pinard also report that children often consider the movement of inorganic objects to be self-generating.[25] Why? Because movement or dynamism is taken as a sign of life. Therefore, when objects change their state, they are either directly propelled by God or by their own unregulated desires. Causation is either wholly supernatural or wholly personal. It is never natural or social. Even some college students mistake the movement of an object for a sign of life. Dennis found that many college students in his sample believed that the sea is a living organism because it moves.[26] A belief in the personal autonomy of objects and individuals except when they are subject to God's will is incompatible with the knowledge that political processes can alter events.

The unifying and summarizing principle of these precausal tendencies may be called *animism,* the tendency for the child to explain all things anthropomorphically. As Laurendeau and Pinard explain, the child attributes some form of life to virtually all phenomena.[27] Even inanimate objects have unique personalities which are peculiarly free of physical limitations or laws. Furthermore, his beliefs about the behavior of individuals become the child's tools for explaining the actions of social institutions. Adelson and O'Neil note that young children, unable to conceptualize such impersonal terms as "government" and "society," reduce such abstractions to imaginary persons able to determine their own destinies.[28] In a world in which people and things are either entirely free or entirely subjected to divine control, no intervention based on personal conviction or understanding is possible. There are no natural or social cause-effect linkages. Consequently, there exists no cognitive base on which political ideologies can be constructed.

Precausal thought in the child is accompanied by what Piaget calls egocentrism. As Piaget describes it, childish egocentrism takes two interrelated forms, personal and logical. Personal egocentrism is well illustrated by Piaget's description of childhood argumentation. The youn-

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Thought of the Child (Cleveland and New York: Meridian Books, 1955). For an extensive review of the pertinent literature and a helpful bibliography, see Monique Laurendeau and Adrien Pinard, *Causal Thinking in the Child* (New York: International Universities Press, 1962), chaps. 1–4.

[20] *op. cit.*

[21] Lawrence Kohlberg, "The Development of Children's Orientations Toward a Moral Order: I. Sequence in the Development of Moral Thought," *Vita Humana,* 6 (1963), 11–33, 18.

[22] Laurendeau and Pinard, *op. cit.,* 107.

[23] *Ibid.,* 161.

[24] *Ibid.,* 188–192.

[25] *Ibid.,* 192–196.

[26] Wayne Dennis, "Animistic Thinking Among College and University Students," *Scientific Monthly,* 76 (April, 1953), 247–249. Other sources cited therein and in Laurendeau and Pinard are particularly useful on this topic.

[27] Laurendeau and Pinard, *op. cit.,* chap. 9.

[28] Adelson and O'Neil, *loc. cit.,* 8–9.

ger the child involved in debate, the more likely he is merely to assert and reassert his own position. Rarely does he confront and deal with the views of his opponent. Nor does he attempt to prove his own contentions or to probe his opponent's logic. The child is encased in his own world[29] Such egocentrism in discussion may be traced to an even earlier tendency. Piaget observes that very young children go through a prolonged period of egocentric play and speech before any habits of cooperation arise. Although several children may be playing in the same area with the same toys, they generally remain relatively unconscious of each other. Nor are they able or disposed to adopt a common play pattern.[30] The expectation of cooperation, of abiding by rules of conduct, and of observing rules of thought is slow in arriving. Childhood play, talk, and debate are variants of a single monologue.

Egocentric logic is defined by Piaget in the following way:

"Egocentric logic is more intuitive, more 'syncretistic' than deductive, i.e., its reasoning is not made explicit. The mind leaps from premise to conclusion at a single bound, without stopping on the way. . . . Little value is attached to proving, or even checking propositions. The vision of the whole brings about a state of belief and a feeling of security far more rapidly than if each step in the argument were made explicit. . . . Personal schemas of analogy are made use of. . . . Visual schemas also play an important part. . . ."[31]

In short, the child intuits the meaning of things without ever laying bare to others or to himself the principles by which his meanings are derived. It is, therefore, not surprising that he should make the sorts of logical and causal errors which hinder the development of political ideologies. Furthermore, his personal egocentrism prevents him from gaining consensual validation for his logic, faulty though it be. His thought, consequently, remains unexamined, defective, and idiosyncratic.

To be capable of ideological thought the child must surmount egocentrism and precausality. He must develop a consensually validated sense of logic and an ability to reason from social cause to social effect. Furthermore, growing ideologically necessitates his recognition that the world of human events is contingent on the behavior of men and social forces, neither of which is entirely beyond human control. Because the earliest stages of human thought are not conducive to such ideological development, it seems fair to speculate, accepting the Freudian position on the psychological dominance of earliest modes of thought, that adult political ideologies always rest on an unstable base. We shall explore the implications of this conclusion shortly.

Many of the principles of cognitive development which we have described apply, with some modifications, to the course of moral development. While in the area of cognition the child views the world as given and unalterable by human intervention, in the moral realm he finds it impossible to account for the origins of rules, regulations, or moral standards except by reference to history. When, for example, Piaget asked his youthful subjects to explain the origins of the rules which govern the game of marbles, the children replied that the rules had existed from time immemorial and were simply passed on unchanged from one generation to the next. The children could not imagine the possibility of their or anyone else's changing the rules.[32] It is easy to see what function is performed by attributing all rules and judgments to historical figures or processes. The rules, having weathered the test of time and having been sanctified by parental transmission, need not be questioned. Therefore, there exists no impulse to the formulation of political ideologies.

Because the moral standards which govern the world are given, he who violates them, regardless of motivation, is guilty. Young children evaluate rule breaking primarily in terms of the objective material effects of violation, rather than in terms of the motivation of the lawbreaker or his relation to the law. It is the degree of injury done, not the motivation of the lawbreaker or the facts of the case, which determines the extent of punishment. The world is viewed as a sort of moral machine in delicate equilibrium which, if disturbed, must be set right by the proper amount of retribution and expiation. "An eye for an eye" is the principle which insures the moral order of the world.[33]

But how is this tribute to be exacted? Again, we find in the child's answer to this question a method of removing from human agencies the necessity to make a choice and justify the choice one made. Human beings do not exercise retribution; rather, the rules themselves and the objects which they protect become the agents of punishment. The child's credo is "immanent jus-

[29] Piaget, *op. cit.*, pp. 45–46.

[30] Jean Piaget, *The Moral Judgment of the Child* (New York: The Free Press of Glencoe, 1965), p. 45.

[31] Piaget, *The Language and Thought. . .*, p. 66.

[32] Piaget, *The Moral Judgment. . .*, pp. 50–65.

[33] It is this tendency on the part of the child which Piaget has labeled "moral realism." *Ibid.*, pp. 109–197.

tice," which may be defined as a belief in "the existence of automatic punishments which emanate from things themselves."[34] Piaget, for example, discovered that children explained accidents to people who had earlier violated a rule by attributing the accident to the violation.[35] All disruptions of the law produce their own remedies, much as in classical Greek tragedy the Furies seek out and punish any man who presumes to challenge the natural order. The child is a great *aficionado* of the "deus ex machina."

This view of the moral world, which lifts the burden of choice from the child, is based upon a belief that laws and moral codes are objective.[36] The child conceives that moral regulations are set down clearly and unalterably within the world itself and, further, are understood consensually by all. There can be no conflict of interpretation nor failure of understanding. Therefore, if an individual violates the law he cannot defend himself by citing mitigating or idiosyncratic circumstances. The child has no sense of the relationship between his own mind and the law, nor does he see that rules are the creation of individuals and, therefore, can be modified by individuals.

It may be conjectured that the child's belief in the invariance and immanent justice of the moral order is caused by his total dependence on his parents. The human child is much more dependent on parents for a longer time than are the offspring of other species. In some primitive way, the child may compensate for this vulnerability by proclaiming the ultimate rectitude of those who control him and who are, at the same time, the symbols of history and rules—his parents. This universal tendency may become exaggerated, however. Piaget postulates that the child's natural glorification of history may be reenforced by parental resort to arbitrary commands.[37] I shall have more to say about this problem momentarily.

As maturation proceeds, the child emerges from his early conception of morality. He acquires a more subjective, more contingent view of rules, morality and transgression. This new position requires him both to investigate the motives of those who do not conform to the law and to vary his judgments accordingly. His early emphasis on punishment is replaced by a consideration of equity. Retribution and expiation give way to distributive justice. By adolescence rules are accepted only in relation to the purposes for which they are intended. For example, the adolescent understands that the rules which govern the game of marbles can be manipulated for the transitory enjoyment of the players.[38] Curiously enough, this transition may be illustrated especially well by the deviant case of psychopaths. Stephenson discovered that adolescent psychopaths, who had obviously been retarded in development, were actually *more* punitive in their judgments of other people's transgressions than were normal adolescents.[39]

Not only does the child develop a relativistic sense of morality, but he also gradually internalizes his own moral norms. The apparent inconsistency between these two processes can be dispelled quickly. The child's reliance upon sacred authority or history as the source of morality and rules, coupled with his belief that moral standards are automatically enforced, relieves him of any need to develop a conscience of his own. Conscience, on the other hand, grows only when the child desires an internal compass for the evaluation of a newly complex, contingent world. Indeed, the child's early belief in the external regulation of the moral order actually frees him to indulge in erratic behavior and inconsistent judgment. As Piaget reports, echoing Converse's observations about the political statements of non-ideologues, "It may even happen that one and the same child judges sometimes one way, sometimes the other."[40] Because the world is itself reliable and can substitute its own controls, the child does not need the judgmental consistency which conscience provides him. But once the world is seen as morally contigent and risky, the child must formulate his own rules and be able to defend them against other people's standards. The child thus moves from a "punishment and obedience orientation" to a "morality of individual principles of conscience."[41]

Cognitive and moral development are closely linked. Cognitive growth produces the realization that there are controllable human and social forces which have effects. Neither willful inanimate objects nor an invariant divine order determines the character of events. These discoveries enable the child to recognize the existence of moral variety and situational contingency. In turn, these latter realizations provide

[34] *Ibid.*, p. 251.

[35] *Ibid.*, pp. 251–263.

[36] *Ibid.*, pp. 121–163.

[37] *Ibid.*, pp. 101–197. For a recent attempt to test this hypothesis in the area of childhood attitudes about political authorities, see Dean Jaros, "Children's Orientations Toward the President: Some Additional Theoretical Considerations and Data," *Journal of Politics*, 29 (May, 1967), 368–388.

[38] Piaget, *op. cit.*, p. 83.

[39] Geoffrey M. Stephenson, *The Development of Conscience* (London: Routledge and Kegan Paul, 1966), p. 43.

[40] Piaget, *The Language and Thought . . .*, p. 91.

[41] Kohlberg, *op. cit.*, pp. 13–14.

the impulse for the development of his own moral standards. On the other hand, the child's newfound desire to internalize his own sense of morality motivates him to inquire into the actual causal structure of events. Before he can make his own judgments about human behavior, he must understand the motivations and social forces which underlie that behavior. Therefore, the development of cognitive skills and the construction of a conscience are reenforcing processes. Laying the groundwork for ideology is all of a piece.[42]

As the citations in this section indicate, much of the theory upon which we have based our description of cognitive and moral development is traceable to Jean Piaget.[43] Before leaving this discussion, therefore, it is important to point out briefly some of the shortcomings of the Piagetian stage-developmental approach. A number of criticisms have been leveled at Piaget's theories and, as a result, some important modifications have already been or are in the process of being made.

Students of child development since Piaget have become well aware of his methodological errors. His theories, which he describes as being universally applicable, are now known to suffer from cultural bias.[44] In addition, not only are his samples all Swiss, but they overrepresent the working class.[45] Nor are their sizes sufficient to generate much confidence.

There is also uncertainty about the fate of those stages through which the child has passed. Some authors hold that new stages replace and erase old, while other authors, such as Sullivan,[46] contend that new stages are terraced on old. If this latter conception is true, no stage would ever be entirely surmounted. Earlier stages would simply remain in a latent form, capable of reinstated impact under special conditions. Piaget was himself inconsistent in his formulations on the subject. At some points he implies that early stages crumble entirely under advanced phases of development, while at other times he argues that stages are only statistical tendencies and never disappear entirely.[47] The implications of this ambiguity will concern us shortly.

There is, finally, much uncertainty about the relative importance of genetic and environmental factors in Piaget's developmental scheme. Piaget attempts to recognize the importance of environment. He argues that aging forces the child into increasingly demanding social relationships, and it is these relationships which are the major intervening variables in the developmental process.[48] But he assumes that the social relationships themselves are constant at every level for every child. Such an argument implies that *all* children, excluding those who have physical and mental defects, should, as adults, manifest

[42] Parenthetically, it is striking the extent to which the development of the child, as outlined in this theory, resembles historical developments over whole eras. For example, we have talked much about the child's shift from an external, but wholly dictatorial form of morality to an internalized conscience. This shift seems to parallel what many scholars believe to be the difference between late Old Testament Judaism and early Christianity. Many scholars have commented upon the emphasis on legalism and expiation in Old Testament Judaism, and the disintegration of codification thereafter. We have no wish to argue seriously that ontogeny does recapitulate phylogeny. We only wish to indicate the parallelism. The young child's belief in ritualism, supernaturalism, the deus ex machina, etc., remind one, however, of the thought processes of "primitive" people. To develop this argument so as to make it more palatable to the reader would require knowledge and space not available to the author. The general idea seems worth pursuing, however.

[43] Piaget's immense corpus of work is only now having a major impact in the United States. For an enjoyable introduction to his thought, see David Elkind, "Giant in the Nursery—Jean Piaget," *New York Times Magazine*, May 26, 1968.

[44] See Urie Bronfenbrenner, "The Role of Age, Sex, Class, and Culture in Studies of Moral Development," *Religious Education* (Research Supplement), 62 (July-August, 1962), S-3, S-5, S-18. For contrasts between American and English children, see E. M. and M. Eppel, *Adolescents and Morality* (London: Routledge and Kegan Paul, 1966), pp. 157–170. For interesting comparative studies on Senegalese children, see Jerome S. Brunner *et al., Studies in Cognitive Growth* (New York: Wiley, 1966), chaps. 11–13. A well known study which also indicates Piaget's cultural bias is Gustav Jahoda, "Children's Concepts of Nationality: A Critical Study of Piaget's Stages," *Child Development*, 35 (December, 1964), 1081–1092.

[45] Bronfenbrenner, *loc. cit.* For the first empirical study to touch this deficiency, see M. R. Harrower, "Social Status and Moral Development," *British Journal of Educational Psychology*, 5 (1935), 75–95.

[46] For an overview of Sullivan's work, see Patrick Mullahy (ed.), *The Contributions of Harry Stack Sullivan* (New York: Hermitage House, 1952), chaps. 1–2.

[47] For example, Piaget sets only a percentage boundary on thought categories; he also admits that a particular child may evidence thought characteristic of a variety of levels at once. Piaget, *The Moral Judgment . . . , passim.*

[48] *Ibid.*, chap. 3.

similar levels of cognitive and moral development. To accept such a contention would be to undercut the major theoretical problem of this article, the widespread absence of ideology. In fact, of course, adults have widely different patterns of cognition and morality. Therefore, they evidence disparate capacities for ideological thought. Piaget's theory is probably understood best as an ideal type. People must pass through the stages he describes if they are to think ideologically, but most people do not succeed in making the passage. What are the factors which determine whether the process will be completed or not? It is these intervening factors, responsible for inhibiting the maturation process and producing different levels of ideological development, to which we now turn.

III. FACTORS PRODUCING DIFFERENT LEVELS OF IDEOLOGICAL DEVELOPMENT

The major factors which affect the course of ideological development are of two sorts: those relevant to identification and child rearing practices, and those involving the many and complex components of morality and cognition. We will investigate each sort in turn.

Of course, both psychological and psychoanalytic theory have long focused on the character of a child's identification with his parents. Theories about the identification process would not be germane to this paper, however, were it not for evidence indicating a link between positive identification and the development of moral and cognitive skills.[49] It now appears that a positive form of identification encourages the growth of political ideology.[50]

Suggestive recent evidence indicating the interrelationships of identification and ideological development may be found by turning to Keniston's studies of college students. Keniston reports that his alienated and politically apathetic Harvard students felt themselves to be rebelling against their parents and, particularly, struggling to surmount the demands placed on them by their mothers.[51] Elsewhere, Keniston discovers that the majority of campus activists identify strongly with parental authority.[52] He finds that activists are not only more likely than the other students to respect their parents, but also generally enjoy the support of their parents in their political activities. Lipset and Altbach claim that most campus activists are actually following a line of political activity gratefully in imitation of their parents.[53] Clearly, therefore, close identification with parents, at least for these primarily middle class students, forges an important link in the chain of ideological development.

We know that the structure of the family affects the likelihood of successful identification. There is considerable evidence which indicates, for example, that the children of husbands and wives who value the same things are more likely than are other children to identify with their parents.[54] This finding is easily interpretable. It is unlikely that there will be much tension in homes where the parents approve of each other. Furthermore, parents who profess the same values will present a fairly consistent picture of themselves to their children, and will also display a single set of ideals for emulation. In short, parents who approve of each other minimize cognitive and moral confusion in their children.

Unfortunately, however, the effects of most

[49] See the review and codification of the relevant literature in Robert F. Winch, *Identification and Its Familial Determinants* (Indianapolis: Bobbs-Merrill, 1962).

[50] This formulation should not be taken to imply that identification has an unambiguous meaning. Lazowick, for example, has specified at least three major definitions which seem currently in use: 1) acting as if the subject were the same person as his model; 2) imitating the model; 3) introjecting the model's norms and values. Lionel M. Lazowick, "On the Nature of Identification," *Journal of Abnormal and Social Psychology,* 51 (September, 1955), 175–184, 175–176. Bronfenbrenner specifies yet a fourth meaning, i.e., acting in terms of an ideal image of the model rather than on the basis of his actual behavior. Urie Bronfenbrenner, "The Study of Identification through Interpersonal Perception," in *Person Perception and Interpersonal Behavior,* eds., Renato Tagiuri and Luigi Petrullo (Stanford: Stanford University Press, 1958), pp. 110–131, 123. Despite their importance for psychology, these definitional ambiguities do not affect the basic argument of this paper.

[51] Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York: Harcourt, Brace and World, 1965), chap. 6.

[52] Kenneth Keniston, *Young Radicals* (New York: Harcourt, Brace and World, 1968), chap. 2.

[53] Seymour Martin Lipset and Philip G. Altbach, "Student Politics and Higher Education in the United States," in *Student Politics,* ed., Seymour Martin Lipset (New York: Basic Books, 1967), pp. 199–252, 216.

[54] John A. Clausen, "Family Structure, Socialization, and Personality," in *Review of Child Development Research,* v. 2, eds., Lois W. and Martin L. Hoffman (New York: Russell Sage Foundation, 1966), pp. 1–55, 42.

family structure characteristics on identification remain obscure. This is true particularly of such factors as father absence.[55] Consensus comes nearest to existing only on the matter of child rearing patterns as they affect identification.

Within the bounds of psychological theory there have been two major schools of thought about patterns of child rearing in the identification process. One school, populated largely by Freudians, has postulated that identification is the result of a childhood defensive reaction against a highly punitive and physical parental environment. The child, supposedly fearing his severe parents, fantasizes himself retaliating against them. But his retaliation fantasies, because they would bring abnormally severe retribution if acted out, are even more disturbing than the parental environment itself. Therefore, the child tries to repress his rebellion. But his repression can succeed only if he internalizes the values of his parents. The process ends with the child identifying with the agressors, his parents.[56]

Anaclitic theories of identification, which comprise the second school, dispute the truth of these assertions. According to the anaclitic theorists, successful identification is most likely to occur in a warm, tightly knit, relatively permissive family atmosphere. Discipline in such a family proceeds primarily through threats to withdraw love rather than through the imposition of physical sanctions. Anaclitic theorists claim that love and succor encourage the child to reflect on his own behavior and internalize those standards which will prevent withdrawal. Also, according to Hoffman, parents who do not punish their child physically provide him with a model of restraint that he is motivated to emulate.[57] The behavior of parents who discipline by physical aggression, however, encourages the child to believe that internal controls are unnecessary. Therefore, physical sanctions actually encourage rebellion. Anaclitic theorists also claim that physical punishment breeds the primitive, expiatory form of morality which we have described, rather than an internalized set of values.

Students of identification also differ over the autonomy-supervision dimension in child rearing. Theorists of defensive identification argue that the imposition of close parental supervision is necessary for the growth of identification. The anaclitic theorist Robert Sears claims, however, that identification is dependent upon parental willingness to grant the child a measure of autonomy to develop his own personality.[58] The child must be expected to adhere to certain standards, but the parent should not remain hovering over him monitoring his behavior and correcting minor faults.

As the debate indicates, interpreting the effects of varying child rearing patterns on identification has not been easy. Nor has the dialogue been as clear on terms and hypotheses as one might desire. For example, the control relationship between parent and child—as most parents and virtually all children realize—is both multidimensional and subtle. A parent who is demanding in one phase of his child's life may be permissive or uninterested entirely in another. Measures of identification and control capable of dealing with these subtleties have been noticeable mainly by their absence. Not surprisingly, therefore, research linking child rearing practices to identification is not entirely consistent or reliable. However, there seems to be a growing consensus along three dimensions. These dimensions are the mode of punishing children, the warmth of the relationship between parent and child, and the age at which responsible behavior on the part of the child is first expected. Let us investigate these three dimensions as they affect identification, cognitive development, and moral development.

As Hoffman puts it, summarizing the research on punishment and moral development, "The relatively frequent use of discipline that makes some sort of appeal to the child's inner need seems to foster the development of an internalized moral orientation, especially as reflected in

[55] For concise statements of opposing positions, see Winch, *op. cit.*, p. 35, and Joan McCord, William McCord, and Emily Thurber, "Some Effects of Paternal Absence on Male Children," in *Studies in Adolescence,* ed., Robert E. Grinder (New York: Macmillan, 1963), 118–133. For studies indicating the possible importance of father absence to both identification and political ideology, see Keniston, *The Uncommitted . . .* , esp. 113–118; and Dean Jaros, Herbert Hirsch, and Frederic J. Fleron, Jr., "The Malevolent Leader: Political Socialization in an American Sub-Culture," this REVIEW, 62 (June, 1968), 564–578, 573.

[56] Morton Deutsch and Robert M. Krauss, *Theories in Social Psychology* (New York: Basic Books, 1965), p. 158.

[57] For a review of these theories, see Martin L. Hoffman, "The Role of the Parent in the Child's Moral Growth," *Religious Education* (Research Supplement), 57 (July-August, 1962), S-18 through S-33, S-22.

[58] Robert R. Sears, "Identification as a Form of Behavioral Development," *The Concept of Development,* ed., Dale B. Harris (Minneapolis: University of Minnesota Press, 1957), pp. 149–162, 160.

the child's reactions to his own transgressions. The use of coercive measures that openly confront the child with the parent's power, on the other hand, apparently contributes to a moral orientation based on the fear of authority."[59] According to available evidence, the parent who resorts to physical punishment not only provides an aggressive model for his child, but also demonstrates vividly to him the unbridgeable gulf between his own standards and those of the child. Such a parent makes no appeal to the child's own latent moral sense. The use of psychological punishment, such as threatened love-withdrawal, assumes a standard shared equally by the child and his parents. Such a technique accustoms the child to think through and judge the behavior which has disappointed his parents. Physical sanctions also appear to suggest to the child that merely his willingness to absorb pain provides an expiation for wrongdoing. Moral rectitude does not require his own set of values. The child who is, implicitly, treated as an adult temporarily unable to meet shared standards learns to evaluate himself; but the child who is not taken into his parents' confidence and who is punished physically begins to expect society to judge him. An internalized conscience, obviously, is the moral base from which ideological preferences grow. Reliance on external authorities to define what is proper builds no stable moral framework within which political events can be arrayed.

There is also evidence that parental warmth toward children stimulates the growth of a consistent, internalized moral orientation.[60] Of course, an affectionate environment may be viewed partly as an off-shoot of certain discipline techniques, but the warmth of family relationships seems to have its own independent effect. The same principles operating in the punishment dimension seem to obtain here. In addition, however, unlike the children of punitive and distant parents, the children of affectionate parents have good reason to feel conflicted about violating parental standards. The kindness of parents may well seem to demand better repayment. Tensions of this kind may force children to evaluate their own behavior carefully. The habit of self-evaluation is certainly a major requirement for the growth of a stable, internalized moral orientation.

Finally, the time at which responsibility is first expected of the child seems related to the growth of a moral sense. Studies by Grinder[61] and by Whiting and Child[62] show that the child's early assumption of responsibility results in a propensity to feel guilt and to experience remorse after transgression. The principle may be illustrated simply. When, for example, the child is given early responsibility for his own toilet and cleanliness, he is, in effect, on notice that he is to be held responsible for an important part of his behavior. This sense of responsibility encourages him to make autonomous decisions and to think for himself.

To sum up, the optimal child rearing pattern for the growth of conscience appears to combine rapid shouldering of responsibility, the use of psychological discipline, and continuing parental warmth. This pattern is found rarely, with two factors primarily responsible. First, many parents believe that warmth, affection, and "permissiveness" are incompatible with the child's early assumption of responsibilities. They construe permissiveness broadly so as to make it require late and relatively lax training in fundamental areas. Second, many parents who do attempt early toilet training find it difficult to remain affectionate toward their children when errors occur. Their punitive response often sets a continuing pattern. Hence, early training may well become associated with physical punishment, relatively cold parent-child relations, and considerable anxiety on all sides.

We have less evidence about the effects of differing child rearing styles on the growth of cognitive capabilities than we have of their influence on moral development. However, some experimental findings permit speculation. Solley and Murphy report that punishment by the use of electric shock disrupts the ability of experimental subjects to distinguish between figure and ground.[63] Such findings suggest that large doses of physical punishment may disorient the conceptual apparatus which permits the individual to make the elementary perceptual distinctions necessary for ideological thought. Physical punishment concentrates the subject's mind on only those of his acts which have painful consequences, thereby depriving him of varied sensory stimulation. The subject focuses primarily on his suffering, rather than on the world around him. This sensory deprivation effect may be thought of as a kind of psychological "tunnel

[59] Hoffman, *op. cit.*, S-24.

[60] *Ibid.*, S-25.

[61] Robert E. Grinder, "Parental Child-rearing Practices, Conscience, and Resistance to Temptation of Sixth-Grade Children," *Child Development*, 33 (December, 1962), 803–820.

[62] John W. M. Whiting and Irvin L. Child, *Child Training and Personality* (New Haven: Yale University Press, 1953), pp. 254–258.

[63] Charles M. Solley and Gardner Murphy, *Development of the Perceptual World* (New York: Basic Books, 1960), p. 274.

vision." As Solley and Murphy indicate, sensory deprivation may become associated with autism, the pathological rejection of environmental stimuli.[64] The autistic person finds the world dull, undifferentiated and, ultimately, unworthy of attention. In short, the excessive use of physical punishment may reduce both the desire and the ability to view the outer world in all its richness. In non-experimental contexts, such as concentration camps, observers have noted similar effects.[65] The relevance of this discussion both to child rearing and to the growth of ideological thought needs no elaboration.

We referred earlier to the debate over whether child development proceeds via a substitution of entirely new phases for old or by a terracing of new stages over latently powerful older, more "primitive" stages. If we assume that the former process operates, non-adoption of the optimum child rearing process which we have described may fixate the child at a particular stage of development. But if we assume that development proceeds regardless, imposition of punishment or threat may still cause at least temporary regression to a more primitive behavioral stage. In the latter case, we would also expect that an adult whose early development had been disrupted by faulty child rearing would be especially prone to regress under imposition of punishments identical or symbolically equivalent to those he experienced as a child. Experiments on both humans and lower animals provide some interesting illustrations.[66]

Let us examine the fixation effect first. Kleemier reports that rats who have been frustrated by the use of electric shock develop severe response rigidity. Not only do they find it difficult to respond to new demands, but even after shock is removed so as to open up new options, the rats cleave to old behavioral patterns which will not reward them.[67] Frustration induced by severe punishment glues them to a particular developmental level. Comparable behavioral and perceptual rigidity in humans would cut down the range of attention necessary to interpret the political world ideologically.

Frustration, no matter what its source, has its own fixation properties. Maier reports on a variety of experiments which indicate that frustrated rats and humans settle on a few patterns of behavior, often the ones least functional, and remain with them. Rats, for example, resort to "symbol stereotypy," an invariant but unrewarded response to a specific sign.[68] Their behavior becomes almost purely ritualistic. Lawson and Marx show that frustration retards perceptual acuity, problem solving abilities, and learning.[69] In short, both excessive physical punishment and frustration rigidify behavior and retard cognitive development. These effects persist even after the cessation of frustration and physical punishment. They inhibit the growth of an ideological framework responsive and alert to new pieces of information.[70]

The psychodynamic process by which fixation occurs may have as its core the experience of anxiety. Sullivan, for one, believes that the onset of anxiety can be traced either to psychological distance from the mother or to excessive punishment. He also claims that anxiety inhibits moral and cognitive development.[71] Odier describes the close relationship between the experience of anxiety and consequent primitive, magical, and animistic interpretations of events.[72] The obsessive, repetitive, ritualistic thought patterns of anxious people are similar to, though more elaborate than, the early forms of thought in the

[64] *Ibid.*, p. 62.

[65] Bruno Bettleheim, "Individual and Mass Behavior in Extreme Situations," *Journal of Abnormal and Social Psychology*, 38 (1943), 417–452.

[66] Some may question this introduction of data drawn from studies conducted on lower animals. I have used this material not because I think it definitive (I would not even argue the point), but precisely because it is illustrative. Were I to attempt a definitive argument on this point, I would naturally rely wholly on studies of humans. It is, however, possible that the same reactions which exhibit themselves in purity with lower animals also occur in humans, but are masked or distorted. We ought not to assume that the responses of lower animals and humans are incomparable. They may be different in expression, but not in substance.

[67] Robert Kleemier, "Fixation and Regression in the Rat," *Psychological Monographs*, v. 54, #4, Whole No. 246 (1942), 13.

[68] Norman R. F. Maier, *Frustration: The Study of Behavior Without a Goal* (Ann Arbor: Ann Arbor Paperbacks, 1961), p. 28.

[69] Reed Lawson and Melvin H. Marx, "Frustration: Theory and Experiment," *Genetic Psychology Monographs*, 57 (March, 1958), 393–464, 437.

[70] I do not contend that psychological modes of punishment may not be frustrating. But physical intervention to block desired behavior probably is more frustrating than psychological intervention.

[71] Mullahy, *op. cit.*, p. 34.

[72] Charles Odier, *Anxiety and Magic Thinking*, trans., Marie-Louise Schoelly and Mary Jane Sherfey (New York: International Universities Press, 1956), pp. 52–53. Odier's work is one of the few attempts to bring together Piaget's and Freud's perspectives on human development.

child as Piaget and his followers have described them.

If we prefer the terracing rather than the substitution theory of development, we can also marshal illustrative evidence showing that frustration, physical punishment, and anxiety may cause behavior to regress, at least temporarily, to a primitive stage. In a famous experiment, Barker, Dembo, and Lewis found that children who were permitted to see but were forcibly prevented from playing a second time with some desirable toys regressed. Specifically, their behavior became disorganized and their physical movements undifferentiated.[73] Barthol and Ku demonstrated that college students reacted to task-induced frustration by returning to their first learned successful task behavior, even when such behavior was no longer appropriate.[74] Adelson and O'Neil discovered that when children became angry or anxious, there was a "momentary cognitive regression, expressing itself in a loss of abstractness, and a reversion to personalized modes of discourse."[75] The terracing theory would predict that even persons normally capable of expressing a political ideology would, under stress, regress cognitively and morally, becoming at least temporarily unable to think ideologically. Any such reversion, as Odier points out, is likely to be characterized by special attention to the symbolic aspects of experience rather than to the factual, by a disposition to make absolute, rather than contingent, judgments about events, and by a willingness to rely heavily upon supernatural "signs."[76]

To sum up, the frustrations and anxieties which inadequate child rearing methods produce may inhibit the identification process and prevent the growth of cognitive and evaluational skills sufficient for the development of political ideology. Furthermore, the imposition of excessive physical punishment and frustration on normally ideological people may induce a regression to less ideological conceptual frameworks. These remarks have obvious implications for understanding the paranoic and disorganized behavior of individuals during political crises.[77]

The second class of variables which influences ideological development includes the components of the moral and cognitive dimensions themselves. Two such components are the sense of time and the ability to handle language. To think ideologically requires the capacity to link present trends to preferred future states or past experiences. Therefore, the character of political cognition is partly determined by the experience of time. Although little is known about the development and stability of the consciousness of time, Ames discovered that the younger the child the less likely he is to have a conception either of past or future.[78] The very young child lives in an eternal present, attaining a sense of future by age three and a sense of past only by age three and a half.[79] The most natural and earliest mode of viewing the world is in the present. Child rearing patterns which fixate or cause regression might restrict the child to the incomplete experience of time evident in the early months of life. The effects of traumatic events may alter even the adult's consciousness of time.

Nor is the sense of time important only for cognition. Mischel discovered that children who prefer immediate small reinforcements to delayed larger reinforcements, when compared to children with an expanded time orientation, scored extremely low on a scale of social responsibility.[80] Grim, Kohlberg and White claim that ". . . there is substantial evidence relating indexes of moral character to ego-strength factors such as . . . anticipation of future events. . . ."[81]

We would also expect to find in language a second cognitive component relevant to ideological thought. Traditionally, and Ames seconds this conclusion,[82] we have assumed that children first use language concretely and then ab-

[73] R. F. Barker, T. Dembo, and K. Lewin, "Frustration and Regression: An Experiment with Young Children," *University of Iowa Studies in Child Welfare,* 18 (1941). For a reanalysis of this experiment, see John M. Davis, "A Reinterpretation of the Barker, Dembo, and Lewin Study of Frustration and Regression," *Child Development,* 29 (December, 1958), 503–506.

[74] Richard P. Barthol and Nani D. Ku, "Regression Under Stress to First Learned Behavior," *Journal of Abnormal and Social Psychology,* 59 (July, 1959), 134–136.

[75] Adelson and O'Neil, *op. cit.,* p. 10.

[76] Odier, *op. cit.,* pp. 48–49.

[77] The most spectacular tendency involved is, of course, scapegoating.

[78] Louise Bates Ames, "The Development of the Sense of Time in the Young Child," *Journal of Genetic Psychology,* 68, First Half (March, 1946), 97–125, 110.

[79] *Ibid.*

[80] Walter Mischel, "Preference for Delayed Reinforcement and Social Responsibility," *Journal of Abnormal and Social Psychology,* 62 (January, 1961), 1–8.

[81] Paul F. Grim, Lawrence Kohlberg, and Sheldon H. White, "Some Relationships between Conscience and Attentional Processes," *Journal of Personality and Social Psychology,* 8 (1968), 239–252, 239.

[82] Ames, *op. cit.,* 115.

stractly. If this were true, we might postulate that the abstract language ability necessary for ideological thought rests on as unstable a developmental base as does the sense of time. However, Roger Brown disputes Ames' position by arguing that, "The child's vocabulary is more immediately determined by the naming practices of adults."[83] In some cases, as Brown shows, language development may proceed from abstract conceptions to more precise differentiations within such conceptions. At present it only seems safe to conclude, with Brown, that "The vocabulary of a child is not a very direct index of his cognitive preferences."[84] Language development may be sufficiently idiosyncratic to defy generalizations about its relation to ideological thought.

While cognitive and moral development depend on a complete sense of time and an ability to abstract, the actual character of cognitive and moral judgments presents its own problems. Let us use as an example the development of morality, about which we have expended so much effort. Piaget assumed that morality was a unidimensional quality which could be measured accurately by asking children about their likely behavior in situations which presumably tempted them to violate their sense of rightness. However, verbal responses to hypothetical temptations expose only one aspect of the moral sense. Investigators of moral development have utilized at least three other designs. Some researchers have examined the actual behavior of children in situations where they have been tempted to violate moral injunctions. The earliest and most influential studies of this sort were conducted by Hartshorne and his associates.[85] Other researchers have investigated the extent to which children express guilt after transgression. Yet others have asked children to imagine their likely reactions to hypothetical situations in which they are supposed to have transgressed.

Interestingly enough, MacRae shows that intercorrelations along these four dimensions are far from perfect.[86] Consequently, there is reason to believe that conscience is a multidimensional collection of attributes. Complete moral development in a person might be characterized by high correlations between moral dimensions; in fact, high correlations might serve as an operational *definition* of complete moral growth. But such development is hindered by the fact that at least four complex moral dimensions must be mastered and integrated. The child must learn to resist actual temptation, feel remorseful after transgression, resist his own imaginary temptations, and imagine his guilt after fantasized transgressions. Development along these dimensions apparently does not proceed at parallel speeds with identical end-points.

The complexity of cognition and moral judgment raises the chances of cognitive and moral instability. The more capacities to be mastered, the less likely it is that there will be consistency along any single dimension. Evidence confirms the suspicion. The Hartshornes discovered that children who cheated at one time refrained at another, despite the fact that objective circumstances had not changed.[87] More recently, Durkin found that the same child judges instances of a single class of reciprocity problems differently with no apparent rule tying the judgments together.[88] These findings parallel exactly Converse's discovery that as one proceeds from more to less informed publics, persons judge the same political events and objects differently within relatively short periods.[89] Perhaps the explanation of Converse's finding inheres in the complexity and fluidity of those moral and cognitive skills which underlie political judgment. Converse's finding, in other words, becomes less perplexing when we recognize that, for many, political perceptions are underpinned by unstable moral and cognitive senses.

These remarks are not intended to suggest any inadequacy in the picture of development Piaget and his followers have sketched. All we have meant to stress is that both the multidimensionality of moral and cognitive maturation

[83] Roger Brown, "How Shall a Thing be Called?" in *The Cognitive Process: Readings,* ed., Robert J. C. Harper *et al.* (Englewood Cliffs: Prentice-Hall, 1964), 647–655, 651.

[84] *Ibid.*

[85] H. Hartshorne and M. A. May, *Studies in the Nature of Character,* vol. I (New York: Macmillan, 1928). H. Hartshorne, M. A. May, and J. B. Maller, *Studies in the Nature of Character,* v. II (New York: Macmillan, 1929). H. Hartshorne, M. A. May, and F. K. Shuttleworth, *Studies in the Nature of Character,* v. III (New York: Macmillan, 1930).

[86] Duncan MacRae, Jr., "A Test of Piaget's Theories of Moral Development," *Journal of Abnormal and Social Psychology,* 49 (January, 1954), 14–18. Grinder's assertion that by age 11 the various measures of conscience intercorrelate closely seems to represent a minority position. The evidence simply does not support his argument. Grinder, *op. cit.,* 818.

[87] As cited in Hoffman, *op. cit.,* S-29.

[88] Dolores Durkin, "The Specificity of Children's Moral Judgments," *Journal of Genetic Psychology,* 98 (January, 1961), 3–13.

[89] Converse, *op. cit.,* 238–245.

and the rarity of optimal child rearing techniques make development problematic. It is therefore not surprising to discover that so few individuals develop into political ideologues.

IV. POLITICAL SOCIALIZATION AND THE DEVELOPMENT OF IDEOLOGY

While this paper's focus has been on the developmental roots of political ideology, it should by now be apparent why I feel that the theory it explicates also has applicability to the political socialization literature. Indeed, we have already considered a few findings in the socialization area which are interpretable in terms of the developmental factors we have outlined. More data on political socialization can be integrated into this general framework, thereby providing some conceptual unification for a field which, currently, may well be accused of vulgar empiricism.[90] Let us briefly review some socialization literature which our framework subsumes and illuminates.

Two classes of socialization findings fit into our discussion. One set may be understood primarily by means of the developmental sequence presented in Part II of this paper, the other in terms of the retardation or facilitation factors discussed in Part III.

The developmental sequence helps account not only for the growth of political ideology, but also for the oft-noted "authoritarianism" of the child. We can now see that the child's authoritarian view of politics is only an extension of, first, his natural tendency to idealize history and authority; second, his belief in the inability of humans to alter the world; and third, his conception of justice as a self-regulating, mechanical, expiational process. Similarly, the "benevolent leader" syndrome[91] by which the child links himself to the polity may be explained partly by his inability to reason abstractly, to be self-conscious about his thinking, or to relate concrete judgments to general rules. Because the child reasons intuitively and syncretically, he reaches the personal application side of thought very rapidly. Reliance on a benevolent leader is both the outcome and the symbolic representation of a syncretic rather than analytic thought process. The "benevolent leader" syndrome, in other words, is an elliptical and condensed expression[92] of syncretic, personalized, unself-conscious thought. As Adelson, Siegel, and Piaget agree, not until the crucial 11–14 age range do children gain the capacity to reason abstractly and thereby arrange their relationship with the political system on different terms.

The intervening factors which determine the outcome of the developmental process illuminate a second set of socialization findings. We have argued that failures in cognitive and moral development seem uniquely linked to the psychological process of identification. This contention has already permitted us to interpret the perhaps puzzling discovery that student rebels and ideologues are *not*, by and large, also rebels against their parents. This generalization may be extended now to other kinds of political leadership. For example, Malcolm X in his *Autobiography* speaks in glowing and sympathetic terms of his mother, whose tribulations apparently sparked a latent political interest in him.[93] Nor should it be surprising that the children of clergymen seem to be especially prone to political activism.[94] These children, more than others, must incorporate moral norms in order to identify successfully with their parents.

The modalities of intergenerational transmission of party identification—at the heart of much socialization literature[95]—also become more understandable by use of the framework. We can explain why such transmission is easiest in the child's early years, because it is then that the child views the parent as the embodiment of a stable moral order. But we would also predict that when the conditions for identification between parents and children are weakened, the transmission process will similarly be endangered. Jennings and Niemi find, for example, that transmission of party identification from

[90] For a recent article which evidences realization of this problem, see Jack Dennis, "Major Problems of Political Socialization Research," *Midwest Journal of Political Science,* 12 (February, 1968), 85–115.

[91] For a discussion, see Fred I. Greenstein, *Children and Politics* (New Haven: Yale University Press, 1965), chap. 3, and the sources cited therein. Also, Robert D. Hess and Judith V. Torney, *The Development of Political Attitudes in Children* (Chicago: Alldine, 1967), chaps. 2–3; Jaros, Hirsch, and Fleron, *op. cit.*; and Jaros, *op. cit.*

[92] These aspects of childish thought seem similar to "dreamwork" in the psychoanalytic theory of dreams.

[93] *The Autobiography of Malcolm X* (New York: Grove Press, 1964), chap. 1.

[94] The names of Woodrow Wilson, Harriet Beecher Stowe, Martin Luther King, Jr., and Malcolm X come most readily to mind.

[95] See Herbert Hyman, *Political Socialization* (Glencoe: The Free Press, 1959), pp. 69–85; Greenstein, *op. cit.,* chap. 4; Hess and Torney, *op. cit.,* chap. 9; Campbell *et al., op. cit.,* chap. 6; and M. Kent Jennings and Richard G. Niemi, "The Transmission of Political Values from Parent to Child," this REVIEW, 52 (March, 1968), 169–185, 172–174.

parent to child is considerably less successful today than it appears to have been a generation ago.[96] How can this change be explained? It is no secret that the present generation of American families is more plagued by divorce than were its predecessors. In most cases, divorce results in family disintegration, bitterness, and a breakdown of communication between the generations. Children viewing the breakup of their parents' marriage can seize on more than enough reasons not to identify with those parents. Obviously, divorce is not the only factor at work in weakening the identification process; I have used it merely as an illustration. Those many aspects of contemporary life which are responsible for the celebrated "generation gap" no doubt contribute as well.

Of course, a framework's analytic utility should be judged not only by the existing findings it interprets, but also by the originality of the hypotheses it yields. Does our framework pass this test? Let me indicate three kinds of nonobvious questions which our framework leads us to investigate. I shall start with a relatively low-level hypothesis, move through a middle range question, and continue on to what might be called a systemically relevant problem.

First, though I have indicated the importance of identification to the acquisition of a political ideology and have specified some factors which affect the identification process, the specification is incomplete. For example, identification might be related to birth order because, as Stephenson points out, ". . . the higher the birth order of the child, the more time will the parents be able to spend with the child."[97] Assuming Stephenson is correct, we would predict that firstborn children would be especially likely to become political ideologues. But we should also expand and qualify this hypothesis somewhat. It is proverbial that not only first but also last-born children get heavy doses of parental attention. It is true that for a time the oldest child has exclusive claim to parental interest and, often, to parental hopes, but the last child, normally coming relatively late in the reproduction cycle, often receives a tenderness and warmth not afforded his brothers and sisters. To his parents he may seem nature's going-away present. Therefore, intermediate children may be less likely to identify with parents and, consequently, less likely to become political ideologues.

A middle range hypothesis takes up the differing and inconsistent moral standards evidenced by most children. We have already noted the relationship of these findings to the adult ideological inconsistencies uncovered by Converse. This parallelism indicates the need for a searching inquiry into the variety of moral standards people employ in their perceptions of political events. Are different standards applied to different classes of events, institutions, or personalities? Are there ways of predicting or describing the standard in use for any particular perception? Until now, most investigations of public opinion have generally assumed, very superficially, that people employ a single judgmental dimension in evaluating politics. Only Lane's work has described fully the many moral standards applied to politics.[98] Our framework leads us to hypothesize that single moral standards and judgmental consistency will be applied primarily by people whose parents were warm to them, accorded them responsibility early, and disciplined them psychologically. Other child rearing patterns will lead to a fragmented view of the political world.

Finally, at the systemic level, we may investigate the intertwining of moral and cognitive development. I argued that moral and cognitive progress reinforced each other. The need to evaluate the world gives rise to the desire to uncover the causal patterns *in* the world, and vice versa. I also implied that impairment of development affects cognition and evaluation equally. But there are certainly *uneven* rates of moral and cognitive development. What might be the outcome if, for some reason, a person's cognitive development proceeded faster and more smoothly than his moral development? Though there is currently no way by which to explain such aberrant processes, we can at least hypothesize about effects. For example, we might predict differing occupational patterns for people with different balances of moral and cognitive abilities. Children whose moral development exceeds their cognitive development might be attracted to occupations where moral questions are central. Such people might enter religious

[96] Compare *ibid.* with Hyman, *op. cit.*, p. 74.

[97] Stephenson, *op. cit.*, p. 112. For evidence indicating the superiority of first-born children over middle children in two other spheres—occupational and educational achievement, see Peter M. Blau and Otis Dudley Duncan, *The American Occupational Structure* (New York: Wiley, 1968), pp. 307–308.

[98] Lane, *op. cit.* For an excellent empirical description of one kind of ideological structure, with moral standards at the forefront of the discussion, see Leonard Berkowitz and Kenneth G. Lutterman, "The Traditionally Socially Responsible Personality," *Public Opinion Quarterly*, 32 (Summer, 1968), 169–186.

careers. On the other hand, children whose cognitive capacities outstrip their moral faculties might be attracted to occupations requiring advanced cognitive skills but little moral judgment. Might this be true of mathematicians? Finally, what mix of moral and cognitive development will lead a child to the political life, and might this mix differ from culture to culture?

So much for the organizing and heuristic values of this framework. The argument developed here has several specific implications for the understanding both of political structures and of ideology—implications which I wish to highlight in conclusion. Assuming, as I have, that early learning and conceptualization processes have lasting impacts, the sequence of development favors some political movements and regimes over others. Specifically, movements of the left rest on a less secure psychological base than do movements of the right. Left movements, in their call for innovation and their emphasis upon the secular over the sacred and equity over expiation, appeal to high levels of moral and cognitive development. Movements of the right stress the need to respect authority, tradition, and punitive law. In so doing, they appeal to the earliest inculcated and most "natural" forms of thought. Therefore, more people are capable of reaction than reform.

For these reasons the revolutionary cycle from reform to reaction, which Brinton describes in structural terms,[99] may also be understood as a psychological problem. It is difficult for the mass of people to sustain commitments to left movements. Therefore, after such movements have succeeded in redistributing wealth or property, the less ideologically sophisticated adherents become apathetic or even antagonistic. As a consequence, leadership passes from agitators to colorless administrators or ruthless pragmatists. The maxim, "The revolution swallows its own children," is not entirely accurate. Rather, the children of the revolution defect, and can be brought back only by coercion. This formulation need not imply, however, that reactionary movements are invariably successful. Movements of the right in democratic regimes are forestalled both by leaders whose psychological capacities allow them to maintain a commitment to gradual reform, and by the apathy, inconsistency, and material self-interest of mass publics.[100]

We can also understand how difficult it is to meet the psychological requirements for a stable democratic regime.[101] Democracy demands much with its emphasis on openness, flexibility, gradual reform, progress through secular endeavor, and tolerance for those on the margins of society. Most people do not reach a high enough level of moral or cognitive development to maintain a long-run commitment to such a system. Either democracy must support itself by assuring material well-being for most of its citizens[102] or it must extract commitment at the cost of developing in its people a sense of nationalism. Both forms of support may eventually come into conflict with the norms of the system itself. We are led to the conclusion that diverse kinds of democratic commitment, resting as they do on different modes of perception and evaluation, provide considerable potential for fission and fragmentation. As Lasswell argued long ago, insufficient cognitive and moral development constitute continuing threats to democracy.[103]

We have also considered evidence and theory suggesting that individuals may slide between levels of political perception depending upon events. The implications of this discovery are far-reaching, and take two forms. First, we need to identify both the characteristic processes by

[99] Crane Brinton, *The Anatomy of Revolution* (Englewood Cliffs: Prentice-Hall, 1965).

[100] For evidence on these characteristics of mass publics, see Samuel Stouffer, *Communism, Conformity, and Civil Liberties* (Gloucester: Peter Smith, 1963); Herbert McClosky, "Consensus and Ideology in American Politics," *Political Opinion and Electoral Behavior*, ed., Edward C. Dryer and Walter A. Rosenbaum (Belmont, Calif.: Wadsworth, 1966), 236–267; James W. Prothro and C. W. Grigg, "Fundamental Principles of Democracy: Bases of Agreement and Disagreement," *Journal of Politics*, 22 (Spring, 1960), 276–294; Bernard R. Berelson, Paul F. Lazarsfeld, and William H. MacPhee, *Voting* (Chicago: University of Chicago Press, 1954), chap. 14. For a recent critique, see Peter Bachrach, *The Theory of Democratic Elitism* (Boston: Little, Brown, 1967).

[101] A study which addresses this problem is Harry Eckstein, *A Theory of Stable Democracy* (Princeton: Center of International Studies, 1961).

[102] Seymour Martin Lipset, *Political Man* (Garden City: Anchor Books, 1960), chap. 2.

[103] Harold D. Lasswell, "Democratic Character," in *The Political Writings of Harold Lasswell* (Glencoe: The Free Press, 1951), pp. 465–525. For a recent discussion of these same problems, see Fred I. Greenstein, "Personality and Political Socialization: The Theories of Authoritarian and Democratic Character," *The Annals*, 361 (September, 1965), 81–95.

which people shift and the circumstances which contribute to such movement. Edelman explores the mechanisms by which feared or experienced sanctions can cause regression to less advanced modes of political perception.[104] But political crises of a magnitude sufficient to cause widespread severe regression rarely occur. We should concentrate, therefore, on the process by which less traumatic but more frequent political events induce movement between cognitive and evaluative levels. At the very least, as Converse's discussion demonstrates,[105] there is need for periodic replication of all attitude studies.

Second, we should not ignore the effects of our making attitudinal inconsistency and slippage a primary assumption in the study of politics. Accepting such a premise makes our tasks doubly difficult. Many of our most cherished findings, not only from the "behavioral" sides of political science—such as public opinion and electoral behavior—but also from the more institutional areas of the discipline, become problematic. Still, these retrenchments seem required if the foregoing argument is persuasive.

[104] Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1964), chap. 9.

[105] Converse, *op. cit.*

EXPANDING THE POLITICAL PRESENT

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"Nothing we have created, in politics or literature, is necessary...."

—RICHARD POIRIER

Theory expressing new dimensions of man's political predicaments has rarely emerged from well-accredited academic quarters. Yet political scientists have come to hope that their own discipline, imaginatively pursued, might nevertheless yield comprehensive theories for coming to terms with man's present and prospective situations. To cope with emergencies—if not those of the year 1984 then those of the now more fashionable year 2000—they have proceeded to delineate possible futures. It is hardly necessary to document how industriously they have been extrapolating from the present, becoming interested in simulating, linear programming, gaming, and projecting into the future. With methodological ingenuity and resourcefulness, practitioners of futuristics have provided a new literature which is unconventional, exciting, and at times intriguingly surrealistic.[1] Like Bentham's and Saint-Simon's more buoyant pieces, it has all the trappings of authentic radical thought: it appears to challenge the prevailing intellectual and institutional boundaries.

Yet in view of the ideological constraints inherent in every professional discipline, it would be surprising if the new blueprints and scenarios were to inspire men to transcend the present political order, if the new utopians (to use Robert Boguslaw's apt label) acknowledged something other than the prevailing system of psychological, economic, and industrial norms as their point of departure. They understandably begin with the present, that which *is*. They perceive what various forms of systems analysis have shown to exist: man as egotistical utility and power maximizer, public policy as interest group inputs, the economic sector as primary generator of community goods, governmental structures as hierarchical organizations, politics as a sacrifice of personal values, psychological and economic resources as scarce, and development as whatever leads toward the fulfillment of this empirically confirmed vision. Notwithstanding the criticisms of Peter Bachrach, Robert Paul Wolff, Murray Edelman, Graeme Duncan, Irving Louis Horowitz, and others,[2] the new utopians begin with observed experience. And as they proceed, they reveal that not even the injection of temporal concepts into systems analysis—careful references to change, development, or dynamic processes—is sufficient to prevent their work from vindicating what is patently present.

Projections being impossible without some sort of baseline, it may be hard to see how it could be otherwise. Still, we know that what has existence for us depends on what we perceive to be there, on conditions we admit (sorrowfully or cheerfully) to be present. Few would disagree with Kant that "the present" is at least partially the result of intention, decision, and action. It is constituted by our power to designate and signify, the power to establish values.

But such power is not claimed by the authors of those exercises in futuristics which are the most frequently cited.[3] The new utopianism professes no value commitments. To be sure, it favors formal coherence, structural tidiness, and the use of denotative terms. Moreover, it reveals a preference for a rationally controlled future

[1] See especially Robert Boguslaw, *The New Utopians* (Englewood Cliffs, N.J.: Prentice-Hall, 1965); Bertrand de Jouvenel (ed.), *Futuribles* (Geneva: Droz, 1963), Vols. I and II; Olaf Helmer, *Social Technology* (New York: Basic Books, 1966); Daniel Bell (ed.), *Toward the Year 2000* (Boston: Houghton Mifflin, 1968); Andrew Kopkind, "The Future-Planners," *The New Republic* (February 25, 1967), 19–23; William Ewald, Jr. (ed.), *Environment for Man: The Next 50 Years* (Bloomington: Indiana University Press, 1967); Office for Industrial Associates, *The Next Ninety Years* (Pasadena: California Institute of Technology, 1967); Robert Prehoda, *Designing the Future* (Philadelphia: Chilton, 1967); Herman Kahn and Anthony J. Wiener, *The Year 2000* (New York: Macmillan, 1967); Zbigniew Brzezinski, "America in the Technetronic Age," *Encounter*, 30 (January, 1968), 16–26; Stanford Anderson (ed.), *Planning for Diversity and Choice* (Cambridge: M.I.T. Press, 1968).

[2] Critiques of an arrested, excessively narrow empiricism may be found in two collections of readings: Charles A. McCoy and John Playford (eds.), *Apolitical Politics* (New York: Crowell, 1967); and Henry S. Kariel (ed.), *Frontiers of Democratic Theory* (New York: Random House, 1969).

[3] The best example is the work by Kahn and Wiener, *The Year 2000*.

over one which might merely happen. But its practitioners refrain from discussing the valuation implicit in their concern for the integrity of one set of contemporary phenomena as opposed to others. The constitution of the present, they assume, is valid, or at least given. For them, "the present" is not so much a concept as a benign state of being. For them, there *is* a present to which their charts and words presumably give full credit. Because they do not see themselves as creatively attaching names to experiences, as accrediting institutions and behavior patterns, they simply treat as present whatever is fortunate enough to have been caught in their net of concepts. The present virtually presents itself, and the new utopians turn out to be no more than attentive, solicitous representatives of established reality. Playing this passive role, they seem to make no decisions. In the name of science, they behave like primitive man for whom, as Wendell Bell and James Mau have noted, "ends were seldom questioned and means were often invested with ritualistic significance."[4] Giving expression to a seemingly autonomous process of history, they do not appear to act either in favor of some aspect of reality or against an infinity of dormant alternatives. They do not appear to discriminate against those parts of the past (and the future) which remain buried in the present moment. The new utopianism, it would seem, is altogether "realistic" and "empirical."

I

Reifications serve to restrict politics to the few. Less epigrammatically, to identify reality exclusively with such abstractions as "balance of interests," "national honor," "private property," or "industrial man" is to acquiesce in the policies of those in society who have the power to create reality, who are free enough to structure man's consciousness of space and time. Moreover, to assume the finality of experiences which might yet change is to betray a commitment to *de facto* conditions—and to those who benefit by them. Such reification—and its consequent acceptance of the exercise of political power by the few—becomes insidious when done in the name of realism and science: it degrades the unperceived part of reality and the whole of science.[5]

Insofar as the new utopianism eliminates alternative realities by embracing as "real" the very institutions which the social sciences properly subject to continuous criticism, it is anti-empirical as well as elitist. When it fails to acknowledge the problematic—not to say grotesque—character of the present, it is unable to specify how men are kept underdeveloped by the dominant order of commitments: government by a plurality of elites, a functional division of labor, the distinction between private and public sectors, the system of fixed social and biological roles within hierarchical organizations, the market economy, the identification of security with military power, the achievement orientation, the separation of means from ends, and finally the organized repression of action and pleasure, of life and death, of politics and play.

There is so little doubt that these commitments and the institutions which implement them are functional—signs of good sense and maturity—that their explicit justification would seem to have become dispensable. Action to establish alternative realities is thus discredited without discussion: it is *a priori* "unrealistic"—unhinged from experienced reality. Moreover, authentic utopian speculation comes to be seen as irrelevant for scientific analysis. As frameworks for analysis scrupulously exclude unrealized norms, they are attached to the status quo, treating reactions to suffering or anxiety as malfunctions of existing systems—be they the systems of production, distribution, recreation, education, communication, transportation, law enforcement, riot control, medical service, prison management, welfare, or warfare.

That neither cost-benefit accounts nor systems analysis is inherently conservative would seem to be clear enough.[6] But what would be the practical consequence of attempts to overcome their present conservatism have hardly been acknowledged. True, it is noted that a sociocultural system should not be exhaustively identified with an equilibrium of interests; the equilibrium metaphor, it is recognized, fails to account for

[4] Wendell Bell and James A. Mau, "Images of the Future and Research Strategies," manuscript prepared for J. McKinney and E. Tiryakian (eds.), *Theoretical Sociology* (New York: Appleton-Century-Crofts, forthcoming), p. 7. The major premise of this essay is that "modern sociologists have so reified [technologies and organizational settings] that they have neglected the study of the processes involved in social development and change resulting from the actions of men themselves" (p. 34).

[5] Indictments of closure are legion. The charge is committing the fallacies of reductionism, misplaced concreteness, hypostatization, historicism, and essentialism. The record shows how readily science is converted into scientism, an analytical construct into an absolute, a useful tool into a fetish, or a contingency into a certainty.

[6] See Oran R. Young, *Systems of Political Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1968).

man's creative response to tensions. Hence Walter Buckley, for example, has pointed to the need for "a new concept to express not only the *structure-maintaining* feature, but also the *structure-elaborating and changing* feature of the inherently unstable system."[7] He has consequently introduced the concept of "morphogenesis," quoting C. A. Mace's argument for extending the concept of homeostasis so that it might "cover the case in which the goal and/or norm is some state or relation which has never previously been experienced."[8] But this begs the question: how might social scientists remain loyal to their paradigm and express what has "never previously been experienced"? To what reality would their terms refer? How would they have to define reality so that their terms might acquire meaning? Would they not have to *create* experiences so as to come to terms with them, and would this not entail engaging in something more idealistic and hazardous—more active—than systems analysis as currently practiced?

The reluctance to face up to these questions is understandable. That social science should be inclined to maintain public morale and embrace established interests within self-maintaining structures should occasion no surprise: documentation of its readiness to serve is itself a minor industry.[9] If there is nonetheless a disciplined concern today with enlarging options by postulating the unity of theory and action—the inextricable linkage between fact and norm—we should not in any case expect to find it at the center of the social sciences. After all, such a concern would have to take the form of an attachment to potential interests which are all but eclipsed, which are present though invisible.[10] Attempts to represent unexpressed interests by bringing them into the light of day are bound to seem suspect to those who have the resources to see clearly and speak properly. The manners and idiom of spokesmen for the polarities of experience are unavoidably perceived as unseemly, if not incomprehensible. The utopias negatively implied or positively projected, for example, by Herbert Marcuse, Paul Goodman, Kenneth Burke, Wilhelm Reich, Francis Golffing, and Erich Fromm are seen to be irresponsibly conceptualized, expressed without regard for professional etiquette and scientific convention. Their metaphors fail to refer to manifest experience. Their works are out of order. Their vision, in Nietzsche's precise term, is untimely. Their prose discredits certified reality not only explicitly but also by its disconcerting ambiguity. Who can tell the genuine from the false prophet?

In view of these criticisms one can hardly blame either Marx or Wittgenstein (almost unthinkable company) for having declined to speak about worlds (and values) which had not been brought into being. And if their restraint is believed to be sensible, on what grounds can one entertain the desperate negations of Nietzsche, Heidegger, Merleau-Ponty, Marcel, Laing, or Mailer? Before accepting them, should one not wait until their prose has become more chaste and their posture less ambiguous?

Whether we are justified in rejecting efforts to communicate as unbearably eccentric, as pol-

[7] Walter Buckley, "Society as a Complex Adaptive System," in *Modern Systems Research for the Behavioral Scientist* (Chicago: Aldine, 1968), p. 493.

[8] C. A. Mace, "Homeostasis, Needs and Values," *British Journal of Psychology*, 44 (1953), 204–205, as quoted *ibid.*

[9] See Alexis de Tocqueville, *Democracy in America* (New York: Vintage, 1954), Vol. II, pp. 42–49; Thorstein Veblen, *The Higher Learning in America* (New York: Huebsch, 1918); C. Wright Mills, *White Collar* (New York: Oxford University Press, 1951), pp. 129–136. See also Loren Baritz, *Servants of Power* (Middletown: Wesleyan University Press, 1960); Frederik L. Polak, *The Image of the Future* (New York: Oceana, 1961), Vol. II, pp. 169–200; Sol Stein, "The Defense Intellectuals," *Ramparts* (February, 1967); H. L. Nieburg, *In the Name of Science* (Chicago: Quadrangle Books, 1966); Christopher Lasch, "The Cultural Cold War," in Barton J. Bernstein (ed.), *Towards a New Past* (New York: Pantheon Books, 1968), pp. 322–359; James Ridgway, *The Closed Corporatoin* (New York: Random House, 1968); and Noam Chomsky, *American Power and the New Mandarins* (New York: Pantheon Books, 1969).

[10] See Herbert Marcuse, *Eros and Civilization* (Boston: Beacon Press, 1955); Paul and Percival Goodman, *Communitas: Means of Livelihood and Ways of Life* (Chicago: University of Chicago Press, 1947); Wilhelm Reich, *Selected Writings* (New York: Farrar, Straus, & Cudahey, 1960); Francis and Barbara Golffing, "An Essay on Utopian Possibility," *Centennial Review*, 7 (Fall, 1963), 470–480; Erich Fromm, *The Sane Society* (New York: Rinehart, 1955); R. D. Laing, *The Politics of Experience* (New York: Pantheon, 1967); Kenneth Burke, "Dramatism," in *International Encyclopedia of the Social Sciences* (New York: Macmillan, 1968), Vol. VII, pp. 445–452. See also George Kateb, "Utopia and the Good Life," *Daedalus*, 94 (Spring, 1965), 454–473; Denis A. Goulet, "Development for What?" *Comparative Political Studies*, 1 (July, 1968), 295–312; and Henry S. Kariel, *Open Systems: Arenas for Political Action* (Itasca, Ill.: F. E. Peacock, 1969).

itically irrelevant, must depend, however, on available resources. Can we bear to listen? Do we have the capacity for overcoming the manifest present, sustaining new forms of political consciousness, and surviving within an enlarged order of being? These questions may be seen as empirical ones: to what extent is it possible, in practice, to welcome a mode of action, of doing social science, which detaches men from prevailing social fortifications and empowers them to live life more fully? How far can men afford to dispense with the comfort provided by myth, ritual, religion, ideology, and consensus? Can men manage to act once they learn that nature gives no final answer, once they discover that its pervasive ambiguity frees them to choose the constraints which give significance to their lives?

It is easiest, of course, for us to support a view of man as capable of disciplining himself and of constructing his own reality when we insist at the same time that such a view is at best not fully implemented. Thus it has seemed advisable to keep protean man—why settle for less?—out of politics. Anyone who strives to negate the manifest present and design a new reality is accordingly confined to private life, the cultural arena, the realm of art. In private life (in life which some insist *is* private, or at least must be *kept* private) we can tolerate man's creative impulses. But out in the open where men do public business, some higher law—patented as a constitution—is alleged to be fixed and binding. Thus we follow Kant: we free the artist but confine the citizen—and along with the citizen the social scientist.

It may be useful to recall how the Kantian argument that we can have full confidence only in laws made by ourselves served to emancipate man as artist.[11] Prior to Kant, the artist had been confined: his role had been to sustain and illuminate the very nature of things. Expressing what he beheld, his works were to reflect the inherent order of the universe. Kant's *Critique of Pure Reason,* however, made it apparent that whatever order might inhere in the universe, it would surely remain mysterious. Man could no longer presume to say anything conclusively true about reality. He could merely make hypothetical statements—*and put them to the test.* Nature—or what those in power claimed to be nature—might now be made to yield. In the natural world, in what now could become *man's* world to the extent that he could impose on it, man could feel free to make the formulas, myths, designs, languages, ideologies, and laws that satisfied *his* needs. As generations of romantics beginning with Rousseau and the Marquis de Sade made plain, man was now authorized to express himself. The artist thus emerged, free to structure his feelings in the way he gave form to both his life and his work. Reality could be appropriated.

In practice, of course, the artist is kept from enlarging the sphere of his operations. His way is blocked by social prejudice, established institutions, political traditions, governing elites, and public opinion. He is confined to respectable enclaves: the gallery, the concert hall, the *salon,* the theater, the audience of well-bred connoisseurs. Plays are confined to stages, paintings are framed, performances have beginnings and endings. The arts are for special places and special hours, and access is restricted. Apparent exceptions—the decreative, boundary-breaking work of Joyce, Nabokov, Fellini, Borges, or Beckett; open-ended happenings; student rebellions and urban riots; strikes and demonstrations; marches of the poor—are simply not perceived as structured drama, as political projects which create meaning. Such performances are not seen as controlled efforts to break with actuality and display a new reality. It therefore simply does not occur to us that those who partake in them might be engaged in rational action. We fail to recognize that some outsiders might have discovered the theater of Antonin Artaud—not stages on which to resolve conflict and offer answers but stages on which to "express concretely certain secret truths, to bring into the light of day by means of active gestures certain aspects of truth that have been buried. . . ."[12] Failing to see this—failing to see that here might be "victims *signalling* through the flames"—we indiscriminately characterize annoying social events as reflexes, as outbreaks of lawlessness, irrationality, inhumanity, barbarism. They are out of bounds —mindless private usurpations of the public realm.[13]

[11] This paragraph draws on "Society as Crucible," in Helmut Schoeck and James W. Wiggins (eds.), *Psychiatry and Responsibility* (Princeton: D. Van Nostrand, 1962), pp. 135–156.

[12] Antonin Artaud, *The Theater and Its Double* (New York: Grove Press, 1958), p. 70. See also James L. Peacock, *Rites of Modernization: Symbolic and Social Aspects of Indonesian Proletarian Drama* (Chicago: University of Chicago Press, 1968); and Hugh D. Duncan, *Communication and Social Order* (Totawa, N.J.: Bedminster, 1962). Reuel Denney has pointed out to me the relevance of Artaud's conceptions.

[13] For examples of ambivalent concerns about the decline of good form, see Philip Rieff, *The Triumph of the Therapeutic* (New York: Harper

To become anxious about the artist breaking out and engaging in public action it is merely necessary to believe that the totalitarian regimes of the twentieth century were governed not by elites imposing their private visions but by the unleashed masses. The spectres of Nazi Germany or American McCarthyism—reinforced by data about working-class authoritarianism—are enough to inspire fear of the masses.[14] And once one confuses elite-governed regimes with mass society and identifies mass society with barbarism it becomes only sensible to deny public outlets for man's creative impulses. Culture then becomes quite literally private business, and pluralism and constitutionalism are celebrated as the last strongholds of civility. Thus the existing constitutional order—and the economy of motion and emotion it implicitly institutionalizes—emerges as the very embodiment of justice, as an unamendable achievement. It ceases to be regarded as relative to changing human needs, as a design for living which men might change when it obstructs their pursuit of happiness.

Policies, it is true, do continue to be made and unmade in the public sector of constitutional regimes: but it is doubtful that they appreciably affect those fundamental economic institutions which effectively govern the rhythm and texture, direction and quality of everyday existence.[15] Nor are the arenas in the private sector significantly open to political action. To be sure, all manner of men who have been blessed by luck and favorable tax laws may act creatively at the top of the hierarchies of private power. Moreover, scientists and scholars who have escaped scarcity and necessity may explore new space and play their serious games. But genuine latitude is granted only to the clearly identified, narrowly defined artist. Only he would seem to have room to maneuver, to express his enterpreneurial and creative impulses. Authorized to change the forms that constrain him, he above all others may approach convention, law, discipline, authority, or myth in a playful spirit, as forever subject to revision. He is permitted to treat the truly basic questions of style and form as hypothetical, as useful to the extent that they intensify and extend experience. Admittedly, his special sensibilities had initially impelled him to be alienated from the dominant culture. But while he had once rebelled against the bourgeoisie, today he has begun to accept its embraces. More than that, he has been returning the favor, utilizing commercial images and electronic products. Thus the private socio-economic order has been linked with that of art. It is only in the political realm—only in the realm in which man acts as citizen—that theory still denies us the power to create the plans, designs, and concepts to expand reality.

II

The case for extending the range of the artist's operations has hardly received a hearing. Theory may have cleared the ground for action. But it still does not allow for democratizing the impulse to manage one's experiences in public—the political impulse that was expressed at its highest pitch by Nietzsche. On the contrary, the political theory dominant today generally treats man as incapable of managing his interests and assumes a just political system to be one which will account for his allegedly inherent limitations.[16] Although political thinkers from Machiavelli to Kant provided foundations for public action, they never invited the masses to participate. Nietzsche, it is true, did take note of the conditions for self-government; yet he never explored how the mass of men might be enabled to meet these conditions. He failed to ask the question which was to be consistently posed by Dewey: what kind of educational system will enable men to detach themselves from the life-destructive structures of their society?[17]

The obvious strategy for moving ahead is to make *practical* inquiries about environments conducive to democratizing creativity, to expanding politics. This entails raising empirical questions which, because they must be answered

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& Row, 1966); Irving Howe, "The Idea of the Modern," in *Literary Modernism* (New York: Fawcet, 1967); and Edward Shils, "Plenitude and Scarcity: The Anatomy of an International Crisis," *Encounter,* 32 (May, 1969), 37-57. For an approach recognizing the form of seemingly formless behavior, see Murray Edelman, "Public Policy and Political Violence," mimeographed discussion paper, Institute for Research on Poverty, University of Wisconsin, 1968.

[14] See J. L. Talmon, *Origins of Totalitarian Democracy* (London: Secker and Warburg, 1952); José Ortega y Gasset, *The Revolt of the Masses* (New York: W. W. Norton, 1940); and Robert A. Nisbet, *The Quest for Community* (New York: Oxford University Press, 1953). The classic statement is Tocqueville's. For an incisive criticism, see Michael Rogin, *The Intellectuals and McCarthy* (Cambridge: M.I.T. Press, 1967).

[15] See among innumerable others Grant McConnell, *Private Power and American Democracy* (New York: Knopf, 1966).

[16] See the selections in Kariel (ed.), *op. cit.*

[17] Marx, of course, posed this question, but his idiom has been more difficult to Americanize.

experimentally, compel social science to emerge as a form of action. It entails a commitment to an active, creative social science—a *political* science—for widening the scope of publicly controlled experience, for increasing the time and enlarging the space for political life. Reducing self-destructive privacy, it would extend man's various deadlines.

The extent to which social science offers practical demonstrations of the feasibility of democratizing creativity is not readily apparent, for the motives of social scientists are mixed and their work is accordingly ambiguous. Admittedly, the point of some scientific projects has not been obscure: behavioralists have been recurrently pleased to instruct men in power how to manipulate consumers, employees, patients, television viewers, prisoners, students, and rebellious populations.[18] Their very prose style has closed alternatives as they unambiguously defined deviants, reified existing systems, or converted their hypotheses into finalities. In their grading system, the most shameful grade is "Incomplete." Yet opportunism, impatience, and the will to settle things have not been the whole of contemporary social science. Social science has also shown itself capable of remaining experimental, of designing prototypes and constructing social laboratories—whether in hospitals, corporations, prisons, schools, or ghettos.[19] In 1955 Harold Lasswell became explicit about the need to build more bridges "between laboratory situations and field situations, . . . to take the laboratory design into the field and apply it to a whole community context." The community—transformed into a laboratory—may
then be explored more intensively in order to identify the variables that account for the deviation. This can be done by applying more laboratory-type measures at the proper spots and by instituting a program of 'probers,' 'pre-tests,' 'interventions,' and 'appraisals.'[20]

[18] For early examples, see T. N. Whitehead, *The Industrial Worker* (Cambridge: Harvard University Press, 1938); Alexander H. Leighton, *The Governing of Men* (Princeton: Princeton University Press, 1945); and Schuyler Dean Hoslett (ed.), *Human Factors in Management* (New York: Harper, 1946).

[19] Harold D. Lasswell, "Current Studies of the Decision Process: Automation versus Creativity," *Western Political Quarterly,* 8 (September, 1955), 385–386.

[20] See especially Robert Rubenstein and Harold D. Lasswell, *The Sharing of Power in a Psychiatric Hospital* (New Haven: Yale University Press, 1966); and the research strategy advocated by Bell and Mau, *op. cit.*

Progressively expanding its laboratories, the social sciences may thus become openly activitistic.

No less radically, they may also consider the value of a phenomenological orientation which seeks to give recognition to repressed parts of the present—to causes which are latent, unpopular, discredited, or lost. Turning to more of the present than is immediately apparent, they can work toward its fullest possible realization. Cutting beneath prevailing simplifications, they can attempt to give creditable accounts of political potentialities, truly simulating by treating nothing as necessary or real.

Given the pressure to reduce intolerable ambiguities and dissonances, we are of course not disposed to add to life's complications. On the contrary, we are moved to seek relationships, solve problems, and explain political acts by functionalizing them. Yet only when we succeed in suspending our causal analyses and explanatory theories can we perceive the unconditional, innovative, and self-determined aspect of human conduct. Only when we decline to judge and draw conclusions can we discern the expressive element of politics, the display of capacities not ordinarily displayed, or not ordinarily displayed with as much grace or intensity.

To perceive this expressive aspect of experience, we need merely follow the clues of Hannah Arendt and conceptualize political action as a form of play, as characteristically a performing art. When politics is conceptualized in these terms—as something more than a struggle for power to allocate values in short supply—it will not be seen as a function of scarcity. From a phenomenological perspective, it emerges as a manifestation of human freedom, of the determination to use abundant resources to create publicly meaningful structures of being—whether these are demonstrations, research designs, urban redevelopment programs, war games, election campaigns, or entire political systems. Should we wish to understand the way action signifies the presence of ordinarily unrealized structures of being, we cannot regard it as conclusively significant in any other sense—in the sense, for example, of "really" signifying some predefined intention or of being "really" functional to some predefined structure. We must see it as a form of play: *complete in itself.*

The obstacles to applying this orientation to the normal subject matter of political science are considerable. Political scientists whose scope-and-methods courses orginally led them to invest in a definition of politics as economizing behavior are bound to become troubled. It is certainly more conventional (and for some purposes more useful) to view so-called voting be-

havior, for instance, from non-phenomenological perspectives. We may explain it as a function of group membership, as a conditioned response to economic deprivation, or as determined by income level. Or we may interpret it as a gesture intended to influence policy. But to understand it as a form of action, it must be perceived as no more (and no less) than the manifestation of man's desire to appear in public and display his freedom. Such a display, we must acknowledge, proclaims the sheer determination to act, the will of an individual to appear, to *be*. It is hardly surprising, on reflection, that social scientists or men in power should have difficulties seeing this determination in others. The self-assertiveness of outsiders is bound to strike the powerful either as not really intended or as inexplicably arrogant, especially when the resources for effective political action are believed to be scarce. When individuals make their appearance—freeing themselves from necessity—elites readily characterize their emergence as an emergency. But social scientists might be slower to judge, slower to characterize behavior as dysfunctional, recognizing that the behavior of the less privileged, less active orders of society may at least partially be a form of action.

Although political acts can no more be explained scientifically than the will to survive, they may nevertheless be elucidated. While not regarding them as useful or necessary or instrumental, as intended to make an impression or to teach a lesson, as being *for* anything, the social scientist can yet describe their distinctive modes and structures. He can yet communicate his understanding of them (as Lévi-Strauss did in *The Raw and the Cooked* or Norman Mailer in *Armies of the Night*), translating them so that his exposition will reveal not *what* they signify but the *way* they signify. His model is provided by the German critic Walter Benjamin: "A translation, instead of resembling the meaning of the original, must lovingly and in detail incorporate the original's mode of significance, thus making *both* the original and the translation recognizable as fragments of a greater language. . . ."[21] Making us aware of a "greater language"—of a larger symbolic universe than we conventionally recognize—the writer can convey what a political act *is*. And what it is, he can make clear, is a distinctive way of existing, of resisting necessity, of converting absurdities into meaningful reality. Thus the social scientist as translator can publicly ponder and implicitly exalt the sheer appearance of political life—the inexplicable fact that it is present at all.

[21] Walter Benjamin, *Illuminations*, Harry Zohn, tr. (New York: Harcourt, Brace & World, 1968), p. 78; italics supplied.

If his literary act fails to "explain," it nonetheless establishes reality for others as well as himself. Seeking to come to terms with man as self-directed agent, with expressive aspects of behavior, he assumes that his accounts must be idiographic and ambiguous, that the very concept of explanation, as John Gunnell has said, is "context-bound and field-dependent."[22] One test of his success is his capacity for deepening our understanding of men who make their way outside accredited systems, who act in incongruous situations and unfamiliar environments: marching on the Pentagon, growing up in a kibbutz, serving on workers' councils, participating in utopian communities, playing unorthodox roles, or gaining balance in psychoanalytic encounters. A less orthodox test of such a political science is the extent of its impact on those it presumes to understand. It succeeds to the extent that it enables them to recognize more of themselves than they ordinarily recognize, that it raises their expectations, that it expands their consciousness, freeing them to be more playful and political, more active and alive. Perhaps the ultimate test is its impact on the political scientist himself, even if only he can finally tell whether his symbolic action—his contribution to a journal, his lecture, his public performance—has added, not merely to his *curriculum vitae*, but to his life.

In any case, such a political science serves to amplify and enlarge our existence by illuminating previously invisible, generally discredited dimensions. Bringing apparent exceptions into our symbolic frameworks, it demonstrates how little (or how much) discipline is necessary to satisfy human needs under varying conditions. It makes clear, for instance, that however unavoidable social differentiation may be, a system of social stratification is dispensable, that "there is nothing demonstrably 'natural,'" as Melvin Tumin has shown, "about reacting to differential skills in the ways generally found in modern industrial societies."[23] Though the rhetoric of such a polit-

[22] John G. Gunnell, "Social Science and Political Reality: The Problem of Explanation," *Social Research*, 35 (Spring, 1968), 159–201, 173–174.

[23] Melvin Tumin, "On Inequality," *American Sociological Review*, 28 (February, 1963), 19–26. See also how role differentiation, social stratification, and status inequality are avoided in kibbutzim: Richard D. Schwartz, "Functional Alternatives to Inequality," *American Sociological Review*, 20 (August, 1955), 424–430. Arnold Gehlen's work shows that no specific social order is prescribed by nature: *Der Mensch, seine Natur und seine Stellung in der Welt* (Bonn: Athenäum Verlag, 1950); *Urmensch und Spätkultur* (Bonn: Athenäum Verlag, 1956).

ical science is different from that of history, fiction, films, or psychological case studies, it no less reveals man's capacity for role-taking and role-switching, for escaping the assignments hierarchical organizations seem to demand.[24]

As they open alternatives, the social sciences necessarily follow Reinhard Bendix's advice to raise questions, as he says,

> contrary to the manifest evidence in order to bring out those capacities of the social structure which any limited body of evidence tends to omit. By exposing observation at any one time to a wider range of comparison with the past (or with other social structures) than is sometimes customary, we may approach an understanding of social structure and change without at the same time moving too far away from the evidence.[25]

By giving credence to past or distant or unfashionable life styles—to modes of conduct which are *possible* though regarded as failures—the social sciences reverse conventional valuations of goals. Valuing the needs of the child over those of the existing school or, as Chris Argyris has done,[26] the needs of the worker over those of the organization, they introduce options. Positing countervailing values, they enlarge understanding. By engendering a growth in empathy toward modes of action intimated but not clearly expressed by extremist movements, they induce a deliberate shifting in point of view—a rejection of the study of outsiders from the established center, an acceptance of men who are active at the margin.[27]

Whether the social scientist is engaged in experimental prototyping or in expressing his understanding of political acts in phenomenological accounts, he remains in touch with his subject matter, treating it not as an object to be acted upon but as phenomena with which to interact. His involvement entails suspension of that subject-object distinction which leads social scientists to disregard unexpressed potentialities and deal with people as if, like objects, they had no political will of their own.[28] It entails approaching society with an attitude of receptivity to the claims of its members, with an artful, simulated readiness to reinforce them, to go along with them, to say forcefully what they say haltingly.[29]

Offering actual demonstrations of the possibility of revolution, this approach leads to a restructured, transformed reality. While it displays options in altogether practical terms, it does not provide inventories of definitive propositions. It therefore should not lead us to expect what students in the social sciences are generally taught to produce—findings, conclusions, information, or general theories. Prepared to be less goal oriented, we can merely expect (it has become hard to say so) a measure of wisdom—an increase in self-conscious empathy, in managed compassion and governed openness.[30] Thus successful social experiments as well as the writings of phenomenologists can scarcely serve to provide knowledge (or even "insight"). They merely draw us into a process of discovery which enables us to grasp what we tacitly know but fail to acknowledge—namely, that more of life might be lived rationally. Rejecting the reified abstractions of Social Science as well as the homilies of Wisdom Literature, they compel us to accept only what cannot be mastered by human action. Ideally, they then leave man to himself, but to more of himself than he knew was his before the social sciences engaged him. "A child may feel hungry and not know that it wants to eat, until it is offered food," Michael Polanyi has noted[31]—implying that the probing of social analysts exposes repressed needs and incites action simply by putting man's immediate responses into question. A science which thus challenges the present by adducing unestablished dimensions of reality assumes not that men are ignorant but that they are burdened by an excess of insinuating knowledge, by elite-

[24] See R. W. B. Lewis, *The Picaresque Saint* (Philadelphia: Lippincott, 1959); Ralph H. Turner, "Role-Taking: Process versus Conformity," in Arnold M. Rose (ed.), *Human Behavior and Social Processes* (Boston: Houghton Mifflin, 1963), pp. 20–38; and Robert Jay Lifton, "Protean Man," *Partisan Review*, 35 (Winter, 1968), 13–27.

[25] Reinhard Bendix, *Nation-Building and Citizenship* (New York: Wiley, 1964), pp. 13–14.

[26] Chris Argyris, *Personality and Organization* (New York: Harper, 1957). See also Warren G. Bennis, *Changing Organizations* (New York: McGraw-Hill, 1966).

[27] See Alvin Gouldner's criticism of sociologists as condescending patrons of outsiders: "The Sociologist as Partisan," *American Sociologist*, 3 (May, 1968), 103–116.

[28] See *ibid.* and works cited in note 18, *supra*.

[29] For a lucid discussion of gaining knowledge through empathic activity, see Edwin A. Burtt, *In Search of Philosophic Understanding* (New York: New American Library, 1965), Ch. 9.

[30] To recognize this may reveal the pathos in efforts to make public administration cases yield general propositions: Frederick C. Mosher (ed.), *Governmental Reorganizations: Cases and Commentaries* (Indianapolis: Bobbs-Merrill, 1967). Of course, cases may make the student sensitive to the complexity of administrative processes; to claim more, however, is to succumb to scientism.

[31] Michael Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy* (London: Routledge & Kegan Paul, 1958), p. 202.

serving solutions, by needless words and theories —in short, by ideologies. Consequently writers such as Mead or Sartre or Dewey have always fought the temptation of offering answers. Refusing to simplify our lives, they have deepened our predicaments. But if the studied emptiness of their work is annoying, it does free us to provide answers for ourselves, to construct our own futures now, to revolt and make history.

III

Short of efforts to vacate our practical and theoretical edifices and to invite men to move in and establish themselves—short of efforts to annihilate and reconstruct reality—we cannot possibly perceive the defeats implicit in the various offered projections of our present victories. The most precise cost-benefit accounting or the most elegant exercises in futuristics cannot expand the present as long as their designers simply acquiesce in the Facts of the Situation, the Inner Logic of Industrialism, or the Natural Laws of Organizational Development. Without an idealistic orientation which, by alerting us to deranged and disorderly interests, serves to enlarge the center, we are bound to let our present achievements become permanent fixations. We thereby allow ourselves to be arrested in our development simultaneously as citizens and social scientists. True, while arrested we would, like prisoners, still sense the passage of time. But we would have no future. We would have lost the opportunity for giving time publically significant meaning, for politicalizing it.

When social scientists succeed in being creative in each present moment, destroying meaningless time by acting, they deprive the prevailing concept of "the future" of its current ideological value: they thereby make it serve more than the established present reality. De-emphasizing planning and prediction (both functions of scarcity and insecurity), questioning pre-ordained sequences and breaking continuities, they multiply realities and keep progressively more of them in a state of suspense. As social scientists induce the mass of men to let go, to find release from the merely contemporary objectification of time, they disregard the sense of time within which elites find it profitable to imprison others.

So far, it is true, only elites have been able to live for the moment and to make the most of it.[32] But it is possible to test experimentally whether others, too, might bear the freedom to impose alternative meanings on history. Encouraged to defy the linear predictions of the new utopianism, more men may be able to identify concealed political potentialities; given the power to make more of the present, they may be able to let more of the future take care of itself. As they become aware of the openness of the future—cherishing its emptiness, its mystery —they might learn to dispense with some of their defense mechanisms, to repudiate timeless reifications, and to create unanticipated, still disreputable realms of being.

[32] Robert Jay Lifton has suggested that the mass of men lack tolerance of a variegated present and consequently crave connections with the future because of a pervasive fear of death, and that this in turn generates an interest in progeny, an obsession with durable achievements, and a devotion to undying causes and death-defying leaders: *Revolutionary Immortality: Mao Tse-Tung and the Chinese Cultural Revolution* (New York: Random House, 1968).

PRISONERS OF WAR: DOES THE FIGHT CONTINUE AFTER THE BATTLE?

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I. REFUGEES AND PRISONERS OF WAR

Just beyond the state there is a kind of limbo, a strange world this side of the hell of war, whose members are deprived of the relative security of political or social membership. Different sorts of people live there, mostly for indefinite periods of time, people who have been expelled from their state or otherwise deprived of legal rights, people whose state has been defeated in war and occupied or who have been separated somehow from its jurisdiction. Among the residents, two groups endure conditions paradigmatic for all the others: refugees, deprived of their rights by persecution; and prisoners of war, separated from their state by captivity. The two are very different, since refugees are stateless persons, radically dependent on their hosts and unable to look backward to any protecting authority, while prisoners remain citizens still and receive such protection as their states can provide. However distant and isolated they may be from their home country, their captivity is (hopefully) temporary; both captives and captors may one day be required to account for their behavior. Nevertheless, prisoners and refugees belong alike to the limbo world. They cannot expect effective help from any organized society; they do not know when, if ever, they will be "at home" again; they are compelled to reconstruct or redefine their obligations without reference, or without clearcut reference, to authoritative laws and commands.

Refugees face their hosts with a special kind of helplessness and pathos, but also with an unaccountable and peculiar freedom. They are men free of all allegiance except to other men, without political obligations, at least without obligations to any state in the world of states. And their condition testifies to the wretchedness of such freedom.[1] Frightening as it may be to the authorities (at least, the authorities generally profess to be frightened), it is far more so to the refugees themselves. Until they are given a home, they have a right to do whatever they can to save themselves, within limits imposed only by their humanity and the relations they form with particular other people. But in fact they can do painfully little, and theirs is a freedom any of us would speedily exchange for membership and protection, despite the restraints these impose. The value of the state as an inclusive community, the importance of even the most minimal kinds of social solidarity and legal definition—these are never more clear than when we regard ourselves as possible participants in the limbo of statelessness.

The prisoner, on the other hand, is doubly unfree, since he is not set loose from his former allegiance, or so he is told, by his captivity. If he is forced to face his captors, bound or at gunpoint, he must also look over his shoulder to the authorities of his own state. He is required continually to balance the obligations he knows he once had, and may still have, as a citizen and soldier, against the more immediate threats and coercions of his captors—and perhaps also against his new obligations to the society of prisoners. What should he do? It is never easy to say. If the statelessness of a refugee is frightening, so is the citizenship of a prisoner of war. To regard ourselves as possible captives is to learn how oppressive political obligations can be. Even limbo, as we shall see, has its temptations.

II. THE CONDITION OF PRISONERS OF WAR

Captivity can be conceived in two different ways: first, as the termination of combat for an individual soldier, the imposition by the captor and the acceptance by the captive, of a total quarantine for the duration of the war; secondly, as the termination of one sort of combat and its replacement by another sort, where the fighting is relatively circumscribed and its conditions radically unequal.[2] Both of these are modern conceptions; both depend on the recognition of the prisoner as a moral and legal person, possessed of certain rights, entitled at the very least to be kept and provided for by what the lawyers call "the detaining power." The

[1] For a brilliant discussion of the problem of statelessness, see Hannah Arendt, *The Origins of Totalitarianism* (New York, 1958), Ch. 9.

[2] See G. S. Pugh, "Prisoners at War: The P.O.W. Battleground," 60 *Dickenson Law Review* 123–138 (1956), and "The Code of Conduct," 56 *Columbia Law Review* 678–707 (1956). The best book on the nature and laws of captivity is William E. S. Flory, *Prisoners of War: A Study in the Development of International Law* (Washington, 1942).

prisoner is "detained," and that means that whatever else is done to him or with him during his detention, he cannot be killed or enslaved. Not so very long ago, a prisoner was thought to have forfeited his life by his surrender. Then his slavery was justified as the end result of an exchange made possible solely by the benevolence of his captor, an exchange of life for perpetual service. On that view, captivity was in no sense a status within which a man might be more or less secure even if impotent. Prisoners were rapidly converted into dead men or living slaves, and the choice between these two conditions was only secondarily their own.[3] (In practice, during the early modern period, prisoners were often required simply to join the army of their captors, common soldiers being little more than chattel anyway.) Today, a prisoner can expect to remain a prisoner for as long as the war lasts, and then he can expect to be released. In the interim between capture and release, he is forced into a relation with his captors unlike that of slave and master, though not entirely unlike it, and into a relation with his fellow prisoners unlike that of any normal political (or military) association. The prison or prison camp is the scene of a new society, which exists precariously in the shadow of the detaining power and whose members feel the pull of their previous political commitments, not least because these are likely to be their future commitments as well.

This new society of prisoners exists by virtue of, or at least in accordance with, international law. Its members have even been called, by a serene theorist of the nineteenth century, "citizens of the world."[4] The world is, unfortunately, not so organized that citizenship in it is much of a benefit—as stateless persons have learned over and over again in the past 30 years. But the name does serve to suggest something very important, if true: that prisoners are no longer at war. It presses us toward the first of the two conceptions of captivity. For the war is being fought by citizens of this state and of that one, and citizens of the world, even if they hold dual citizenship in one of the belligerent states, are presumably exempt from the compulsions of the struggle. The rules of quarantine replace the code of battle, and this replacement is not merely legal, but moral as well; it frees the prisoner from all obligations to continue the war.

The same replacement would seem to be implied by the conventional practice of "surrendering" or "giving up," though this has sometimes been denied. In the *Code of Conduct* for US soldiers issued by President Eisenhower in 1955, it is said that soldiers must never surrender "of their own free will."[5] If it were possible for soldiers to surrender in any other way, the act might indeed have no moral consequences. But unless the *Code* is intended to bar surrender altogether—and then its subsequent provisions, which deal with the conduct of prisoners, would be superfluous—the phrase "of their own free will" is very odd. The authors presumably want to say that only extreme duress and the threat of imminent death are acceptable reasons for surrender, but that does not mean that the act of surrendering can never be a decision of the men who surrender or an agreement between them and their captors.[6] It most often is a deci-

[3] Hugo Grotius, writing in the early seventeenth century, still defended the right of enslavement, *De Jure Belli ac Pacis,* Book III, Chapter 14, Section 9. The decisive theoretical critiques of this idea are by Montesquieu, *The Spirit of the Laws,* Book IV, Chapter 2, and Rousseau, *The Social Contract,* Book I, Chapter 4.

[4] James Lorimer, *Institutes of the Law of Nations* (London, 1884), Volume II, p. 72. The whole passage is excellent and should be consulted.

[5] The *Code of Conduct* is the most important official statement on how U.S. soldiers are to behave when overpowered by an enemy, and I will refer to it frequently below. At the time it was issued, its legal status was unclear (see Pugh, "The Code of Conduct"), but it has recently been described, in the aftermath of the *Pueblo* incident, as "only a guideline." Violations of the articles of the *Code* do not constitute a criminal offense: *New York Times,* December 29, 1968. It is presumably still the opinion of the authorities that they constitute a moral offense, and the *Code* continues to play a very important part in the training of US soldiers.

[6] Something must be said here about the difficult problem of coercion, though to deal with it in even a minimally adequate way would require another essay. The common and plausible view is that no man is morally bound to fulfill a contract or commitment he was coerced into making. Since the word "coerced" is usually taken to mean "forced by violence or the threat of violence," surrender would appear to have no moral validity. But it might be argued against this that the definition of coercion depends or ought to depend on the situation of the individual said to be coerced, and that in the context of war, violence exercised in accordance with the laws of war is not coercive. Surrender is a social practice designed to accommodate the human difficulties of war; it can't be judged by conventions appropriate to other, and very different, settings. This calls into question the dictionary definition and ordinary usage of the word "coercion." But there are only two alternatives, neither

sion, and it is probably always an (implicit) agreement. Even when no decision seems possible, as when the sailors of a sunken ship are simultaneously rescued and captured, it can probably be assumed that the prisoners agree to their captivity, if only for the sake of their rescue, and that they approve of the general practice of taking prisoners. In battle or in the aftermath of battle, surrender usually involves a more explicit judgement and choice on the part of the men surrendering, a judgement of their own continuing effectiveness as soldiers and of the risks they face, a choice to fight on or not. And this is in some sense a free choice, though it is made under varying degrees of duress, since soldiers can and have been known to accept enormous risks and even fight to the death. Surrender is precisely a way of choosing not to take enormous risks and not to fight to the death. It takes the conventional form of an agreement not to fight at all, to "give up" fighting, in return for life itself and then benevolent quarantine.

This agreement obviously requires a second party; surrender must be accepted as well as offered, and armies can refuse, though no longer lawfully, to take prisoners. Clearly, there would be a positive incentive to refuse if surrender were thought to involve no commitment to cease (and not to resume) fighting, just as there would be a positive incentive to fight to the death if the acceptance of surrender were not viewed as a commitment to one or another form of quarantine. Surrender as a social practice is only possible in the absence of these two incentives, that is, it is only possible when it is conceived as a mutual engagement with moral consequences. This conception is clearly reflected in international law, which requires benevolence of captors and simultaneously deprives a prisoner of all combatant rights.[7] If he kills a prison guard, for example, the act is murder and not war, for he has given up fighting and thereby reassumed some of those civilian obligations which are suspended for soldiers in the face of an enemy.

The essence of surrender is the agreement not to fight, and this agreement may not require quarantine at all. In the days when officers were also gentlemen and aristocrats, it was perfectly normal to release a prisoner on parole, that is, to accept his word that he would not rejoin the fighting and then to permit him personal freedom of movement, even to permit him to go home. This is only possible on a very strict view of surrender, which holds the captives temporarily free of political allegiance and so able to make arrangements on their own behalf: "separated for the time being from any political community, they . . . belong to humanity and to themselves."[8] Eighteenth and nineteenth century lawyers argued—and their arguments were for a time widely accepted—that if a prisoner did return home on parole, his home state could not force him to fight again or in any way require him to violate his faith.[9] This clearly implies that the prisoner did not break faith when he gave parole in the first place; it grants that he belonged at that moment in time "to himself." Perhaps that is a status which aristocrats resume more readily than other men; nevertheless, its recognition is, I think, an extraordinary tribute to the liberality of eighteenth and nineteenth century states, or of some of them, and of their lawyers and philosophers. Modern states, whether liberal or not, are not inclined to grant a similar recognition. Most of them, including the United States, deny prisoners the right to give their parole (except under very restricted circumstances and for short periods of time). International law has been adjusted so as to deny validity to any parole arrangement contrary to the laws or regulations of the prisoner's home state.[10] Very occasionally the old ethic is rediscovered. Thus, a small number of captured American soldiers gave their parole to the Japanese in the Philippines during World War II, and "these paroles were recognized by the US as prohibiting military and other resistance to the Japanese on the part of the individuals concerned."[11] But such agreements are rare in this century and today the *Code of Conduct* explicitly declares that prisoners must not give their parole to their captors. Prisoners, it would appear, never belong, or are never admitted to belong, merely "to humanity and to themselves."

It is perfectly understandable that the state should continue to claim the allegiance and sup-

of them attractive: either to suggest that no morally binding agreements are possible in wartime, or to accept Hobbes' argument that "Covenants entered into by fear . . . are obligatory" (an argument Hobbes explicitly applied to prisoners of war—see *Leviathan*, Chapter XIV).

[7] See the discussion in Pugh, "The Code of Conduct," pp. 682–683, 690n., and Eric Williams' introduction to *The Escapers* (London, 1953), p. 15.

[8] Lorimer, *op. cit.*, II, 72.

[9] See the British *Manual of Military Law* (1884): "A State has no power to force its subjects to act contrary to their parole." Quoted in Flory, *op. cit.*, p. 123.

[10] Geneva Convention I, Article 21; for a discussion of contemporary law on parole arrangements, see Morris Greenspan, *The Modern Law of Land Warfare* (Berkeley, 1959), pp. 108–110.

[11] Pugh, "The Code of Conduct," p. 683n.

port of prisoners. It is even in the interests of the prisoners that it should do so—up to a point. They have no wish to be abandoned, however uneasy they may be about their "obligations." Nor does the state make its claims very effectively, since it has no immediate control over the persons of the prisoners and is unable to offer any but the most minimal guidance as to their everyday behavior in the radically new society of the prison camp. The claims of the state are still enormously important; they point toward the second and at present (in this country and more clearly in some others) the official view of the status of captives and the moral meaning of captivity. On this view, prisoners of war must regard themselves as combatants even after their capture, full-fledged members of a state at war, a state in trouble, needful of all the men it has, unwilling to yield the services of anyone except to death itself. The prisoner, so the *Code of Conduct* tells him, is an "American fighting man" (Article I). He must refuse any sort of cooperation with his captors and seek continually to escape. By implication, he must also help whenever he can to organize mass escapes, to maintain a resistance network in the camp, to harrass the enemy, spy on him and sabotage his behind-the-lines operations. He must always act so as to require as many guards as possible. It is a little hard to see why any state at war would maintain prisoners who actually did or tried to do all these things, and insofar as the *Code* requires them, it is not entirely consistent with the idea of benevolent quarantine. This is partly because it was designed as a response to North Korean and Chinese captors who paid little regard to that idea, but it also reflects, as did the actions of the North Koreans and Chinese, an extension of state sovereignty and an attempt at ideological control which amount to a denial that individuals can ever move, even partially, out of the range of political action and supervision. There simply is no space, it suggests, that might be called limbo, where quarantine is possible and a certain human passivity morally justified.

Contemporary international law, as codified in the Geneva Conventions (1949), does not require the passivity of prisoners; indeed, it provides carefully and at considerable length for attempted escapes, though not for any other kind of resistance.[12] But it is not unfair to argue that the most humane provisions of the present code —for example, the rule that prisoners captured while trying to escape cannot be punished, but only detained more strictly, and that only for a short time—are rooted in the expectation that the vast majority of prisoners will not resume or make any attempt to resume their combatant roles. And that is an expectation most often realized in practice, despite the wishes of the governments whose citizens are imprisoned. The erosion of the idea of benevolent quarantine has come largely from the other side, from captors and not from captives, as one might expect; for if combat is resumed, it is the captors who have every advantage. Still, prisoners have chosen to fight on, sometimes in response to the coercive acts of the enemy, sometimes on their own initiative, and it is worth asking whether there is any justice at all in the increasingly strong presumption of their home states that they ought to do so.

Article III of the US *Code of Conduct* requires that American soldiers, if captured, must "continue to resist by all means available." This is its most extreme provision, though the ambiguous word "resist" was carefully chosen. Mr. Carter Burgess, Assistant Secretary of Defense, argued at the time the *Code* was adopted that physical resistance (which is barred by Geneva Convention I, article 82) was not required of prisoners, but only mental resistance to political indoctrination.[13] That is hardly made clear by the *Code* itself, and the lack of clarity was presumably intentional, since the wording was long debated. One legal critic of the *Code* has pointed out that unless it is read as a call for resistance "by all *legitimate* means available," prisoners "would be encouraged to commit acts that may be war crimes or at least contrary to the spirit of the Geneva Conventions."[14] Needless to say, their captors would be similarly encouraged.

The *Code* was adopted in the immediate aftermath of the Korean War, a struggle in which the noncombatant status of prisoners was undermined by both sides, though most dangerously and cruelly, judging from the available evidence, by the North Koreans and Chinese.[15] In prison camps north and south of the battleline, a new kind of war went on for the minds, that is, for the loyalty and political commitment, of the prisoners. To some extent, this was a war that could be fought within the limits set by the Geneva Conventions. But one of its most ominous results was that in the American-run camps, where the Conventions were fairly rigorously observed, and in contrast to the Commu-

[12] Geneva Convention I, Articles 42, 91–93; see Greenspan, *op. cit.*, pp. 135–137.

[13] Carter L. Burgess, "Prisoners of War," 56 *Columbia Law Review* 676–677 (1956).

[14] Pugh, "The Code of Conduct," p. 690.

[15] But see W. Burchett, *Koje Unscreened* (Peking, 1953).

nist camps where they were not, resistance was amazingly successful. At one point, major sections of the camp on Koje Island were entirely controlled by North Korean prisoners, who even captured and for several days held the American commander of the camp. It quickly became clear that "measures permitted under the Geneva Conventions would not completely destroy the Communist organization within the compounds." The North Korean prisoners could no longer be regarded, or treated, "as passive human beings in need of care and protection. . . ."[16]

General Mark Clark's comment on the Koje mutiny suggests what American officials learned from this incident: ". . . my experience," he wrote, "had been with *old-fashioned* wars in which prisoners were people to be fed, housed, clothed, and guarded, nothing more. Never had I experienced a situation in which prisoners remained combatants and carried out orders smuggled to them from the enemy high command."[17] Only a few years later President Eisenhower brought American policies up to date by issuing the *Code of Conduct*, recommended to him in a Defense Department report entitled "P.O.W.—The Fight Continues After the Battle."[18] The *Code* was explicitly intended to provide a secure moral basis for resistance to the indoctrination of prisoners as this had been practiced by the North Koreans and Chinese. But its careful ambiguity suggests that it was also intended to encourage action on the scale of the Koje mutiny. What happens then to the "old-fashioned" notion that prisoners must be maintained and guarded and nothing more? Surely there would be some value in reiterating that notion, rather than merely conceding its antiquity.

III. OBLIGATIONS AND FORMS OF RESISTANCE

On the other hand, when serious pressure is put on prisoners to collaborate with the enemy, either by supplying military information or expressions of political support, we do want "our" soldiers (at least) to refuse and resist. It is important to understand just what is involved in such refusals: they don't represent a continuation of the war so much as an unwillingness to enlist or be enlisted on the other side; they are a defense of all that is implied by the idea of quarantine. Prisoners refusing to collaborate are also, of course, refusing to "sell out" their comrades or their recent comrades and to betray their country. But the obligations which they thus acknowledge and sustain are minimal in character. They can most readily be expressed in the negative: not to inform, not to confess, not to collaborate, and so on. I don't mean to suggest that they are for that reason easy to sustain. Whether they are easy or hard (or impossible) to sustain depends on the actions of the enemy. They are significantly different, however, from obligations that lend themselves to positive statement: to try to escape, to harrass, to sabotage, and so on. In practice, most states attempt legal enforcement only of negative obligations. Prisoners who collaborate with the enemy may be charged after the war with misconduct or even with treason, but not prisoners who refuse to join escape organizations.[19] Treason obviously requires some overt act of "aid and comfort," so mere refusals to participate in escape or harrassment attempts don't qualify. But the inability and unwillingness of states to enforce positive obligations has other reasons, which call into question their very status as obligations.

Negative and positive obligations (I mean the two adjectives merely as rough indications of certain sorts of actions and refusals to act) are mediated very differently to the individual soldier. Negative obligations are his as an individual, a member, let us assume a willing member, of a particular state or of its army, and they clearly survive temporary separation from either. In captivity (as out of it) these obligations may be over-ridden, or they may be violated in extenuating circumstances. Refusals to collaborate, for example, may endanger the society of prisoners as a whole or some of its individual members, and then the man who is being pressed to perform this or that service must weigh relative evils and make a difficult and painful decision. It is important to stress the word "must," for whatever decision he comes to, he is faithful only if he takes into account his obligation not to act so as to injure his country or endanger his comrades-in-arms.

Similarly, in those cases where a prisoner collaborates under duress, he is later judged, and

[16] Pugh, "Prisoners at War," citing UN Command Reports, p. 131. A complete account of the Koje mutiny, from the American point of view, can be found in Hal Vetter, *Mutiny on Koje Island* (Tokyo, 1965).

[17] Quoted in Vetter, *Mutiny*, p. 10 (emphasis added).

[18] Report by the Secretary of Defense's Advisory Committee on Prisoners of War, August, 1955.

[19] Rebecca West reports on the trials of several British prisoners of war for treason in *The Meaning of Treason* (London, 1947). For a complete review of judicial treatment of prisoner misconduct, see "Misconduct in the Prison Camp: A Survey of the Law and an Analysis of the Korean Cases," 56 *Columbia Law Review* 709–794 (1956).

rightly so, by the integrity of his resistance as long as it lasts, that is, by his own manifest sense of his obligations. I cannot say, and I am not sure any free man can say, what constitutes an adequate manifestation.[20] Once a prisoner has done what he can on behalf of his home state, by refusal, evasion, or deceit, and can resist no more, his fellow citizens can only mourn with him, and not condemn, the disloyal or degrading acts he is driven to perform. Some manifestation of resistance there must be, however, so long as the prisoner regards himself and hopes to be regarded as a citizen. Negative obligations survive every separation except explicit renunciation, which is only possible under certain conditions and which terminates citizenship. They are paralleled, it should be added, by the obligations of the home state to provide protection whenever it can, to protest every breach of the rules and agreements that make for benevolent quarantine, and to do nothing and demand nothing of its soldiers that might prompt or seem to excuse the coercion of prisoners.

Positive obligations belong to the individual as one of a group of citizens or soldiers. This can be a group of conscripts, whose members are designated as falling under one or another law or legitimate command, or a group of volunteers mutually engaged in some enterprise of value to the larger society. It seems most unlikely, in either case, that positive obligations survive clear-cut (even if temporary) separation from the group. The prisoner is alone and on his own, or he is the member of a new society, and while he remains in an important sense a citizen, he can no longer be regarded as a servant of the state. For he is cut off from the group support and organizational structure that once made his service possible. Positive obligations can be reinstated by joining an escape organization or a resistance network in the prison camp. But such reinstatements are actually commitments to a new group, and the precise relation of the new group to the larger society, the state or the army, is radically unclear. It seems unlikely that the state's right to conscript, for example, can ever devolve to an association of prisoners, though such groups have sometimes claimed that right. A kind of rough and ready conscription may, of course, be effective in a camp where an escape organization has widespread support. There are many examples of this sort of thing in prison camp literature: certain kinds of minor helpfulness have often been required of prisoners who have no personal interest in escaping. It is less their patriotism that one appeals to at such moments, however, than their loyalty to fellow prisoners.

This appeal to the others may or may not be successful. That will depend on the nature of the society of prisoners as a whole (which may in turn depend on the nature of the state and army from which the prisoners come) or on the character of the personal relations involved. But it is clear that no prisoner has a positive obligation *to the state or army* to work and take risks for the escape of himself and others or for the harrassment of the enemy. Unless he actually commits himself to such purposes, he has what must be called a perfect right to "enjoy" his quarantine and to sit out the war—so long as doing so involves no conflict with his other obligations.

There are obligations, however, in addition to those positive and negative duties owed or said to be owed to the state, despite the prisoner's separation from the state. Captivity is not only a separation; it also brings men together and does so under conditions that make their cooperation both vitally necessary and extremely difficult. It can't accurately be said that anyone joins the society of prisoners willingly, even though there is a sense, as I've already argued, in which their surrender is a voluntary act. But that society can be organized on a consensual basis, and probably ought to be, either by intelligent officers who recognize that their own authority survives in a tenuous way, if at all, or, in the absence of officers and in accordance with the Geneva Conventions' provisions for enlisted

[20] The courts must say something, however, and what they have said is discussed in "Coercion: A Defense to Misconduct while a Prisoner of War," 29 *Indiana Law Journal* 603-621 (1954). In passing judgment on prisoners who have given military information, it is worth noting the finding of army experts after World War II: "It is virtually impossible for anyone to resist a determined interrogator." Cited in "P.O.W.—The Fight Continues After the Battle," p. 61. For a full discussion of this problem, see Albert D. Biderman's careful study of the behavior of American prisoners in Korea, *March to Calumny* (New York, 1963).

The whole question has recently been reopened because of the *Pueblo* case, and considerably complicated by the fact that the sailors of the *Pueblo* were imprisoned by a state with which the United States was not at war. They were thus formally unprotected by the Geneva Conventions and entirely without any sense of when or whether they might be returned to the United States. Perhaps for this reason, but also because of increased American sensitivity to the plight of prisoners, their "confessions" were not thought by the Secretary of the Navy to warrant court martial proceedings.

men's camps, by the election of representatives.[21] The political organization of the prison camp, whatever form it takes, considerably simplifies the moral life of the prisoners. It makes possible, for example, the rationing of available food, clothing and medical supplies. It also permits communal decisions on whether or not to support escape attempts, when these involve, as they often do, predictable reprisals against the whole society.[22]

Even when the society of prisoners cannot be politically organized, obligatory ties are likely to develop between a particular prisoner and each or any of his fellows. Shared suffering is a powerful bond among men and seems to entail—though I cannot specify the method of the entailment—very strong positive obligations to mutual assistance. These bonds seem to arise among groups of men assembled with or without regard to their previous citizenship, though they may be stronger or given practical effect more quickly among prisoners with common political and social commitments.[23] There may be some sense, that is, in which the new society is parasitic on the old. Nevertheless, these new obligations are owed to other prisoners and not to the state from which they come. When the US *Code of Conduct* requires captured soldiers to "keep faith with their fellow prisoners" (Article IV), it does little more than point to the specific context within which obligations have to be negotiated. It does not strengthen the ties, nor determine their precise character. And insofar as the obligations of prisoners to each other are enforced, later on, in state or army courts, this must be by virtue of a kind of proxy from the (now dissolved) society of prisoners.[24] Such enforcement can be justified, at least in part, by the need to prevent acts of private revenge, but not by the unqualified assertion that the state's writ runs in limbo.

IV. THE OBLIGATION TO TRY TO ESCAPE

We do admire prisoners who actively resist their captors, and such men were thought admirable even before the ideological struggles of the present day provided new motives for continuing the fight after the battle. There is a large literature, dating chiefly from the two World Wars, which documents and celebrates the efforts of prisoners of war to escape and harass the enemy. And this literature suggests that such efforts have been felt as obligations, at least by a minority of the prisoners. Moreover, the detaining powers expect (some) prisoners to try to escape; their military authorities often regard the attempt as honorable and clearly accept the necessity of establishing a secure quarantine. Thus General Clark's statement of the "old-fashioned" view: that prisoners must be maintained "and guarded." The same sense has worked itself into international law, as I have already indicated. But it is worth stressing again that the security of the right to try to escape (which is implied by the rule against punishing escapees and obviously does not include a right to succeed in the attempt) is dependent on the relative infrequency of its exercise. If attempts to escape are common, if whole camps are organized for the sole purpose of facilitating escape, as were many British officers' camps during the two World Wars,[25] then benevolent quarantine will almost certainly break down. Its cost to the captors will simply be too high, and they can argue that they had reckoned on a much lower cost when they accepted the surrender of the men in question. Thus the attempts of states like our own to require soldiers to "make every effort to escape" (Article III of the *Code of Conduct*) might well be disastrous for the soldiers, if they took the requirement more seriously than in fact they do. When we admire the men who

[21] Geneva Convention I, Article 79. Resistance groups within the camp can also be organized democratically; thus an American group in Korea, one of whose members is quoted in Biderman, *March to Calumny*, p. 171: "We . . . agreed there would be no such thing as a leader. Before we would do anything, it would go before a vote and the majority would rule."

[22] These decisions can be informally made, as in the camp described by A. J. Evans in his memoir of World War I, *The Escaping Club* (London, 1922), pp. 180–181: "Escaping came before everything, and was an excuse for any discomforts which one or two members might bring on the rest of the community. If you wished for help, almost any man in the fort would have helped you blindly, regardless of consequences."

[23] P. R. Reid discusses some of the problems that arose in the multi-national camp that the Germans ran for confirmed escapers at Colditz in World War II, in *Escape from Colditz* (New York, 1956).

[24] Sometimes prisoners set up their own courts in the camp, but these are not encouraged by the authorities on either side. See Pugh, "The Code of Conduct," pp. 683n. and 702.

[25] ". . . nearly everyone was working in some way on the X (escape) organization," Paul Brickhill, *The Great Escape* (New York 1967) p. 42; ". . . we pooled our knowledge. The camp was nothing less than an escaping club," Evans, *op. cit.*, p. 68. (The best books on escape are almost invariably written by British officers.)

"make every effort," we are really admiring heroes. It seems best to say of them, whatever they say of themselves, that their efforts are above and beyond the call of duty.

Escape is heroic only in part because of the physical risks involved, though these can be considerable, as a famous incident from World War II makes clear. In 1944, some 80 British officers escaped through a tunnel from the German camp at Sagan. All but three were eventually recaptured, and 50 were shot, apparently on direct orders from Hitler. Under the international laws of war, these executions constituted murder, and after the war the men who carried them out were tried and, some of them, executed in their turn.[26] That surely was justice done, yet it is hard to imagine a detaining power that would not go to considerable lengths to deter such mass escapes—for they tie down large numbers of troops, demoralize the civilian population, and, if successful, supply valuable information to the enemy. Hopefully, prisoners will not again have to anticipate Nazi brutality. But something less than that (and how much less can never be known) must be regarded as a normal risk. There is more involved, however, in the heroism of escape. Once a prisoner gets out of the camp, he is likely to have to spend weeks behind enemy lines, hiding or in disguise, constantly in danger, expecting discovery and arrest at every moment, able to trust no one, often without adequate food and shelter. For many men the strain must be unbearable, worse than anything that happens in battle. No prisoner can be obligated to undergo such an exacting test of his nerves and his endurance. Escape is precisely the sort of action that a man must choose for himself.

But perhaps this is not always true. In one of the best of the many books on escape, Aidan Crawley, an RAF officer captured during World War II, later a Labor MP and Undersecretary of State for Air, argues "that should a prisoner see a reasonable chance of escape, it [is] his duty to take it. Suppose a prisoner had been abandoned by his guard and deliberately sought captivity again, he would have been the equivalent of a deserter."[27] Perhaps so, but in this example, it should be noted, captivity is ended without any effort by the prisoner himself. Whether he then resumes his former military role is hard to say. It is certainly possible, but if we imagine such a man seeking refuge in the home of a friendly farmer and remaining there for the rest of the war, I doubt that we would have to say, or want to say, that he had deserted. Crawley attempts to argue that there was a duty to attempt escape even when a prisoner was securely held in a camp, but he fails to argue this with much conviction. And in fact, despite the military ethic, the sense of an obligation to escape does not seem to have been widely or deeply felt, though it is almost always acknowledged in the memoir literature. "I am sure," writes one of the bravest of British escapers, "that the majority of the men who sought to escape did it for self-preservation. Instinctively, unconsciously, they felt that resignation [to an indefinite imprisonment] meant not physical, but mental death. . . ."[28] But what of the men who did not feel this way, who decided that they could make a worthwhile life even in the prison camp?

Crawley provides one of the most sympathetic accounts of these men, and he is worth quoting at length:

> From the first moment of captivity . . . there began in every prisoner's mind a conflict which lasted often until the day of liberation. Should he, or should he not try to escape? Ought he to spend his time in what would almost certainly be fruitless endeavor, or should he use it to equip himself to be a better citizen later on? There were many who from the start decided on the course of self-improvement. With great force they argued that, however heroic escape might appear, the odds against success [were] so enormous that the realistic and truly patriotic thing to do was to put the idea out of their minds. . . . To every thinking man, the wisdom of spending years in hopeless effort must at some time have seemed questionable and no one could blame those who decided escape was not worthwhile. Provided they stuck to their guns and held their point of view with tolerance, they were often most valuable members of the community.[29]

To call such refusals "patriotic" is to put a good construction on them, but not necessarily the only good, or the best, construction. Refusal to escape might also express a man's sense of belonging, now, "to humanity and to himself," or his sense of personal obligations to family and friends. It might express his solidarity with his fellow prisoners, or with some of them doomed to remain in the camp for the duration of the war and in need, perhaps, of his help building a decent community.[30] And it might express his

[26] Brickhill, *op. cit.*, pp. 211–223.

[27] *Escape from Germany* (New York, 1956), p. 8.

[28] Reid, *op. cit.*, p. 35.

[29] *Op. cit.*, p. 7.

[30] Exactly what this means and how such a community can be built are the main concerns of J. Davidson Ketchum's fine sociological study, *Ruh-*

human fear or his relief at finding himself relatively safe and far from the battlefield. Surely if a man makes himself "a valuable member of the (prison) community," even these latter reasons need not be judged any more harshly than the others.

Curiously enough, one of the best arguments on behalf of escape, even mass escape, is that the attempt is itself of value to the society of prisoners. Crawley has once again put the argument very well:

> Most important of all, the effort to escape preserved the morale of the prisoners themselves. One of the great difficulties of prison life was that almost all effort, apart from the business of feeding and existing, was directed to goals which could be achieved only in the indefinite future. The mere fact that in preparing for a mass escape hundreds of people were co-operating in an enterprise which held the prospect of an immediate result was the best tonic a prison camp could have. In building a tunnel, making clothes, forging papers, or preparing maps, men took part in a common effort and once again got the feeling of serving a community.[31]

It may be that this new community is parasitic upon the old: that escape can be a common enterprise of the prisoners only because it is an enterprise of value to the state and army from which they commonly come. But this is not necessarily so. In Pierre Boule's novel (and in the better film by David Lean), *The Bridge Over the River Kwai,* the common enterprise of the prisoners is not of value to their state.[32] On the contrary, it poses a serious threat and even requires a military response. Yet it has an effect, and a plausible effect within the novel and the film, identical to that which Crawley describes. In the River Kwai case, the obligations that may arise to work on the bridge, and to work strenuously, are owed to one's fellow prisoners and to no one and nothing else. There is a certain absurdity in the spectacle of hundreds of men working with such zeal on a bridge that they "really" (as members of the conventional world, citizens and soldiers) don't want built. But this is an absurdity always possible in limbo, and it is the achievement of the novel and film to have evoked it. Nor is it clear that enterprises of more conventional value are very much less absurd. The extraordinary discipline and skill, the zeal and the sheer genius, that went into the British escape from Sagan were surely out of all proportion to its salutary effects upon the British war effort. George Harsh nicely summed up "the great escape" when he wrote of the prisoners involved that "they proved for all posterity that men, working together, can dig a damned deep hole in the ground. . . ."[33] Yes, they did prove that, and that hole was their triumph over captivity, their human triumph, as was the bridge for another group of men; and they dug it first of all for each other.

V. CONCLUSION

In limbo obligations are, by and large, not given, not established by any sovereign state, not waiting for discovery; they must be improvised. Attempts to specify them that go beyond those negative obligations which all citizens share are likely to be impositions on the freedom of the men involved and sometimes a cruel threat to their security. That is why they are largely ignored by the prisoners. Citizens and soldiers, safe or relatively safe among their comrades, have every right to hope that prisoners will form some sort of society, find an effective way of making collective decisions, and remain faithful to one another. Armies have every right to train their soldiers to behave in that way. But to insist that this is what prisoners ought to do is to say nothing more than that they owe this much to one another, and finally it is up to the prisoners to say that themselves. Citizens and soldiers, and presidents and generals, may also hope that some of the prisoners will be heroes, that they will discover uncommon resources of courage and endurance, and unilaterally re-establish obligations that in fact have lapsed. But such heroism, it must be recognized, is also a denial of captivity, perhaps even a refusal of benevolent quarantine, and so a potential threat to the status of all those prisoners who don't choose to be heroes. All the more reason, then, that heroism should not be demanded by those of us who cannot expect to feel its consequences.

The fight does not continue after the battle, not for the men who have been captured, unless they choose together to fight on and accept the risks that choice involves. They have every

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leben: A Prison Camp Society (Toronto, 1965). In his postscript to this book, Robert MacLeod argues that the "prevalence of the idea of escape" may be a sign of group disintegration (or, presumably, of social underdevelopment), p. 353. But it is important to note that Ruhleben was a camp for enemy aliens, not soldiers.

[31] *Op. cit.,* pp. 9–10.

[32] They are forced, in violation of the Geneva Conventions, to build a bridge which is of military value to the enemy.

[33] "Introduction" to Brickhill, *op. cit.,* p. 9.

right to choose differently, and this is what most of them do, opting for benevolent quarantine whenever it is offered. It is the responsibility of civilized men to insist that it always be offered and to repudiate decisively and without compromise the very idea of a "total war" to be fought within as without the bounds of the prison camp. The requirements of humanity are clear enough: detaining authorities must assume, and the prisoners' home states must give them reason to assume, that prisoners are not combatants and are no longer required by their citizenship to fight. Then they may safely be treated, and then they ought to be treated, if not like "citizens of the world," at least like men entitled to rest for a while in limbo.

THE GROWTH OF THE SENIORITY SYSTEM IN THE U. S. HOUSE OF REPRESENTATIVES*

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I. THE SIGNIFICANCE OF SENIORITY

Popular discussions of the internal management of the U.S. House of Representatives in the present era generally give great weight to the ubiquity and arbitrariness of the seniority system as a significant determinant of outcomes there. Careful attention to the scholarly literature, however, should long since have modified this view. For it appears that except for relatively unimportant matters such as the allocation of office space on Capitol Hill,[1] the criterion of seniority is generally intermingled in House decision-making with a great many other crite-

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[1] The Speaker has executive responsibility for the House side of the Capitol, including the three House Office Buildings, which he exercises through his chairmanship of the House Office Building Commission. The rules currently in effect on the allocation of office space are: Each member is entitled to the space he currently occupies. Vacancies as they occur are allocated to claimants in order of their seniority. Entering freshman members are ordered by lot and given their choice of the remaining office space at a predetermined time at the beginning of each Congress. Any extra office space over and above members' suites and rooms set aside for the offices of specific committees are allocated at the discretion of the Speaker. See Donald G. Tacheron and Morris K. Udall, *The Job of the Congressman* (Indianapolis: Bobbs-Merrill, 1966), pp. 41–43, 277–279.

Until the opening of the Rayburn Building, the third House Office Building, in 1965, no clear patterns of preference for office space were discernible. Some members liked to be near the Capitol; others preferred more remote locations, away from the crowds of tourists. Many members had their offices near the rooms of the committees to which they were assigned, which were spotted through both "old" buildings. Some members preferred the more modern facilities of the Longworth Building (opened, 1926). Others agreed with Clem Miller: "I don't care for it. Low ceilings, panelled walls smacking of a 1925 corporation lawyer. . . . I am in the Old Office Building [built 1909], a huge square with a hollow center, wide corridors, high ceilings. Old-fashioned railroad car carpeting. High-backed leather chairs. If you take away the electric typewriters, you move right into the world of William Howard Taft." *Member of the House* (John W. Baker, ed.), (New York: Scribner's, 1962), p. 27.

The Rayburn Building has changed some of this: about half of the House, including most of the senior men, are in it. Rayburn Building offices are larger and more commodious; the much greater inconvenience in getting from the Rayburn Building to the Capitol is somewhat offset by the installation of a Toonerville trolley in the subway that runs between this building and the Capitol building. The two older office buildings are being remodeled to provide more or less equal space for everyone once again.

ria of choice, and the business of choosing is not automatic, but remains in the hands of persons having some considerable discretion. This, apparently, is the case with respect to such decisions as the allocation of Capitol Hill patronage,[2] the initial assignment of Representatives to committees,[3] the distribution of responsibilities within committees,[4] and the choice of party leaders.[5] The one important area in which seniority is generally followed is the choice of committee chairmen.

[2] There are a number of kinds of Capitol Hill patronage. Employees of the Capitol fall under the control of the majority party. Major employees at the level of Clerk of the House, Sergeant at Arms, Parliamentarian, Doorkeeper and Postmaster are formally selected by party caucus, in fact are usually appointed by the Speaker, and are elected by vote of the whole House. These are more or less permanent career jobs, and most have minority counterparts built into the organization. When party control of the House shifts, the permanent top bureaucrats of the House exchange jobs with their minority "shadows." When a vacancy occurs, the Speaker appoints what amounts to a permanent member of the Congressional bureaucracy. Jobs at the middle level such as assistant doorkeepers, assistant parliamentarians, folding room superintendent, reading clerks, recorders of debate, and so on are also regarded as career jobs, and require the initial sponsorship of Congressmen of the majority party. These jobs also come under the purview of the Speaker. Low level jobs such as elevator operator, mail room employee, page, are also handled by a system of sponsorship and cleared through a committee of the majority party appointed by the Speaker. An informal quota system based in part upon the seniority of members determines the entitlement of members to sponsor candidates for these jobs.

A second form of patronage is provided by the Congressional campaign committees of each party which allocate funds to House incumbents running for reelection. Seniority as such plays little part in the distribution of these funds. See Michael J. Kirwan as told to Jack Redding, *How To Succeed in Politics* (New York: MacFadden, 1964), pp. 9–11; and Hugh A. Bone, *Party Committees and National Politics* (Seattle: Univ. of Washington, 1958), pp. 120–165, esp. p. 147.

[3] See Nicholas A. Masters, "Committee Assignments in the House of Representatives," this REVIEW, 55 (June, 1961), 345–357. Masters gives the following desiderata for appointments to committees: the candidate must show "legislative responsibility," i.e., personal characteristics of flexibility and willingness to follow norms of predictability, reciprocity and courtesy; his district should allow him flexibility in major matters—which means that norms internal to the House must be allowed to weigh heavily in his committee decision making; the geographical balance of committees must be preserved; and the appointment should bear some relationship to the member's chance of being reelected. In addition to these criteria, Masters says: "When two or more members stake a claim to the same assignment, on the ground that it is essential to their electoral success, both party committees usually, if not invariably, will give preference to the member with longer service." State delegations sometimes but not always use seniority as the criterion determining which among their members will lay claim to a seat on a major committee that the state is thought to be "entitled" to. On the whole, large state delegations are much more successful than small states in getting the committee assignments they want for their members. This is true for both parties, and it operates without regard to the seniority of individual members.

[4] Seniority is generally followed pro forma in the granting of subcommittee chairmanships, but not always. Neither Adam Clayton Powell nor Phil M. Landrum received subcommittee assignments, much less chairmanships, when they were each ranking member of the Education and Labor Committee. This was an unusually frank recognition of estrangement between them and their respective committee chairmen. Ordinarily, senior men out of favor with the chairman receive the recognition of a subcommittee chairmanship, but since the membership and the substantive responsibilities of subcommittees are more or less discretionary with the chairman, it is quite easy for the committee chairman to circumvent a subcommittee chairman he dislikes or mistrusts. Thus the operation of seniority in the organization of subcommittees does not necessarily mean very much. See e.g. Richard F. Fenno, "The House of Representatives and Federal Aid to Education," in Robert L. Peabody and Nelson W. Polsby (eds.), *New Perspectives on the House of Representatives* (Chicago: Rand McNally, 1963), pp. 195–235.

[5] Seniority in a strict sense does not enter in as a criterion for party leadership. To be eligible for consideration as a party leader, a member must have served long enough to be well known; but Carl Albert was tied with nine others for 30th in seniority on the Democratic side of the aisle when he was elected majority leader in a 1962 Democratic caucus. The Republican caucus has twice, in recent years, removed a senior man from the party leadership and replaced him with a more junior one. Factors of personality, sectionalism and ideology seem to matter most in the choice of party leaders—including chairmen of the caucuses, whips, assistant whips, and Congressional campaign committee chairmen. See Nelson W. Polsby, "Two Strategies of Influence in the U.S.

niority seems to play a role of overwhelming significance is in the matter of succession to the chairmanship of committees; this is in turn governed by the custom (not a formal rule) of seniority that guarantees members reappointment to committees at the opening of each new Congress, in rank order of committee service. It is the growth of this method of selecting committee chairmen in the House that is the subject of this paper.

The importance of a committee chairmanship varies, to be sure, with the importance from session to session of the legislation considered by the committee. It is also significantly modified by committee customs and practices, since formally, committee chairmen are subject to the will of committee majorities. Committees vary in the discretion they leave to their chairmen, and many examples exist of committee chairmen who were bypassed or defeated in specific controversies when they had gone "too far"—or not far enough.[6] Nevertheless, it would be foolish to deny that committee chairmen, on the whole, have enormous power over the activities of their committees, and over the ultimate output of Congress. It is generally the chairman who sets the pace that determines the total workload of the committee, the chairman who hires and fires staff, the chairman who forms subcommittees and assigns them jurisdictions, members and aides. It is generally the chairman who manages the most important bills that are assigned to his committee, and at his own option oversees the endless tinkering with the substance of major bills that goes on in committee, on the floor, and in conference. All this is common knowledge; it provides a backdrop for the continuing interest of scholars and men of affairs in the people who become committee chairmen, and in the process that selects them.

Parliamentary bodies differ in the extent to which they divide labor and decentralize power. The two are, moreover, not precisely synonymous, since an efficient division of labor can be achieved while retaining centralized control. Such is the case, for example, in the Italian Parliament and the Japanese Diet, where strong committee systems are combined with strict party government.[7] Decentralization of power without division of labor leads to parliamentary ineffectiveness, as in Colombia and Ecuador.[8]

A more familiar pattern than either, one congenial to the classic British parliamentary features of ministerial responsibility and strict party regularity, is the centralization of power in the hands of party leaders and a division of labor in the legislature that is rudimentary at the very most. Thus Bagehot's magisterial classification of the 19th century House of Commons as an "electoral chamber" that passes upon the legitimacy of the sitting government but engages not at all in policy-making.[9] At the opposite extreme is a legislature where labor is divided and where power is likewise decentralized, such as in the case of the contemporary U.S. House of Representatives.

Operational indices of these characteristics

House of Representatives: Choosing a Majority Leader, 1962," in Peabody and Polsby, *op. cit.*, pp. 237–270; Robert L. Peabody, "The Ford-Halleck Minority Leadership Contest, 1965" *Eagleton Institute Case in Practical Politics,* No. 40 (New York: McGraw-Hill, 1966); and Peabody "Party Leadership Change in the United States House of Representatives" this REVIEW, 61 (September, 1967), 675–693.

[6] Examples would include the following: There was a revolt against Chairman Dan Reed, of the Committee on Ways and Means in 1953. See Raymond A. Bauer, Ithiel de Sola Pool and Lewis Anthony Dexter, *American Business and Public Policy* (New York: Atherton, 1963), p. 33; a revolt against Chairman Clair Hoffman of Government Operations in the same year; a number of small-scale rebellions against two successive chairmen of the Committee on Education and Labor (see Fenno, *op. cit.*); two major rebellions against Judge Smith of the Rules Committee, one in 1961 that packed the Committee against him, and one in 1965 that established the 21-day rule; in 1965–66 and again in 1967 a revolt against Chairman Wright Patman of the Banking and Currency Committee; and in 1967 a revolt against Chairman William Colmer of the Rules Committee. Both these latter revolts eventuated in important changes in committee rules.

[7] See Giovanni Sartori (ed.) *Il Parlamento Italiano, 1946–1963* (Napoli: Edizioni Scientifiche Italiane, 1963), esp. pp. 246–248; Robert E. Ward, "Japan" in Ward and Roy Macridis (eds.) *Modern Political Systems: Asia* (Englewood Cliffs: Prentice-Hall, 1963), pp. 92–94; Hans H. Baerwald "Parliament and Parliamentarians in Japan," *Pacific Affairs* 37 (Fall, 1964), 271–282; Chitoshi Yanaga, *Japanese People and Politics* (New York: Wiley, 1956), p. 255; Nobutaka Ike, *Japanese Politics* (New York: Knopf, 1957), pp. 181–182.

[8] See James L. Payne, *Patterns of Conflict in Columbia* (New Haven: Yale U. Press, 1968), and George Blanksten, *Ecuador: Constitutions and Caudillos* (Berkeley: U. of California Press, 1951), pp. 100–119.

[9] Walter Bagehot, *The English Constitution* (London: Collins, 1963. First published 1867), p. 150.

		DIVISION OF LABOR	
		Strong (Strong committees)	*Weak* (Weak committees)
POWER OVER POLICY	*Centralized* (Strong party leadership)	Italian Parliament Japanese Diet	British House of Commons
	Decentralized (Weak party leadership)	U. S. House of Representatives	Columbian and Ecuadorian Legislatures

FIGURE 1. Relationships Between the Division of Labor and the Centralization of Power over Substantive Policy in Legislatures.

can be suggested, and in so doing, the importance of seniority can be clarified. Briefly, the strength of a division of labor can be defined as the extent to which legislatures rely upon standing committees to conduct their business. When committees are not relied upon, centralization of power can be observed in the extent to which the initiation and modification of policies eventually enacted by the legislature is concentrated in a few hands (normally the hands of party leaders) or dispersed into many. When committees are relied upon, whether they are populated by automatic or discretionary means, and whether discretionary means are at the discretion of few or many members, is an acceptable test of the centralization or decentralization of power.

From these stipulations, we can deduce that an automatic means of assigning members to committees is a sufficient, though not a necessary, condition for decentralization of power, to the extent that committees are heavily relied on in legislatures. A seniority system is automatic. Therefore, in the U.S. House of Representatives, where committees are strong, the seniority system is sufficient to explain the decentralization of congressional power.

The extent of a seniority rule's application may be said to constitute a measure of the allocation of discretion and hence of power as between party leaders and committee chairmen. Previous commentators have agreed upon this even when they agreed on little else.[10] It is of course not the only such measure imaginable, but it is an important one. Committee chairmen subject to the selection of party leaders stand in a different relation to the leadership than chairmen selected by an impersonal process in which the leadership is powerless to interfere. Thus, like pregnancy, seniority is for most purposes a dichotomous variable. When seniority operates as a partial influence upon decision-making rather than as an automatic determinant of committee rank, political influence flows to those empowered to vary the application of the diverse criteria of choice—normally party leaders. When seniority is sovereign and inviolate, power is decentralized to those accordingly protected.

II. WHEN DID SENIORITY BEGIN?

There is disagreement in the literature about how well-entrenched decision making by seniority has been in the House, and for how long. Numerous anecdotes exist, testifying to the violation of seniority in the selection of committee chairmen in former times.[11] Nowadays, the year 1910, which marks the revolt against Speaker

[10] See, for example, on one hand, James K. Pollock, Jr., "Seniority Rule in Congress," *The North American Review*, 222 (1925), 235–245; and on the other, the testimony of George H. E. Smith in the Hearings, Senate Committee on Expenditures in the Executive Departments, *Evaluation of Legislative Reorganization Act of 1946* (80th Congress, 2nd Session) Washington: Government Printing Office, 1948), pp. 61, 71–73, 184–185. These are collected in Walter Kravitz, "Seniority in Congress" (mimeo.), Library of Congress Legislative Reference Service, November 7, 1961.

[11] See, for example, Chang-wei Chiu, *The Speaker of the House of Representatives Since 1896* (New York: Columbia Univ. Press, 1928), pp. 64–68; George B. Galloway, *History of the U.S. House of Representatives* (House Document 246, 87th Congress, 1st Session) Washington: Government Printing Office, 1962), 62–63; Pollock, *op. cit.*

Cannon, is often mentioned as the effective beginning of the system. George Galloway says: "Seniority in point of service has been the prevailing principle governing both committee assignments and the selection of chairmen since ... 1910 in the House."[12] However, James K. Pollock maintains, "From early years the rule that has been generally followed in the appointment of committees is the so-called seniority rule. It has made no difference whether Republicans or Democrats controlled Congress; the method of selection has been the same.... The practice ... is a universal one. . . ."[13] Chang-wei Chiu shows that in the four Congresses just prior to 1910 (the 58th to 61st), with respect to ten important committees, many appointments were made at the discretion of the Speaker, but "more than four-fifths of the members . . . were appointed under the seniority rule."[14] Writing in 1884, Woodrow Wilson says: "The Speaker is expected to constitute the committees in accordance with his own political views . . . [and he] generally uses his powers as freely and imperatively as he is expected to use them." But he also writes: ". . . by custom, seniority in Congressional service determines the bestowal of the principal chairmanships."[15]

These writers seem to be reporting upon a situation in which seniority figured as a criterion of choice, but where discretion was left to party leaders. Hence they are describing a period preceding the advent of a seniority *system* such as operates today. Earlier, in 1865, there seems to be no doubt or ambiguity about the locus of power to allocate committee chairmanships. W. R. Brock writes, "The Speaker was a man of considerable influence and much depended upon him. He appointed members of committees, acted in effect as majority leader in the arrangement of business, recognized or failed to recognize would-be participants in debate, interpreted the rules of the House, and dealt with the numerous points of order. . . . A good deal of power rested with the chairmen of committees, though perhaps less than in more recent times. . . ."[16] Likewise, in 1871, the Speaker constituted the committees. A member of Speaker Blaine's household wrote in a family letter, "Your father sits here at the table toiling away over his committees. . . . As fast as he gets them arranged, just so fast some after-consideration comes up which disarranges not one, but many, and over topples the whole row of bricks. It is a matter in which no one can help him . . . [He] had wool and cotton manufacturers to meet in Boston, dinners, breakfasts, and lunches . . . to give and take in New York, and, over and above all, pressures, to resist or permit, of Congressional committees."[17]

There is much other commentary on the wide discretion of the Speaker before 1910. George Rothwell Brown observed, "The right of the Chair to delay making committee assignments for long periods was recognized, Mr. Reed, for example, refraining from appointing the committees in the 55th Congress for 131 days."[18] James Bryce, in the revised edition of *The American Commonwealth* (1905) says: "In America the Speaker has immense political power, and is permitted, nay expected, to use it in the interests of his party . . . His most important privilege is . . . the nomination of the numerous standing committees. . . ."[19] Alexander, writing just after the 1910 revolt against Cannon, says, "The Speaker's greatest power has its source in his authority to appoint committees."[20]

There is, obviously, an important empirical question to be settled which these quotations, taken all together, raise. This has to do with the extent to which seniority was actually followed in the assignment of committee chairmanships before a hard and fast seniority system was established. The answer to this may, in turn, raise questions about the processes by which seniority came to be established as an inflexible norm in the House of Representatives.

The data we shall presently consider consist of a report on the selection of all chairmen of House standing committees, by Congresses, from the 47th to the 88th Congress (1881–1965). This information was gathered from *The Congressional Directory* for each Congress. From the 75th to the 88th Congress, committee

[12] George B. Galloway, *Congress at the Crossroads* (New York: Crowell, 1946), p. 187. See also George Goodwin, "The Seniority System in Congress," this REVIEW, 53 (June, 1959), p. 417.

[13] Pollock, *op. cit.*, pp. 235–236.

[14] Chiu, *op. cit.*, p. 71.

[15] Woodrow Wilson, *Congressional Government* (New York: Meridian, 1956; First published, 1884), pp. 82, 85.

[16] W. R. Brock, *An American Crisis* (New York: St. Martin, 1963), pp. 58–59.

[17] Gail Hamilton, *Biography of James G. Blaine* (Norwich, Connecticut: Henry Bill, 1895), pp. 260, 263. A slightly garbled version is in DeAlva Stanwood Alexander, *History and Procedure of the House of Representatives* (Boston: Houghton Mifflin, 1916), p. 69.

[18] George Rothwell Brown, *The Leadership of Congress* (Indianapolis: Bobbs-Merrill, 1922), p. 109.

[19] James Bryce, *The American Commonwealth* (New York: Macmillan, 1905), Vol. I, pp. 138–139.

[20] Alexander, *op. cit.*, p. 67.

memberships are listed and the party affiliations of members designated in the *Directory*. Prior to the 75th Congress, the *Directory* lists committee members without designation as to party. However, from the 74th back through the 55th Congress, the committee lists were, with a few discrepancies, columnized by party affiliation. After an unsystematic but reasonably thorough cross-check in the *Biographical Directory of the American Congress*,[21] these columnizations were followed. From the 54th to the 47th Congress, a systematic and complete checking of names in the *Biographical Directory* enabled us to establish party designations.

The Congressional Directory was published for each Congress we studied; but its publication schedule has been erratic. The most common practice seems to have been for the *Directory* to appear once for each session of every Congress, that is, annually. However, in some years, the first edition of the *Directory* appeared before committee lists were established. Since we proposed to sample committee lists only once per Congress, we adopted the policy of consulting the first edition of the *Directory* available to us for each Congress that contained full listings of committee members. Thus changes in chairmanships within Congresses may have been lost to us.[22]

The *Directory* is neither a wholly uniform, nor a strictly official source; much superior would be the committee lists appearing in the *Journals* or the assignments as they are recorded in the verbatim accounts of debates, currently entitled *The Congressional Record*. However, the former of these sources is not widely available, and the latter is not indexed under the rubric "Committee Assignments." Hence both are exceedingly cumbersome for the purposes of research and, in any event, would in all probability disclose only the most insignificant variations from the *Directory*.

Our most important finding can be summarized in a single table which shows for each Congress the number of times that seniority was and was not followed in the appointment of committee chairmen for each of 40 Congresses from 1881 to 1963. When a committee chairman met the following conditions he was classified as a chairman by virtue of seniority: In the preceding Congress he was the chairman, or if his party was in the minority, the ranking member, or he had been on the committee in the previous Congress and all those listed above him for that Congress were no longer serving in

TABLE 1. THE GROWTH OF SENIORITY 1881–1963

VIOLATION OF THE SENIORITY OF CHAIRMEN, BY COMMITTEE, U.S. HOUSE OF REPRESENTATIVES BY CONGRESS

Congress	Year	Speaker	Party	Seniority Followed	Seniority not Followed	Total Committees*
47	1881	Keifer	R	2	37	39
48	1883	Carlisle	D	8	30	38
49	1885			21	19	40
50	1887			20	21	41
51	1889	Reed	R	20	27	47
52	1891	Crisp	D	12	35	47
53	1893			25	24	49
54	1895	Reed	R	13	39	52
55	1897			36	16	52
56	1899	Henderson	R	42	15	57
57	1901			49	8	57
58	1903	Cannon	R	43	11	54
59	1905			51	8	59
60	1907			45	13	58
61	1909			42	18	60
62	1911	Clark	D	25	27	52
63	1913			33	20	53
64	1915			50	6	56
65	1917			45	10	55
66	1919	Gillett	R	35	22	57
67	1921			44	15	59
68	1923			40	17	57
69	1925	Longworth	R	37	22	59
70	1927			43	1	44
71	1929			38	7	45
72	1931	Garner	D	27	18	45
73	1933	Rainey	D	38	7	45
74	1935	Byrns/Bankhead	D	32	13	45
75	1937	Bankhead		42	4	46
76	1939	Rayburn	D	37	9	46
77	1941			39	7	46
78	1943			34	11	45
79	1945			37	9	46
80	1947	Martin	R	9	4	13
81	1949	Rayburn	D	19	0	19
82	1951			18	0	18
83	1953	Martin	R	17	1	18
84	1955	Rayburn	D	19	0	19
85	1957			19	0	19
86	1959			19	0	19
87	1961	Rayburn/McCormack	D	20	0	20
88	1963	McCormack	D	20	0	20

* Total equals the number of committees in the Congress minus the number of new committees and the number of committees in which the three top members in the previous Congress are absent in the present Congress. The latter are excluded because we have no data on men below the number three position in the previous Congress.

[21] *Biographical Directory of the American Congress*, 1774–1961 (Washington, Government Printing Office, 1961).

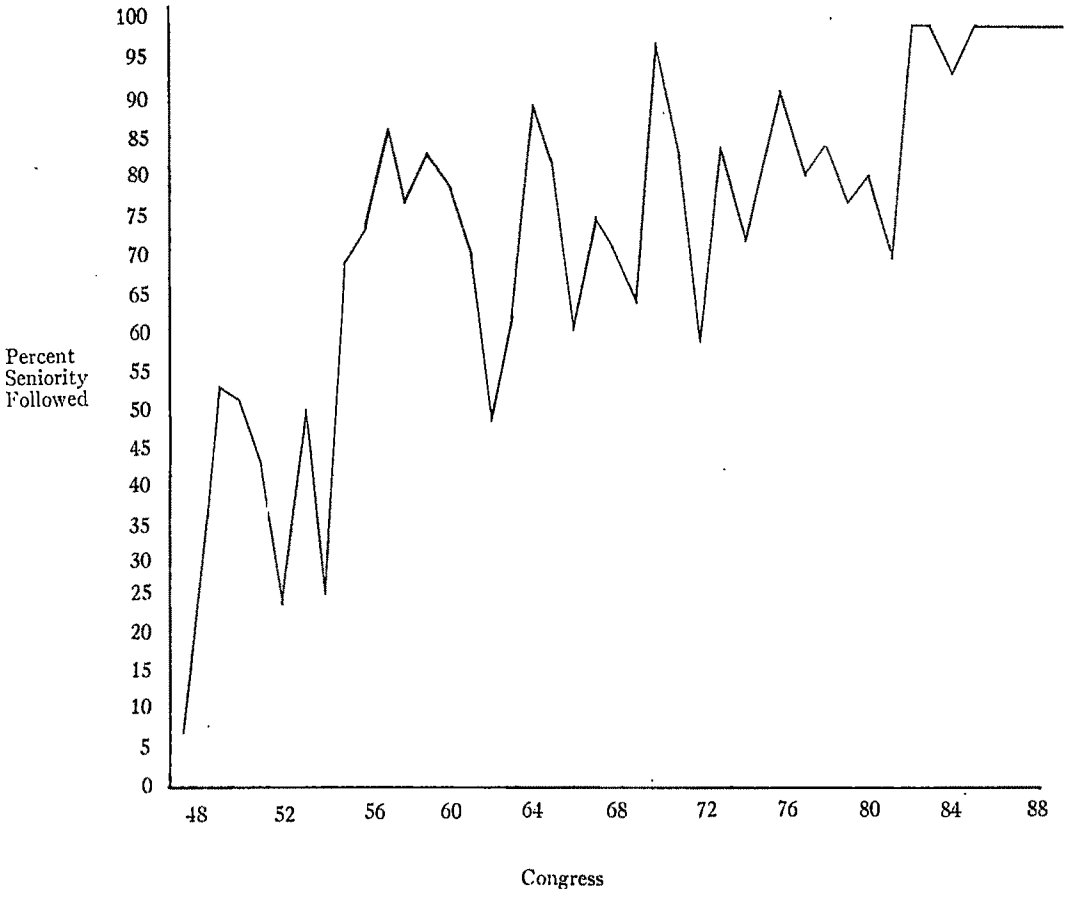
[22] We doubt that this would make much difference in our findings. Paul DeWitt Hasbrouck says, "Even Speaker Cannon did not go so far as to remove a committeeman in the middle of a Congress." *Party Government in the House of Representatives* (New York: Macmillan, 1927), p. 50.

Congress. If a chairman could meet none of these conditions, he was classified as having been designated by some method other than seniority. These simple rules of classification yielded Table 1.

The general trend, over the 80-year period, is toward the increasing use of seniority as a determinant of committee chairmanships, as Figure 2 shows.

FIGURE 2. The Growth of Seniority: 1881-1963

Percentage of Committees on which Seniority was followed in the Selection of Chairman, by Congress*



* Percentages were calculated as follows: Seniority (from Table 1) divided by Total Committees (from Table 1) for each Congress.

Several different kinds of seniority violation are summarized in Table 1. Some members of Congress who by our criteria should have been committee chairmen were demoted. Others stayed at the same rank but were jumped by members from below them on the list or from elsewhere. Others left their committees. Some of these received new committee assignments, others did not. Of those who received new assignments, some did as well as or better than if they had stayed put.[23]

[23] In classifying committees by order of importance (and therefore desirability) we relied upon two sources; Chang-wei Chiu's listing of important committees (*op. cit.* pp. 68–69) for the early period, and the sorting of committees into three classes by the Amended Legislative Reorganization Act of 1946, which provided that 1) Three committees are *exclusive*—namely Appropriations, Rules, and Ways and Means. A member who serves on any of these can serve on no other committee. 2) Ten committees are *semi-exclusive:* members may serve on any one of them and any one of the seven non-exclusive committees. The ten are: Agriculture, Armed Services, Banking and Currency, Education and Labor, Foreign Affairs,

Finally, some members who otherwise might have become, or remained, chairmen were found to hold chairmanships of other committees, or party leadership positions. Thus Table II distinguishes instances where potential chairmen whose seniority was violated seem to have been compensated in the appointment process from instances where no such compensation is visible.

Compensation was not fully in the discretion of House leaders. A senior member who picked between chairmanships was not necessarily in the debt of the Speaker. Appointments to new positions as good as or better than a relinquished chairmanship were undoubtedly negotiated through the party leadership of the majority party (and, after Speaker Cannon established the custom, for minority members in negotiation with the leaders of the minority party) but it is difficult to interpret compensations as entirely within the Speaker's control, since it was necessary for the Speaker to use the appointment power to build a coalition that would support him in office by rewarding his allies and mollifying the leaders of rival factions within his party.[24]

Uncompensated violations of seniority can be interpreted as purer examples of central leadership, since they represent instances where the committee assignment process was used by party leaders to intervene in the careers of members but where no payment in kind can be detected. As Figure 3 makes clear, this sort of intervention declined rapidly over the years surveyed, barely surviving after the 1911 rules changes vesting the Speaker's jurisdiction over committee assignments in party committees on committees.

Compensated violations of seniority, however,

TABLE 2. COMPENSATED AND UNCOMPENSATED VIOLATIONS OF SENIORITY*

Congress	Year	Compensated Violations	Uncompensated Violations (#)	Uncompensated Violations (%)	Total Violations
47	1881	12	25	68	37
48	1883	16	14	47	30
49	1885	7	12	63	19
50	1887	12	9	43	21
51	1889	19	8	30	27
52	1891	22	13	37	35
53	1893	15	9	38	24
54	1895	30	9	23	39
55	1897	11	5	31	16
56	1899	13	2	13	15
57	1901	5	3	38	8
58	1903	7	4	36	11
59	1905	5	3	38	8
60	1907	10	3	23	13
61	1909	8	10	56	18
62	1911	21	6	22	27
63	1913	13	7	35	20
64	1915	6	0	0	6
65	1917	7	3	30	10
66	1919	19	3	14	22
67	1921	15	0	0	15
68	1923	16	1	6	17
69	1925	20	2	9	22
70	1927	1	0	0	1
71	1929	7	0	0	7
72	1931	16	2	11	18
73	1933	7	0	0	7
74	1935	13	0	0	13
75	1937	4	0	0	4
76	1939	9	0	0	9
77	1941	6	1	14	7
78	1943	10	1	9	11
79	1945	9	0	0	9
80	1947	4	0	0	4
81	1949	0	0	0	0
82	1951	0	0	0	0
83	1953	1	0	0	1
84	1955	0	0	0	0
85	1957	0	0	0	0
86	1959	0	0	0	0
87	1961	0	0	0	0
88	1963	0	0	0	0

* Uncompensated violations of seniority occur when members eligible to be chairman are not and meet one or more of the following conditions: Demoted, Jumped, Received no new assignment, or Held a chairmanship on a less desirable committee.

Compensated violations occur when potential chairmen receive or hold better assignments, or equal or better chairmanships or are elected to party leadership.

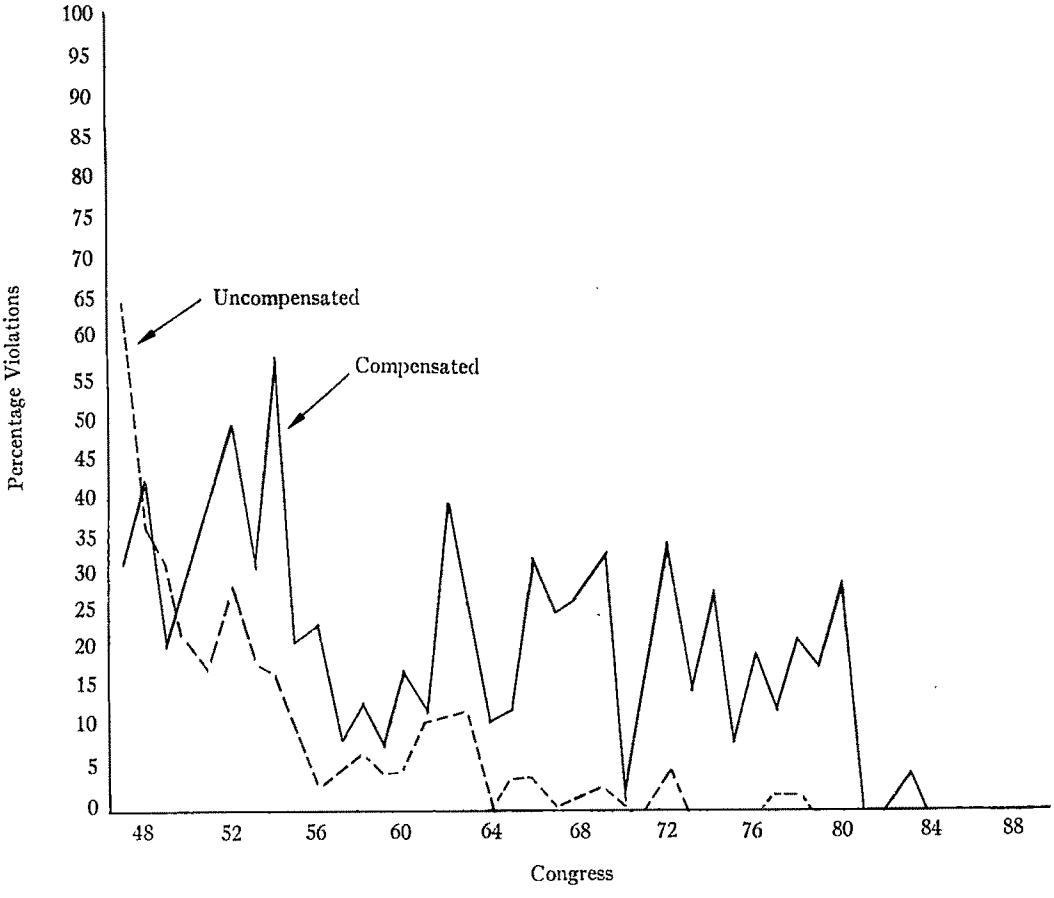
For rankings of committees, see footnote 23.

Interstate and Foreign Commerce, Judiciary, Post Office and Civil Service, Public Works, and Science and Astronautics. 3) Seven committees are *non-exclusive*. A member may serve on any two of these seven, or on any one plus one of the ten-semi-exclusive committees. The seven are: District of Columbia, Government Operations, House Administration, Interior and Insular Affairs, Merchant Marine and Fisheries, Un-American Activities, and Veteran Affairs. Chiu mentions four committees not listed above as having some importance in the period (1896–1928) covered by his study. These are committees on Military Affairs, Naval Affairs, Post Offices and Post Roads and Rivers and Harbors. For our purposes, we considered these four semi-exclusive committees.

A total of 57 standing committees other than the ones mentioned in our sources also existed at some point in the period covered by our study. These were added to the non-exclusive committees for our purposes. In general, changes in the total numbers of committees over the years (until the major consolidation of 1946) reflect changes in the number of minor committees.

[24] For examples, see Abram and Cooper, *op. cit.*, pp. 63 ff.

FIGURE 3. Compensated and Uncompensated Violations of Seniority, 1881-1963



survived well into the 20th century. Table 3 demonstrates that most of these violations occurred on committees whose chairmen had become chairmen of committees of equal or greater importance. Less significant numerically are potential chairmen of insignificant committees who take up appointments instead on more important committees. Before 1911, these appointments to new committees were not necessarily at the bottom; committee rank was assigned at the Speaker's discretion—although this was not frequently used to disturb committee rank orders.

Seniority is now strictly observed in the placement of new members, which means that the costs of transferring from a chairmanship to membership on a better committee have risen over the years. Even so, a compensated violation of a chairman's seniority can be recorded for the 91st Congress. In the waning days of the 90th Congress Rep. Omar Burleson of Texas gave up his chairmanship of the moribund Committee on House Administration to fill a vacancy on the Ways and Means Committee—where he will rank below Democrats far junior to him in House service. For obvious reasons, this is not a frequent occurrence.

Almost as infrequent, indeed, is the transfer of any member from the middle or top range of any committee to the bottom of any other, no matter what the disparity in the prestige of the two committees. When such a transfer does take place, it is generally at the request either of party leaders or of the leaders of a particularly large and cohesive state delegation that can find no other or more satisfactory way of exercising its option to occupy a seat on a key committee.

Compensated violations, in short, do not reflect the concentration of political power in any reliable way, since they may occur as the outcome of bargains between party leaders and members, but more often involve a choice between chairmanships. Uncompensated violations, on the other hand, disclose more about options available to party leaders. The sharp decline in the frequency of this sort of violation during the

TABLE 3. COMPENSATED VIOLATIONS

Congress	Year	Chairman of Equal or Better Committee		Better New Assignment		To Leadership		Total Compensated Violations
		(#)	(%)	(#)	(%)	(#)	(%)	
47	1881	8	67	3	25	1	8	12
48	1883	15	94	1	6	0	0	16
49	1885	7	100	0	0	0	0	7
50	1887	8	67	4	33	0	0	12
51	1889	12	63	7	37	0	0	19
52	1891	13	59	9	41	0	0	22
53	1893	6	40	9	60	0	0	15
54	1895	25	83	5	17	0	0	30
55	1897	8	73	3	27	0	0	11
56	1899	10	77	3	23	0	0	13
57	1901	3	60	2	40	0	0	5
58	1903	2	28	4	57	1	14	7
59	1905	1	20	4	80	0	0	5
60	1907	8	80	2	20	0	0	10
61	1909	5	62	3	38	0	0	8
62	1911	9	43	10	48	2	9	21
63	1913	7	54	6	46	0	0	13
64	1915	3	50	3	50	0	0	6
65	1917	5	71	2	28	0	0	7
66	1919	10	53	8	42	1	5	19
67	1921	10	67	5	33	0	0	15
68	1923	13	81	3	19	0	0	16
69	1925	18	40	2	10	0	0	20
70	1927	1	100	0	0	0	0	1
71	1929	6	86	1	14	0	0	7
72	1931	11	69	4	25	1	6	16
73	1933	5	71	0	0	2	28	7
74	1935	10	77	2	15	1	8	13
75	1937	3	75	0	0	1	25	4
76	1939	6	67	3	33	0	0	9
77	1941	5	83	1	17	0	0	6
78	1943	9	90	1	10	0	0	10
79	1945	8	89	1	11	0	0	9
80	1947	4	100	0	0	0	0	4
81	1949	0	0	0	0	0	0	0
82	1951	0	0	0	0	0	0	0
83	1953	0	0	1	100	0	0	1
84	1955	0	0	0	0	0	0	0
85	1957	0	0	0	0	0	0	0
86	1959	0	0	0	0	0	0	0
87	1961	0	0	0	0	0	0	0
88	1963	0	0	0	0	0	0	0

period covered by our investigation suggests that these options were somewhat circumscribed and could hardly have been exercised whimsically or without cost. A closer look at patterns of violation before 1911, to which we now turn, may suggest ways in which party leaders managed and conserved their options in the committee assignment process.

III. HOW SPEAKERS MANAGED: PATTERNS OF VIOLATION BEFORE 1910

Until the 1910 revolt against Cannon, Speakers held the formal power to name committees from top to bottom, majority and minority. They were expected to use this power to enhance their ability to lead their party to legislative success. Their allies expected to be favored,

their rivals to be placated. The extent to which Speakers fulfilled these expectations is reflected in the pattern of uncompensated seniority violations before 1910.

The Speaker's discretion in the appointment of committee chairmen was not a free good. It had to be employed carefully. Thus, instead of tearing up committee rosters root and branch at the opening of each Congress, Speakers concentrated on a relatively few appointments on only a few committees. Pollock mentions a tabulation by Representative Luce showing "that in the seven appointings that took place from 1896 to 1910 about 407 chairmen were named. Of these 265 were reappointments, 93 were promotions of men already on the committees, and only 49, or an average of seven for each appointing, were new men brought to chairmanships from outside the ranks of the respective committees."[25] Our figures, tabulated on a slightly different base (since we were limited as indicated in the note to Table 1) and classifying as violations instances where potential chairmen were jumped from within the committee as well as cases where they were replaced from outside, are nevertheless confirmatory: for the same seven Congresses (55th through 61st) 304 out of 397 appointments (77%) involved no violations of seniority at all. Only 34 of the 397 (8.5%) were uncompensated violations. For the longer period, 1881 to 1910, 133 of the 750 appointments to chairmanships were uncompensated violations of the seniority principle.

The strategies available to Speakers in the appointment process before 1911 varied according to easily described external conditions. There was, first of all, the problem of creating a coalition, when a new Speaker took control of a new majority after an election which brought a new party to the control of all committees. Under these circumstances a new Speaker had new friends to reward and rivals to neutralize, and so it is not surprising that violations of seniority in these circumstances were higher than in years when the Speaker was new but the majority was not, or when there was no change in leadership. This is the finding expressed in Table 4.

Speakers under these circumstances were also able to replace incumbent chairmen (as contrasted with the more vulnerable men newly eligible for chairmanships) to a degree not possible when there had been no party turnover as Table 5 shows.

When Speakers replaced incumbents, they preferred to move the chairman off the committee rather than keep him on looking over his successor's shoulder; this was less necessary for nonincumbents, as Table 6 suggests.

Table 7 shows a sharp contrast between the treatment of incumbent chairmen and others in

[25] Pollock, *op. cit.*

TABLE 4. WHEN THERE IS A NEW SPEAKER AND A NEW MAJORITY SENIORITY VIOLATIONS ARE FREQUENT 1881–1910

	New Speaker, Party Turnover	New Speaker, but No Turnover	No Turnover
Seniority Followed	25% [55]	77% [83]	69% [287]
Seniority Violated	75% [168]	23% [26]	31% [127]
	100% [223]	100% [109]	100% [414]

TABLE 5. WHEN THERE IS PARTY TURNOVER, THE NEW SPEAKER CAN VIOLATE THE SENIORITY OF INCUMBENT CHAIRMEN WITHOUT COMPENSATION (1881–1910)

	New Speaker Party Turnover	New Speaker No Turnover	No Turnover
Incumbent	58% [40]	17% [1]	26% [14]
Nonincumbent	42% [29]	83% [5]	74% [40]
	100% [69]	100% [6]	100% [54]

TABLE 6. WHEN AN INCUMBENT CHAIRMAN'S SENIORITY IS VIOLATED, HE IS MOVED TO ANOTHER COMMITTEE RATHER THAN DEMOTED

	Uncompensated Violations	
	Chairmen	Nonchairmen
Demoted	20% [11]	61% [45]
Moved	80% [44]	39% [29]
	100% [55]	100% [74]

TABLE 7. IN YEARS WHERE THERE IS NO PARTY TURNOVER, THE POWER OF CHAIRMEN TO RESIST DEMOTION IS INCREASED

	Turnover Years		No Turnover Years	
	Chairman	Nonchairman	Chairman	Nonchairman
Demoted	25% [10]	39% [11]	7% [1]	53% [24]
Moved	75% [30]	61% [18]	93% [14]	47% [21]
	100% [40]	100% [29]	100% [15]	100% [45]

TABLE 8. COMPENSATED AND UNCOMPENSATED VIOLATIONS REMAIN CONSTANT, REGARDLESS OF CHANGES IN MAJORITY PARTY 1881–1910

	New Speaker Party Turnover	No Party Turnover
Violations		
Compensated	59% [99]	61% [93]
Uncompensated	41% [69]	39% [60]
	100% [168]	100% [153]

years when there is no party turnover. Party turnover, in other words, appears in our data as a strong force legitimizing activities that in more "normal" circumstances were not permitted to Speakers.

Even though new Speakers taking control of new majorities violated the seniority of chairmen and potential chairmen much more often than was true in most new Congresses, the ratio between compensated and uncompensated violations remained at about 3 to 2. That is, more violations were compensated than not, even when twice as many chairmanships were changing hands by means other than seniority. This is shown in Table 8.

Clearly, creating a coalition differs in its effects upon committee assignments from situations where a new Speaker had inherited a coalition or was acting to maintain a coalition. When there was a new Speaker but no party turnover, Speakers moved cautiously: these were the circumstances in which there were the fewest violations of seniority, as Table 4 indicates, and in which violations, when they occurred, were overwhelmingly of the compensated type. This is shown in Table 9.

Thus we can conclude that by far the most

TABLE 9. NEW SPEAKERS WITHOUT NEW MAJORITIES COMPENSATE WHEN THEY VIOLATE SENIORITY

	New Speaker No New Majority	Other Cases
Violations		
Compensated	77% [20]	58% [172]
Uncompensated	23% [6]	42% [123]
	100% [26]	100% [295]

leeway existed for Speakers when they came fresh to office on the heels of a party victory establishing a new majority in the House. Violations of seniority in other circumstances were much less frequent, and much more subject to compensation.

This suggests that for the period 1881–1910 a number of small but significant constraints upon the Speaker's *de jure* options were developing. They can be noted in the restraint which seems to have been exercised in the violation of seniority when Speakers acted without a party turnover to legitimize their manipulation of assignments to committee chairmanships.

IV. SPEAKER CANNON: A TRANSITIONAL FIGURE

Speaker Joseph Gurney Cannon (1903–1911) more often than any other Speaker is responsible for exceptions to the patterns we have been describing. This is consistent with an interpretation of the committee assignment process as a method—undoubtedly the most important method before 1911—by which Speakers maintained a supportive coalition in the House in behalf of policies favored by the majority party. When the Progressive movement became powerful at the Republican grass roots, as it did in the 1890's, it was only a matter of time before this was reflected in party insurgency in the House of Representatives.

House chroniclers make it clear that the insurgents' successful challenge to Cannon's leadership in 1910 laid heavy stress upon his arbitrary use of the Speaker's power of appointment. George Rothwell Brown quotes Representative Nelson of Wisconsin:

> "Have we not been punished by every means at the disposal of the powerful House organization? Members long chairman of important committees, others holding high rank—all with records of faithful and efficient party service to their credit—have been ruthlessly removed, deposed, and humiliated. . . ." [Brown continues:] The House knew as it listened to this merciless indictment of the Speakership that it was true. Members were there who had been removed from their committee places, because they had displeased the organization, through the exercise of power which had raised the speakership of Mr. Cannon to an unprecedented height.[26]

George Norris, a Congressman in that era, wrote long afterward:

> Every two years the members knew they were confronted with the appointment of the various standing Committees; and one man, the Speaker,

[26] Brown, *op. cit.*, pp. 158–159.

possessed absolute authority to do what he pleased in these selections. He held in his hands the political life of virtually every member. He could reward the faithful, and he could punish the 'guilty'. I doubt if any Speaker in the history of Congress was as ruthless as Joe Cannon sometimes was.[27]

Booth Mooney says, "[Cannon] stacked the key committees with men who would unquestioningly support everything he favored and oppose everything he was against. He arbitrarily took chairmanships away from members who tried to flout his authority."[28]

Cannon may indeed have been the most ruthless Speaker in House history,[29] but these and other complaints about his "ruthlessness" are hard to substantiate with a casual look at the figures we have. Of the six Speakers between 1883 and 1910, Cannon ranks next to lowest in his overall violations of seniority. (See Table 10.)

TABLE 10. VIOLATIONS OF SENIORITY BY SPEAKER 1881–1910

Speaker	Violations	Violations as a % of all appointments
Keifer (1881–83)	37	92%
Carlisle (1883–89)	70	59%
Reed (1889–91)	27	57%
Crisp (1891–95)	59	61%
Reed (1895–99)	55	53%
Henderson (1899–1903)	23	20%
Cannon (1903–1911)	50	21%

What the data provide are indications of important differences in the timing of his violations of seniority as compared with those of his predecessors. Cannon became Speaker after Speaker Henderson, also a Republican, for somewhat mysterious reasons decided to retire from Congress. While Speakers customarily rearranged committee chairmanships after party turnovers, as we have seen, the custom when there was no party turnover was pretty much to maintain the existing arrangements. In his first term, Cannon succeeded the retired Henderson as chairman of the Rules Committee, a procedure which violated the seniority of John Dalzell (who under Cannon became second ranked member of Ways and Means as well as of Rules) but which was well in keeping with House custom. Then he committed one violation on a semi-exclusive committee, Post Office and Post Roads, and four violations on minor committees. This constitutes the most restrained first term performance of any Speaker, including Henderson, although the latter restricted his eight first term violations to minor committees. In his second term Cannon transferred two incumbents and skipped over three nonincumbents—all on minor committees.

In Cannon's last two terms, there began an increase in factionalism in the Republican Party that solidified into the Progressive and Farmer-Labor insurgent movements. This split in the Republican Party found a focus in hot legislative battles over tariff and conservation measures. Cannon responded in part[30] by *increasingly* tinkering with committee chairmanships. (See Table 11.)

What Cannon did involved a number of innovations as compared with his immediate predecessors. He compensated potential chairmen less often (Table 12). Although he deprived incumbent chairmen without compensating them to about the same degree as his predecessors, (Table 13) he violated seniority without compensation to a much greater extent after his first term (Table 14). He deprived many potential chairmen of major and semi-exclusive commit-

[27] George W. Norris, *Fighting Liberal* (N.Y.: Macmillan, 1946), p. 111.

[28] Booth Mooney, *Mr. Speaker* (Chicago: Follett, 1964), p. 109.

[29] See, e.g., Victor Murdock, "After Cannonism, What?" *The Independent,* September 22, 1910, pp. 622–625, George B. Galloway, *History of the United States House of Representatives, op. cit.,* pp. 51–52; Hasbrouck, *op. cit.,* p. 48.

[30] Charles O. Jones describes a number of the powers available to a Speaker in this era which could be used as strategic resources: "He could appoint committees—including the chairmen, determine the schedule of business, recognize members on the floor, appoint members to conference committees, dispense favors of various kinds. Particularly significant was Speaker Cannon's power as chairman of the Committee on Rules. . . . Speaker Cannon was not above delaying the appointment of committees until his wishes on legislation had been met. In the famous 61st Congress, he appointed the important Rules and Ways and Means committees on March 16, the second day of the session. Most of the remaining appointments had to wait until the Payne-Aldrich tariff bill was in the conference committee—nearly five months after the session began." "Joseph G. Cannon and Howard W. Smith: An Essay on the Limits of Leadership in the House of Representatives" *Journal of Politics* 30 (August, 1968), 617–646. See also Hubert Bruce Fuller, *The Speakers of the House* (Boston: Little, Brown, 1909), pp. 256 ff.

TABLE 11. CANNON COMMITTED A HIGHER PROPORTION OF VIOLATIONS AFTER HIS FIRST TERM THAN ANY OTHER SPEAKER

	Number Post First Term Violations	Total Violations During Speaker-ship	Post First Term Violations as % of Total
Carlisle	40	70	57%
Crisp	24	59	41%
Reed	16	55	29%
Henderson	8	23	35%
Cannon	39	50	78%

TABLE 12. CANNON COMMITTED A HIGHER PROPORTION OF UNCOMPENSATED VIOLATIONS AFTER HIS FIRST TERM THAN ANY OTHER SPEAKER

	Number Post First Term Uncom-pensated Violations	Total Uncom-pensated Violations During Speaker-ship	Post First Term Uncom-pensated Violations as % of Total
Carlisle	21	35	60%
Crisp	9	22	41%
Reed	5	14	36%
Henderson	3	5	60%
Cannon	16	20	80%

TABLE 13. VIOLATIONS BY SPEAKER OF THE SENIORITY OF INCUMBENTS WITHOUT COMPENSATION (BY PERCENTAGES)

	Uncom-pensated Violations of Incum-bents	Uncom-pensated Violations	% 1 of 2
Keifer	13	25	52%
Carlisle	12	35	34%
Reed	5	8	62%
Crisp	12	22	55%
Reed	6	14	43%
Henderson	0	5	0
Cannon	7	20	35%

TABLE 14. CANNON'S UNCOMPENSATED VIOLATIONS OF INCUMBENTS' SENIORITY OCCURS AFTER HIS FIRST TERM AS SPEAKER

	Post First Term Violations of Incum-bents	Total Violations of Incum-bents	% 1 of 2
Carlisle	4	12	33%
Crisp	3	12	25%
Reed	1	6	17%
Henderson	0	0	0
Cannon	6	7	86%

TABLE 15. UNCOMPENSATED VIOLATIONS OF SENIORITY ON MAJOR AND SEMI-EXCLUSIVE COMMITTEES BY SPEAKER

	Uncom-pensated Violations on Major and Semi-exclusive Com-mittees	Total Uncom-pensated Violations	Major and Semi-exclusive as % of Total
Keifer	5	25	20%
Carlisle	10	35	29%
Reed	3	8	38%
Crisp	7	22	32%
Reed	7	14	50%
Henderson	0	5	0
Cannon	7	20	35%

TABLE 16. UNCOMPENSATED VIOLATIONS ON MAJOR AND SEMI-EXCLUSIVE COMMITTEES AFTER SPEAKER'S FIRST TERM BY SPEAKER

	Major and Semi-ex-clusive Commit-tees after First Term	Total Uncom-pensated Violations on Major and Semi-exclusive Com-mittees	% Post First Term
Carlisle	6	10	60%
Crisp	3	7	43%
Reed	0	7	0
Henderson	0	0	0
Cannon	5	7	71%

tees (Table 15) and did so after his first term to an unprecedented extent (Table 16).

Bolles says, "Cannon drove his advantage home in the House by dismissing the most obstreperous progressives and other insurgents from the committees where they could do him harm. Cooper lost his chairmanship of Insular Affairs, and Norris lost his membership on Public Buildings and Grounds. Cannon removed Fowler as chairman of Banking and Currency, and he dropped Lindbergh from Indian Affairs. He put Lenroot to one side by assigning him to the inane Committee on Ventilation and Acoustics."[31] In the Sixtieth Congress (1907) he removed four incumbent chairmen from nonexclusive committees, giving them no new committee assignments. These four removals of incumbents after a Speaker's first term were unprecedented. (See Table 14) At the same time Cannon skipped over John A.T. Hull on Militia and Gilbert Haugen on War Claims—both men from Progressive Iowa, to reward Helvor Stienerson (Minn.) and Kittredge Haskins (Pa.) with chairmanships on these two committees. He also transferred Stevens Henry (Conn.), about to become chairman on Agriculture, to a low ranking position on Merchant Marine and Fisheries and gave Henry's chairmanship to Charles Scott (Kansas).

Insurgents opposed Cannon's reelection to the Speakership in the first session of the 61st Congress (March, 1909), and supported Democratic leader Champ Clark's attempt to change the rules and sharply curtail the powers of the Speaker.[32] Cannon fought back with the main weapon at his disposal. He made John Weeks (Mass.) chairman of the semi-exclusive Committee on Post Office and Post Roads, thus violating John J. Gardner's seniority, filled vacant chairmanships with low ranked members on Banking and Currency and Interstate and Foreign Commerce, and deprived three incumbents and five non-incumbents of their chairmanships on minor committees.

In sum, after a cautious beginning, Cannon committed a higher proportion of violations on major and semiexclusive committees after his first term than any Speaker (Table 16), more post first-term violations of the seniority of incumbents than anyone (Table 14), and more uncompensated violations after his first term than anyone. These irregularities became entwined with Cannon's legislative conservatism in the insurgents' ideology of anti-Cannonism.

Ultimately Cannon's maneuvers were not enough to stem the Progressive revolt in his party. This demonstrated that there were limits to the Speaker's ability to maintain his leadership position by depriving his opponents of chairmanships and awarding them to supporters. Cannon's problem seems to have been that he took over a relatively unified party and had little need at first to build a special supportive coalition. When the party began to split apart in his third term, he responded by attempting to maintain a workable coalition by dumping insurgent chairmen and rewarding loyal Republicans with chairmanships. In so doing he deprived more incumbents and chairmen of major and semi-exclusive committees, than had previously been done in the House. Cannon also resorted disproportionately after his first term to the strategy of demoting chairmen or jumping over potential chairman, rather than moving them to other committees. (See Table 17.) It is costlier to deprive an incumbent than a nonincumbent, a member of an exclusive committee than a minor committee. Likewise, it is costlier to demote than to transfer, and given the customary practice of previous Speakers, costlier to violate seniority after the Speaker's first term. Thus the costs of Cannon's efforts to maintain a supportive coalition are clear and his failure more understandable. It seems likely, then, that it was not the sheer number of his seniority violations that earned Cannon his notoriety as the House's most ruthless Speaker, but how and when they occurred.

Before 1911 each newly elected Speaker enjoyed a period in which he could build a supportive coalition, to maximize his party's legislative success or his own popularity or longevity as Speaker. During this period he could gain supporters by pursuing relatively low-cost strate-

TABLE 17. CANNON NOT ONLY CONTRIBUTES DISPROPORTIONATELY TO THE NUMBER OF UNCOMPENSATED VIOLATIONS OF SENIORITY IN GENERAL AFTER SPEAKERS' FIRST TERMS, BUT ALSO CONTRIBUTES DISPROPORTIONATELY TO THE NUMBER OF DEMOTIONS AND JUMPS

Type of Violation	Uncompensated Violations in Speakers' First and Later Terms					
	Speaker's First Term			After Speaker's First Term		
	1 All Speakers	2 Cannon	% 2 of 1	1 All Speakers	2 Cannon	% 2 of 1
Demotion or Jumps	23	3	13	19	6	32
Transfers to Other Committees	52	1	2	34	10	29

[31] Blair Bolles, *Tyrant From Illinois* (New York: Norton, 1951), p. 195.

[32] See Hasbrouck, *op. cit.*, pp. 4–6.

gies. He could violate the seniority of fewer incumbents than nonincumbents, fewer members of exclusive committees than minor committees, and make fewer changes by internal shifts than transfers. After the initial phase, support became more costly. For each increment of support the Speaker had to expend more resources to employ each possible strategy. The longer the Speaker waited to form his coalition, the more probable it became that he could not afford to do so. Therefore the greater the likelihood he would either have to rely on seniority and give in to the legislative preferences of committee chairman, or resist their preferences and risk losing party support.

Similarly, the earlier a Speaker could build his coalition, the more he could rely on seniority in subsequent terms. Thus, even during the pre-1911 period, seniority had gained a foothold in the House of Representatives.

V. AFTER 1911: GROWING DECENTRALIZATION

After 1911, the power of appointment was taken from the Speaker and vested in party committees on committees, subject to the ratification (which was pro forma) of the whole House. For most of the first decade after this sweeping change, the appointment of chairmen was the business of the Democratic party, as the majority party in the House.

The revolt against Cannon did not immediately result in the decentralization of power in the House. Rather, power was shifted from the Speaker to the majority leader, Oscar Underwood, who chaired the House Committee on Ways and Means, the Democratic committee on committees, and led the Democratic caucus. George Rothwell Brown remarks:

> . . . The authority to name the committees of the House when taken from the Speaker was lodged theoretically in the most important committee of the House, but actually in the chairman of that committee. The Ways and Means Committee appointed the Committee on Rules, and the caucus adopted a resolution that no man should serve on more than one of the fourteen major committees.[33]

The Republican caucus had been destroyed by the revolt against Cannon, but not the Democratic caucus, with its two-thirds rule. In the 62nd Congress, rules changes were put through by the Democratic caucus vastly weakening the powers of the Speaker, with Speaker Clark's consent, but not decentralizing power in the House. The caucus was run by the Ways and Means committee, its chairman, and President Wilson. As American involvement in World War I increased, Wilson took more and more initiative and came to dominate Congressional activity.[34]

A recent study of the period observes:

> The emergence of the Democratic caucus as the real source of legislative power in the Congress was particularly galling to the insurgents. . . . Far from supplanting the caucus, Wilson relied upon it as his prime instrument of legislative power. Agreements on administration programs were hammered out in the Democratic congressional caucuses and then steam-rollered through Congress by solid Democratic majorities. In the standing committees Republican Congressmen and Senators, whether progressives or conservatives, were rarely allowed to play any significant part in the legislative process.[35]

The continued centralization of power in the first decade after the revolt against Cannon is reflected in our data as well. Table 18 shows a sizeable number of uncompensated violations of seniority in this first decade. When a potential chairman moved to a better committee, we interpreted the decision as also reflecting the influence of centralized power, since the shift had to be negotiated through the committee on committees. These two outcomes of seniority violation are contrasted in Table 18 with situations where members chose between chairmanships or themselves become party leaders, which we interpret as reflecting decentralization of discretion over the appointment process. The long-term trend toward decentralizaion is, of course, clear, and it emerges most strongly first in the decade of Republican dominance after the Wilson era, and continues on into the present day.

In 1919, decentralization took a major leap forward. In the election of 1918 the Republican party regained control of the House but was badly split between two leaders, James R. Mann of Illinois, a protege of Cannon (who was still serving in the House and active in party affairs), and the more progressive Frederick H. Gillett of Massachusetts. Gillett was elected Speaker on the first ballot, but did not fare so well in the Republican caucus on the matter of committee assignments, as the *New York Times* account indicates:

[33] Brown, *op. cit.*, pp. 175 ff.

[34] *Ibid.*, pp. 175–187. See also Wilder Haines "The Congressional Caucus of Today," this REVIEW 9 (November, 1915), pp. 696–706. The corresponding story in the Senate is told by Claude G. Bowers, *The Life of John Worth Kern* (Indianapolis: Hollenbeck, 1918), pp. 287–295.

[35] James Holt *Congressional Insurgents and the Party System 1909–1916* (Cambridge: Harvard University Press, 1967), pp. 84–85.

TABLE 18. HOW DECENTRALIZATION IN COMMITTEE ASSIGNMENTS GREW. VIOLATIONS OF SENIORITY, BY PERIOD

Congress: / *Year:*	47–61 1881–1911 Before Revolt Against Cannon	62–65 1911–1919 Democratic Caucus Dominates	66–72 1919–1932 Republican Era	73–79 1933–1946 F.D.R. Era	80–88 1947–1963 Since Reorganization Act
Centralized Decision-Making					
1. Uncompensated Violations	40% (129)	25% (16)	9% (9)	4% (2)	0
2. Potential Chairmen Awarded Better Assignments	18% (59)	33% (21)	22% (23)	14% (8)	20% (1)
	58%	58%	31%	18%	20%
Decentralized Decision-Making					
1. Member Chose Between Chairmanships	41% (131)	38% (24)	67% (69)	75% (46)	80% (4)
2. Member Joined Party Leadership	1% (2)	4% (2)	2% (2)	7% (4)	0
	42%	42%	69%	82%	80%
Total Violations	(321)	(63)	(103)	(60)	(5)

At 12:30 o'clock this morning it was announced that the Mann forces had routed the Gillett faction in the matter of the selection of the Committee on Committees. . . . Mr. Mann offered a [motion] providing that each of the Republican states having representation in the House . . . should elect a member of a Committee on Committees [and providing that] the members of this committee should each respectively have a voting power equal to the State's pro rata Republican membership in the House.

The duties of the proposed new form of Committee on Committees will be to elect the Republican members of the standing committees in the next House, to elect a Steering Committee of five, a Republican whip, and also a floor leader who should be ex officio Chairman of the new Committee on Committees, and likewise a Chairman of the Steering Committee.

The adoption of the Mann substitute was interpreted as indicating that the movement . . . [for] breaking down and overthrowing the domination of House committees under the rule of seniority of service has been blocked and defeated. The Mann substitute was probably adopted because the members who are holdovers and who have worked their way toward the top of the Republican side of the various committees want to retain the benefit of the rule of seniority. The adoption of the Mann substitute was interpreted as meaning that the House committees would be organized and their Chairmen picked in virtually the same old way, after the manner that put Southern men at the top of most of the important committees in both House and Senate in the [outgoing] Congress.[35]

Save for slight differences in the prose style, this passage, now nearly 50 years old, might have been clipped from the *New York Times* of today. In the decades that followed, the split in the Republican majority led to the formation of a legislative coalition with increasingly dissident southern Democrats. Party cohesion and majority rule through the caucus gave way to majority rule in the House based on a powerful cross party alliance which supported and maintained the seniority system.

The great profusion of minor committees up until 1946 gave party leaders a small amount of room for maneuver, however. Table 19 demonstrates that virtually all uncompensated violations of seniority in the period after 1910 involved the chairmen of minor committees. But even these, as Figure 4 shows, occurred predominantly during the first decade, when power was still centralized in the Democratic caucus.

The year 1946 marks the passage of the Legislative Reorganization Act, which reduced the number of committees from 48 to 19, principally by consolidating or abolishing minor committees and, in the process, liquidating large numbers of minor chairmanships. Since 1946, the seniority

[35] *New York Times* February 28, 1919.

TABLE 19. AFTER 1910 UNCOMPENSATED VIOLATIONS TAKE PLACE ON MINOR COMMITTEES

	1881–1910	1910–1963
Major	8% (10)	(0)
Semi-exclusive	22% (29)	4% (1)
Minor	70% (90)	96% (26)
	100% (129)	100% (27)

system has been virtually untouched, and chairmen have stayed put. The exceptions over two and a half decades are Omar Burleson, mentioned earlier, and Adam Clayton Powell, who was deprived of his chairmanship in 1967 in the course of proceedings which led to his exclusion from Congress altogether.[37] If 1911 signals the decline of uncompensated violations, 1946 marks the virtual demise of exceptions to the seniority system, as Table 20 illustrates.

But even before 1946, most exceptions to seniority reflected decentralized choices rather than the manipulations of party leaders. Here is a brief listing of the circumstances surrounding the eleven most recent violations of the seniority of a committee chairman in the House of Representatives, aside from the cases of Powell and Burleson.

(1) In the Republican-controlled 83rd Congress (1953–55), A. L. Miller of Nebraska became chairman of the committee on Interior and Insular Affairs. The ranking Republican member in the previous Congress, Fred Crawford of Michigan, failed to be renominated to Congress. The second ranking Republican, Dean P. Taylor of New York, whose seniority was violated, transferred to the Judiciary committee. Miller was third in seniority.

(2) In the 80th Congress, Richard Welch of California became chairman of the Public Lands Committee. He had been ranking member of three other committees in the previous Congress, two of which (Labor and Insular Affairs) ceased to exist in the 80th Congress when, under the reorganization of that year, they were combined with other committees. Welch's third committee in the 79th Congress was Merchant Marine and Fisheries, which he left to become chairman of Public Lands.

(3) In the 80th Congress, on the Committee on Merchant Marine and Fisheries, Fred Bradley of Michigan, second ranked Republican, became chairman when the ranking member, Richard Welch, took the chairmanship of Public Lands instead.

(4) In the 79th Congress the chairman of the Committee on Elections (Number 3) gave up his chairmanship and became ranking member. He took the chairmanship of the Committee on Territories instead.

There were seven other violations of seniority in the 79th Congress, involving the following committees: Election of the President and Vice President (previous chairman transferred to Agriculture); Irrigation and Reclamation (previous chairman, a congressman from silver-rich Idaho, transferred to the chairmanship of the Committee on Coinage); Memorials (previous chairman, retaining five other committee assignments, transferred to UnAmerican Activities); Enrolled Bills (a freshman member became chairman, violating the seniority of Mary T. Norton, who chose instead to become Chairman of Labor. She retained her position on Enrolled Bills as ranking Democrat rather than move up to the Chairmanship); Expenditures in the Executive Departments (ranking Democrat failed to move up); and Indian Affairs (previous

[37] Seniority has also been pretty much inviolate since 1946 for committee rank and file as well as chairmen. There are only two exceptions. It took an elaborate warning procedure followed by a fight in the full caucus of the House Democratic party in 1965 to impair the seniority of two southern House Democrats, neither of them chairmen, who had campaigned actively for Republicans in the election of 1964. One of these Democrats promptly resigned his seat and was reelected to Congress as a Republican. The other quit the House soon thereafter and became Governor of Mississippi. Under a rule of the Democratic caucus, the so-called Byrnes Rule, the Democratic committee on committees' assignments are subject to review by the caucus. In practice, however, except for these two cases, there has been no review. The overwhelming liberal Democratic majority of 1965, mobilized by the Democratic Study Group, was able to threaten a review and thereby opened the question of the seniority of these two members.

TABLE 20. THE DECLINE OF UNCOMPENSATED VIOLATIONS

	1881–1910	1910–1946	1946–1963
Compensated	59% (192)	88% (204)	(1)
Uncompensated	41% (129)	12% (27)	(0)
	321	231	1

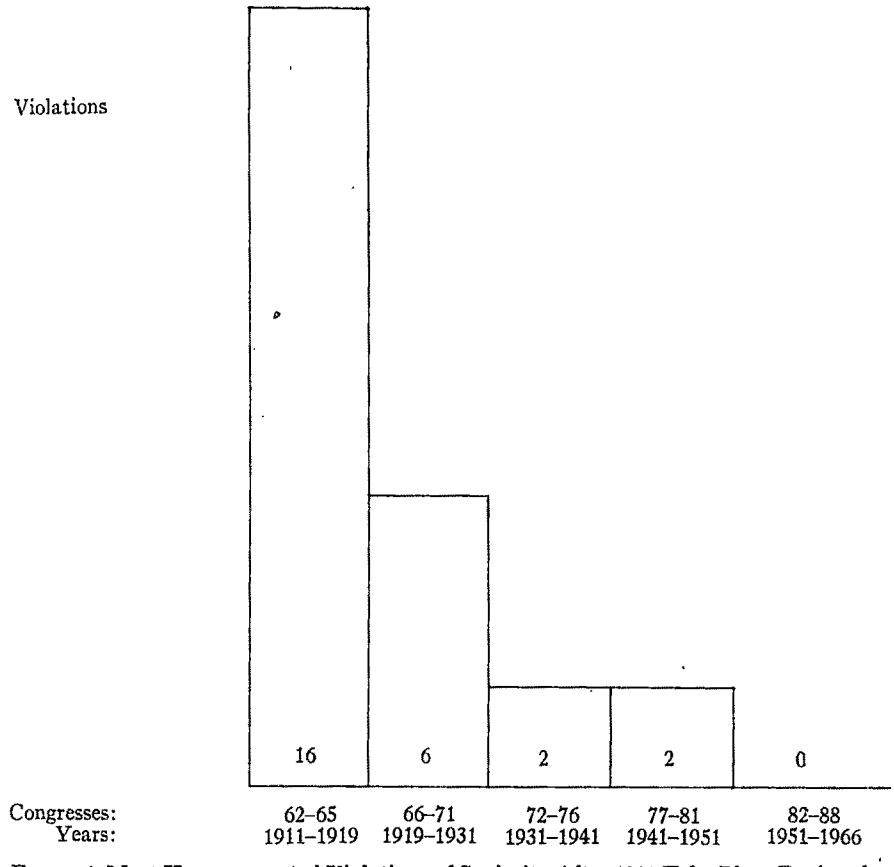


FIGURE 4. Most Uncompensated Violations of Seniority After 1910 Take Place During the Period of Democratic Caucus Centralization

chairman and ranking Democrat failed to return to Congress; third ranking man, Samuel Dickstein of New York, moved up to second place but did not take the chairmanship. Dickstein was at the time chairman of the Committee on Immigration and Naturalization; no doubt there were more immigrants than Indians in his lower east side Manhattan constituency).

The seniority violations after 1919 appear to reflect not the Speaker's discretion, or the discretion of a leadership committee, but rather the application of a norm that no member should chair more than one committee. The exercise by members of their preferences in this connection was capable of setting off chain reactions reshuffling large numbers of committee chairmen; this accounts for the fact that even after 1919 there were a sizeable number of seniority violations. However, the main mechanisms at work before 1911, from 1911 to 1919, and thereafter seem quite different.

Party leaders did retain some room for maneuver, even in the post 1911 situation, owing to the large number of minor committees. When vacancies on important committees opened up, party leaders could offer senior men on minor committees the vacancy and thereby manipulate minor committee chairmanships. This option was effectively closed by the Legislative Reorganization Act of 1946, which reduced the number of committees and also vastly cut down on the number and diversity of manipulable vacancies available at any one time.[38] And, of course, as

[38] It also increased the difficulty for any member of attaining a chairmanship. Richard Brooks shows a tremendous contrast in the average number of years a member had to serve before becoming a committee chairman before and after 1946. From 1889–1910 the average wait was 6.1 years. From 1911–1946 the average wait was 7.5 years. From 1947–1954 chairmen served in the ranks an average of 16.6 years. Richard Stephens Brooks "The Sectional and Seniority Bases of the Standing Committee Chairmanships in the U.S. House of Representatives, 1889–1954" (unpublished Ph.D. dissertation, University of Oklahoma, 1956), p. 89.

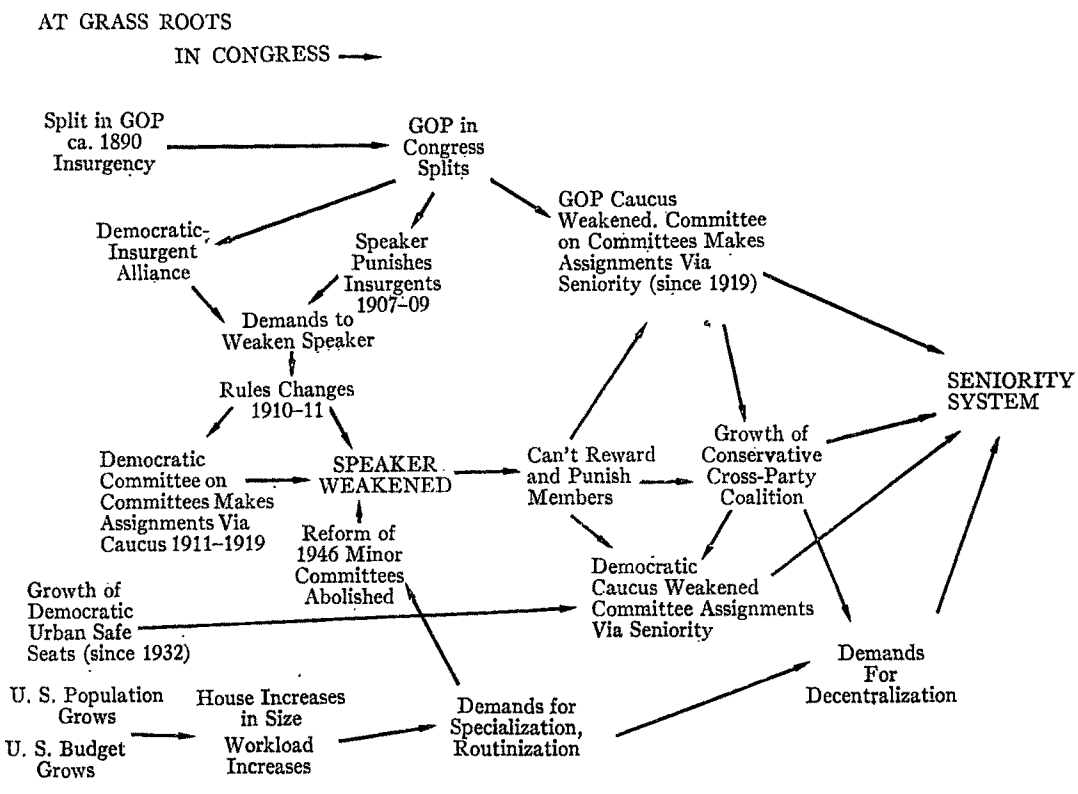


FIGURE 5. Historical Causes of seniority sytem

the number of committee places diminished, the committee assignments of individual members were limited in most cases to only one committee.[39]

VI. CONCLUSIONS

What caused seniority? Historically, the answer seems to have been a split in the grass roots of the Republican party that ultimately produced a strong but unstable Democratic-Insurgent Republican majority coalition in the House—strong enough permanently to weaken the prerogatives of the Speaker, unstable enough to preclude the election of a strong Speaker of their own.[40] One of the consequences of this split was the reinforcement of weak preexisting norms favoring continuity of chairmanships and limiting in practice the freedom of Speakers to appoint chairmen when there was no party turnover. After the Democratic-Insurgent coalition weakened the power of the Speaker and of the Republican caucus, only the Democratic caucus was left as a centralizing force in the House. The Democrats lost control of the House in the 1920's, and in time a conservative cross-party coalition undermined the Democratic caucus.[41] Meanwhile, longer term pressures for routinization and specialization were having marginal effects. Ths is the complexly interrelated set of historical "causes" of seniority as it exists today that we try to diagram in Figure 5.

Seniority is also an ongoing social process that helps to maintain itself by creating conditions favorable to its own perpetuation. Our speculation as to the important causal relations in this process, and their connections with the processes that established the system, are diagrammed in Figure 6.

To summarize, we believe that our data show the effects of four main influences upon the growth of the seniority system in the U.S. House of Representatives. In the period between 1881 and 1911, a large number of committees

[39] See Lewis C. Gawthrop, "Changing Membership Patterns in House Committees" this REVIEW, 60 (June, 1966), 366–373.

[40] Holt *op. cit. passim* makes this argument very persuasively.

[41] On the present-day weakness of the Democratic caucus, see Clem Miller *Member of the House*, John W. Baker, ed. (New York: Scribner, 1962), pp. 88–89, 92.

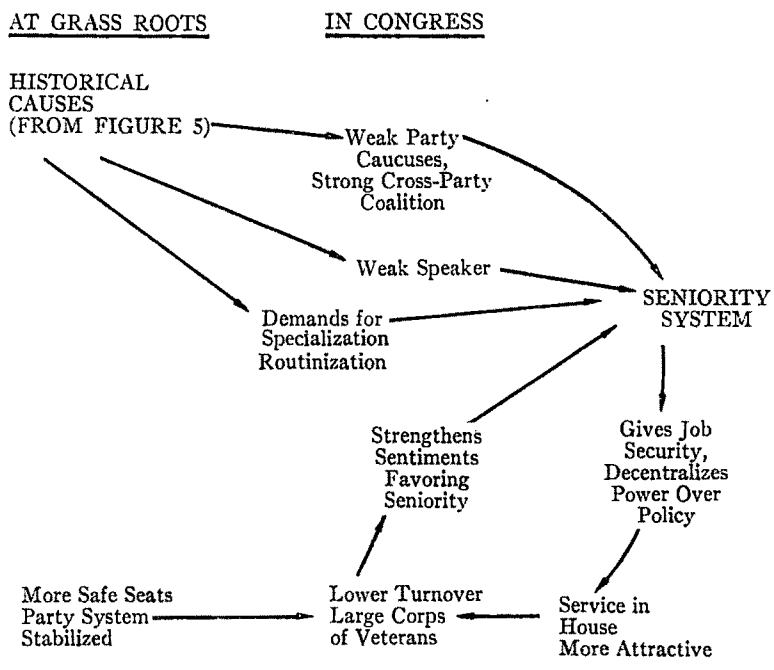


FIGURE 6. Now seniority maintains itself

and a discretionary system of appointment in which seniority and political considerations played a part produced a pattern of relatively frequent violations of seniority. Many of these were responsive to the necessity for new Speakers, after a party turnover in the House, to organize a majority coalition in the committees. After 1911 the system responded at first to the centralized decisions enforced by the Democratic caucus, and later to piecemeal bargaining and tinkering, as Representatives with many committee assignments adjusted their workloads with respect to a large number of narrowly focused committees. This produced some violations of seniority, but within a context providing much less power and discretion for party leaders in the House. After 1946, both the discretionary rights of leaders and the rich array of minor committees disappeared with the "streamlining" of the committee system, and thus from 1946 onward, the rule of seniority is virtually never breached. From a situation where seniority is one of a number of criteria determining committee chairmanships, then, we move through two intermediate periods, where centralized coordination of appointments is first undermined in the Republican party (1910) and later (after 1919) in the Democratic. Finally, the House moves after 1947 to a situation where there exists a full-blown seniority *system*, in which seniority is the single, automatic criterion determining the chairmanships of all committees, and the application of this criterion is not subject to the discretion of any body short of the relatively inactive full party caucuses.

These results are compatible with the argument that power in the House has decentralized rather than concentrated over time. They also reflect a general trend, discussed elsewhere, toward impersonal, automatic, and universalistic methods of conducting internal business, a characteristic pattern in the development of social organizations as they move from tentative, informal, and less complex stages toward institutional maturity.[42] It is an interesting anomaly that these two phenomena, progressive decentralization of power and institutionalization, are not normally associated, and that "ideal types" describing the natural history of organizational development typically presume a movement toward, rather than away from, concentration of power. The House of Representatives may be the exception that proves the rule, or it may serve as an occasion for the reconsideration of at least one small branch of organization theory.

[42] See Nelson W. Polsby, "The Institutionalization of the U.S. House of Representatives," this REVIEW, 62 (March, 1968) pp. 144–168. The present discussion supersedes, and in some respects corrects, the preliminary findings on seniority offered there on pp. 160–161.

SOCIAL STRUCTURE AND POLITICAL PARTICIPATION: DEVELOPMENTAL RELATIONSHIPS, II*

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Mass political participation increases as nations become more economically developed. In Part I of this study we attempted to identify the significant social experiences which explain the high levels of participation in economically developed nations. We formulated and explored the following general theory:

Economic development alters the social structure of a nation. As nations become more economically developed, three major changes occur: (1) the relative size of the upper and middle classes becomes greater; (2) larger numbers of citizens are concentrated in the urban areas; and (3) the density and complexity of economic and secondary organizations increases. These social changes imply political changes. Greater proportions of the population find themselves in life situations which lead to increased political information, political awareness, sense of personal political efficacy, and other relevant attitudes. These attitude changes, in turn, lead to increases in political participation.

The theory can be presented schematically in the following form:

A	→	B	→	C	→	D
Economic Development		Alterations in the Stratification, Urban, and Group Membership Patterns		Changes in the Distribution of Attitudes and Cognitions		Increases in Political Participation

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We were able to demonstrate that individuals' levels of social status and organizational involvement were strongly and consistently related to their levels of political participation.[1] The survey data from five nations showed, further, that organizational involvement was more strongly related to participation, and more consistently from nation to nation, than was social status.[2] Residence in an urban area, contrary to

[1] See our "Social Structure and Political Participation, I," Appendix B, this REVIEW, 63 (June, 1969), for the multiple item scales used to test these relationships. Social status includes individual's education, income, occupation, and an interviewer estimate. Organizational involvement includes primarily number of organizational memberships and having held an office in an organization. Participation includes talking politics, contacting local officials, contacting national officials, and joining formal political parties or organizations—but not voting.

[2] The survey data are taken from the Almond-

our initial theory, was shown *not* to be related to individual political participation, although we did find an interesting sub-pattern.[3] More advanced nations had, of course, more citizens residing in urban areas, but these individuals did not participate in politics more frequently than their rural counterparts in the same nation.

After establishing that social status and organizational involvement consistently predicted rates of political participation, we merged the survey data with aggregate data on the level of economic development of the five nations. We were able to devise several different ways to examine how much of the inter-nation correlation between economic development and participation could be explained by reference to changes in the class structure and organizational life of a nation. Although it was difficult to provide a clear test with only five national cases, our series of tests pointed uniformly in a single direction. There were no development-related differences in mass participation which were not explained by differences in the status and organizational structure variables. That is, insofar as mass participation is related to economic development, such participation is best understood by analyzing how economic development affects the distribution of social status positions and the development of organized group life. Finally, although we observed some idiosyncratic differences in political participation from one nation to another, these differences were minor when compared with the general pattern: citizens of similar social class and organizational involvement participate in politics at about the same absolute level irrespective of the nation in which they reside.

Having explored, as far as our limited number of national cases would allow, the relationships between economic development, social structure, and participation, we turned our attention to the role of political attitudes and cognitions. Our theory suggested that changing attitude patterns constituted the link between social structure and political participation. Aggregate rankings tended to support this possibility. The mean levels of political information, political efficacy, political attentiveness, and citizen duty to participate were consistently higher in the more developed nations, although there were a number of variations within the pattern.

But average or aggregate measures of individual characteristics cannot be used to examine systematically the pathway from development to participation. More sophisticated methods of analysis are needed to explore the theory that social changes are related to participation because they change political socialization patterns. The primary objective of this part of the study is to utilize a technique of causal modeling to investigate the attitudinal and cognitive pathways from individuals' positions in the class structure and their involvement in the society's organizational life to their levels of political participation. Although we cannot, of course, prove causality in the direction of the relationship, we can raise the following questions concerning these pathways:

(1) Which, if any, of the political attitudes and cognitions introduced represent potential causal links between social status and organizational involvement on the one hand and political participation on the other? How much of the relationship between B and D in our schematic theory can be explained by such attitude linkages?
(2) Are there different attitudinal paths associated with social status and with organizational involvement? That is, we shall attempt to determine if the citizen who participates in politics because of his social status differs in his political attitudes from the citizen who participates because of his organizational involvement.
(3) Do the interrelationships among status, organizational involvement, political attitudes, and political participation remain consistent from one nation to the next; or are there significant differences?

Addressing ourselves to these empirical questions leads to several other very interesting problems in the analysis of political participation. Whereas in Part I of this study we were able to say some rather definite things about how economic development affects the *level* of mass political participation, in Part II we shall be able to make statements about the relative

Verba Five-Nation Study. The five nations represented are Germany, the United Kingdom, the United States, Mexico and Italy. See Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), for information on samples and survey design. See Part I of this study, this REVIEW, 63 (June, 1969), for an extended discussion of methodological issues related to how we processed these data.

[3] The size of community in which a citizen lives is negatively related to his local political participation level. However, this negative relationship is not sufficiently strong (it explains less than two percent of the variance in local participation) to account for the absence of any relationship between place of residence and general political participation. We concluded that although urbanization is a major part of economic development, it is not, even in part, responsible for great increases in mass political participation.

contribution of different groupings in the society to the participant stratum. This we can do in two ways. We can consider the degree of under- or overrepresentation of different class groups among citizens actively involved in politics. We can also consider the degree to which politically active citizens are likely, under various conditions, to manifest particular types of political attitudes and cognitions.

Assumptions of our causal model.—The use of statistical causal modeling techniques is gaining acceptance in political science.[4] The analysis undertaken here differs somewhat, however, from usual formulations. We are not using causal inference to eliminate logical alternative models. Rather, we are using it to examine the relative importance of various proposed linkages in a given model. The causal order to independent and dependent variable is fixed. Further, we are not interested in the causal priority of the hypothesized intervening variables. Our research question is whether the five attitude variables explain the persistent relationship between a citizen's socioeconomic traits and his rate of political participation, and, in addition, which of the five provide the strongest links in the chain. Because each link relates to a specific hypothesis and thus has considerable theoretical interest to us, we do not wish to eliminate any of them completely, at least at this time, but instead attempt to assess their relative importance.

For this purpose, as suggested by Otis Dudley Duncan,[5] the simplest procedure is not to construct prediction equations specifying certain relations—usually zero ones—but to compute path coefficients for the given model.[6] The direct path linkages between independent and dependent variables, for example, will be zero if the partial correlations would have been zero. Where not zero, we are able to derive measures of proportional importance in the total relationship. This is the procedure followed.

We must, of course, take note of some of the assumptions which are made in employing these techniques.[7] First, relationships between all vari-

[4] For example, see Hubert Blalock, "Causal Inferences, Closed Populations, and Measures of Association," this REVIEW, 61 (March, 1967), 130–136; Hubert M. Blalock, *Causal Inferences in Non-Experimental Research* (Chapel Hill: University of North Carolina Press, 1964); Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," this REVIEW, 57 (March, 1963), 45–56; Arthur S. Goldberg, "Discerning a Causal Pattern among Data on Voting Behavior," this REVIEW, 60 (December, 1966), 913–933; and Donald J. McCrone and Charles F. Cnudde, "Towards a Communications Theory of Democratic Development," this REVIEW, 61 (March, 1967), 72–79. We owe a particular debt to the excellent discussion by Hayward R. Alker, Jr., "Causal Inferences and Political Analysis," in Joseph Bernd (ed.), *Mathematical Applications in Political Science II* (Dallas: Southern Methodist University Press, 1966), pp. 7–43.

[5] Otis Dudley Duncan, "Path Analysis: Sociological Examples," *American Journal of Sociology*, 72 (July, 1966), 6–7.

[6] These path coefficients are the standardized B (slope) coefficients for a series of regression equations. The causal model is described by these simultaneous equations. The causal properties are linked to the fact that each succeeding equation adds a new variable to the model—and is described as a dependent variable "caused" by all preceding variables in the model, but by none of the ones subsequently added. The final dependent variable, participation in this case, is the product of all other variables. All equations after the first two ($X_2 = B_{21}\ X_1 + B_{2t}\ R_t$, and $X_1 = B_{1s}\ R_s$) are shown in the left hand column in Table 2. As Blalock, *op. cit.*, has suggested, unstandardized b weights provide rich interpretations that these standardized path coefficients do not. However, the scores obtained on the variables in our model are based in many cases on originally arbitrary variances. The different questions on competence, for example, had different numbers of alternative responses and even the meaningfulness of the zero points is not clear on an attitude variable of this type. In this situation the interpretation of the unstandardized scores is clearly very difficult. See the comments by John W. Tukey, in "Causations, Regression, and Path Analysis," in Oscar Kempthorne, *et al.* (eds.), *Statistics and Mathematics in Biology* (Ames, Iowa: Iowa State College Press, 1954). However, we do hope to examine unstandardized coefficients in models involving non-attitudinal variables only, particularly in regard to the lines of inquiry raised in the section on "Social Structure and the Participation of Class Groups," below, in future analysis.

[7] For a more complete discussion of these limitations see Alker, *op. cit.*; Duncan, *op. cit.*; Raymond Boudon, "A Method of Linear Causal Analysis: Dependence Analysis," *American Sociological Review*, 30 (June, 1965), 365–374; and Hugh Donald Forbes and Edward R. Tufte, "A Note of Caution in Causal Modelling," this REVIEW, 62 (December, 1968), 1258–1264. Although the useful Forbes and Tufte discussion was not available to us while this analysis was being conducted, we believe that our approach to causal modelling avoids many of the problems raised by them. We must continue to emphasize, of course, that the causal *order* of our variables is posited from deductive theory, not derived from the data.

ables are assumed to be additive and linear. Second, other causes of each of the eight variables are assumed to be uncorrelated with each other or other variables in the system.[8] Third, all causal relationships are assumed to be unidirectional, rather than reciprocal. This third assumption seems tenable in terms of the position of the independent variables (status and organizational involvement). It is obviously less satisfying and plausible in terms of relationships among the attitude variables and between attitudinal and participatory variables. At the moment this problem cannot be avoided.[9] We believe that it has little effect on the relationships between independent and dependent variables as they emerge in the causal model, but we present findings about the relative position of attitude variables in a more tentative vein. At a future date we hope to explore interactive models. Fourth, the assumptions of the specific causal order have been made for purposes of this analysis, and face the same problems as the assumption of unidirectionality.

In addition to these statistical assumptions, we should note that we are undertaking, in effect, analysis of a longitudinal causal pattern through cross-sectional analysis. Obviously, there is no substitute for over-time observation in finally ascertaining the development of patterns of political participation for various individuals of different social positions. This is a standard problem in analysis of political behavior, given the time and cost requirements of longitudinal observation, but it should be noted nonetheless.

I. THE FINDINGS: DIFFERENCES IN THE CAUSAL LINKAGE PATTERNS OF SOCIAL STATUS AND ORGANIZATIONAL INVOLVEMENT

We begin with the very general hypothesis that the causal relationship between social status and organizational involvement, on the one hand, and political participation, on the other, can be traced through intervening political attitudes. Figure 1 and Table 1 present data relevant to this hypothesis.

Initial examination reveals one pattern of overwhelming strength and consistency: social status and organizational involvement operate through quite different causal paths in their impact on political participation. This finding holds true in all five countries and remains the case in spite of various manipulations of the data, alteration of the order of the intervening variables, changing the number of the intervening variables, and the like.

The basic finding is both simple and important. Virtually all the relationship between social status and political participation is explained by the intervening linking attitude variables. The high social status citizen does not *just* participate in politics; he does so only when he has the attitudes such as efficacy and attentiveness which are postulated as intervening variables. Social status, then, affects rates of political participation through its effect on political attitudes. This does not mean, it must be emphasized, that the correlation between status and participation is spurious; on the contrary, it means that intervening attitude variables explain *how* a citizen's social status affects his political activity.

On the other hand, a very large part of the relationship between organizational involvement and participation *is unexplained by any variable in this model.* In every case, about 60 percent of the correlation between organizational involvement and political participation is accounted for by the direct link, that line that does not pass through social class or the attitudinal variables. (See link 2 → 8 in Figure 1).

The strength and cross-national consistency of this pattern is shown as well in Table 1. In a causal model of this type, the raw correlation (Pearson Product-Moment) between independent and dependent variable equals the sum of all pathways down the causal chain from independent to dependent variable.[10] The score of a "pathway" in this sense is the product of all path coefficients along the chain. Table 1 presents the percent of the raw correlation between social status and organizational involvement, on the one hand, and political participation, on the other, accounted for by: (1) the direct relationship only, unaccounted for by other variables in the model; (2) the path through the other inde-

[8] Urbanization has been deleted as a variable in the model, since we know it to be significantly related to Social Status only, of all the variables in the model, and its deletion greatly simplifies calculation of paths. Deletion of urbanization is a violation of this assumption, in a minor fashion, when considering local participation only, rather than combined scales. It is therefore reintroduced below when considering the causal models of local and national participation separately.

[9] Nonrecursive path analysis is also possible. At a future point we hope to explore further interactive models for this and similar data. We have not yet moved to this stage of analysis. See Duncan, *op. cit.*

[10] In the eight variable causal model there are 54 paths from status to participation. No paths moving back along the model at any point are allowed by the constraints imposed.

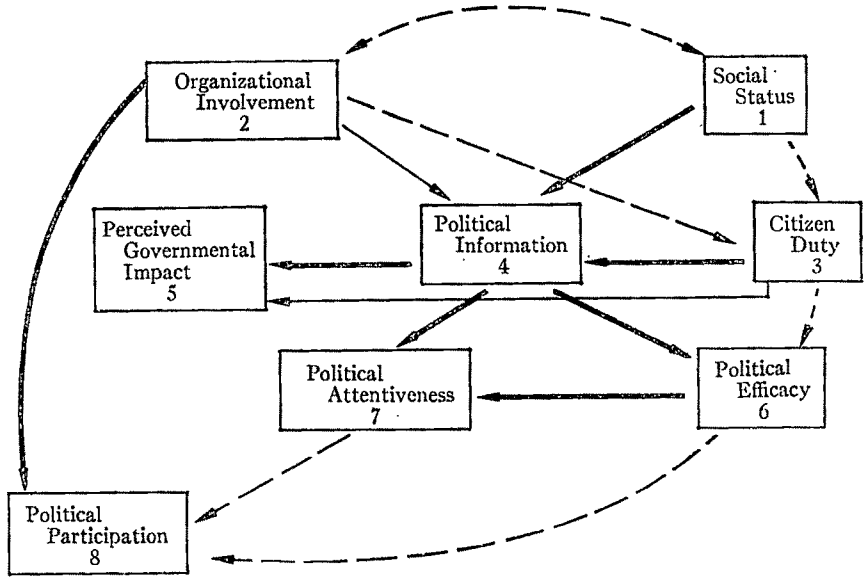


FIG. 1. Eight variable causal model: paths from status and organizational involvement to political participation

Key to Figure 1:

(1) No arrow between variables indicates that the path coefficients are less than .150 in at least three of the five nations. There are 13 paths *not* shown in the figure.
(2) Heavy arrows between variables indicate path coefficients greater than .250 (about 5 times the average standard error) in at least three of five national samples. Light arrows between variables indicate path coefficients between .150 and .250 in three or more of the five national samples.
(3) Unbroken arrows (both light and heavy) indicate *difference* between largest and smallest of the five coefficients is *less* than .150 (about 3 standard errors). Broken arrows between variables indicate a difference greater than .150.
(4) See Table 2 below for full path coefficients for each nation. Variables are explained in text, footnotes, and appendix. All linkages shown are positive.

TABLE 1. PATH COMPONENTS OF RELATIONSHIP BETWEEN TWO INDEPENDENT VARIABLES AND POLITICAL PARTICIPATION AS PERCENTAGES OF TOTAL CORRELATIONS[a]

	Independent Variables									
	Social Status					Organizational Involvement				
Proportion of Correlation Attributable to:	US	UK	GER	IT	MX	US	UK	GER	IT	MX
(1) Direct link to participation	20%	−8%	−33%	−15%	−1%	57%	62%	61%	51%	68%
(2) Paths thru other independent (1 or 2 variable only)	30	30	35	27	34	7	−2	−3	−2	0
(3) Paths thru other ind var *and* attitude variables	10	12	20	21	13	14	12	6	12	6
(4) Paths thru attitude variables only	40	61	78	67	54	20	25	35	40	26
Total percentage	100	95	100	100	100	98	97	99	101	100
Basic correlation between ind var and participation	.431	.303	.181	.283	.238	.523	.480	.486	.490	.515

[a] Path coefficients from Table 2. The correlations are equal to the sum of all pathways from independent to dependent variable. The pathways are computed by multiplying all path coefficients along the given path. Negative scores arise from negative path linkages. For those percentages, each independent variable is treated as if it were the first, undetermined, variable in the chain. Key: US = United States; UK = United Kingdom; GER = West Germany; IT = Italy; MX = Mexico.

pendent variable only; (3) the paths going first through the other independent variable and then through the attitude variables; and (4) the paths going directly from each independent variable through the attitude variables to political participation. For example, the correlation between organizational involvement and political participation in the United States is .523. Fifty-eight percent of *this* is accounted for by the direct path between these two variables (that is, the path that passes through no other variable in the model). Seven percent of the correlation between these two variables is accounted for by the path from organizational involvement to social status and then directly to participation; 14% of the correlation is explained by the path from organizational involvement through social status and then through the intervening attitude variables; and the remaining 21% of the correlation is accounted for by the path from organizational involvement through the various attitude variables but by-passing the social status variable.

The structure of the components of the organizational involvement relationship are almost uncannily consistent from nation to nation. The social status to participation relationships are less consistent, with the U.S. having a fairly large proportion accounted for by the direct link (although the path coefficient is less than twice the standard error and would not usually be considered statistically significant). In Germany the direct link between social status and participation has a high negative score, although the percentage is exaggerated by the weakness of the total correlation (and the score is also less than twice the standard error).[11] The basic pattern, nonetheless, is clear, and we suspect inconsistency reflects in part, at least, some of the problems in adequate measures of social status, or its sub-dimensions.

The general pattern for social status finds about 60 percent of the relationship going through the attitude variables, about 30 percent going directly through organizational involvement, and about 10 percent going through the attitude variables by way of involvement. The general pattern for organizational involvement shows about 60 percent accounted for by the direct link, about 30 percent going through the attitude variables only, and about 10 percent going through social status and then the attitude variables.

Thus, a large part of the relationship between organizational involvement and political participation cannot be explained by reference to other variables included in the model. It is clear that those who are organizationally involved participate in politics at rates far greater than citizens who are not involved. In addition, and of considerable importance, many citizens whose organizational involvement propels them into political life are *not* more politically informed, politically efficacious, or politically attentive than the non-participants. Unlike political participation stemming from high social status, participation stemming from organizational membership does not necessarily imply those attitudes normally thought to be associated with democratic political participation. We return later to the implications of these findings.

There are two alternative, though not mutually exclusive, explanations for why the organizationally involved are politically active even though they lack certain attitudes. First, organizations can mobilize their members toward political goals; and this can occur without an intervening state of changes in attitudes by the members. The organizations might provide inducements and gratifications to their members, such as sociability, which though producing political activity have nothing to do with political information, awareness, and so forth. Further, the organizations can mobilize members for relatively specific, short-term issues, a mobilization which again does not depend upon any facilitating attitudes other than a willingness to follow the lead of the organization leadership. A professional or business group circulating a petition to be signed or a union enjoining their members to vote are examples of such mobilization.

Second, both organizational and political involvement might be strongly affected by an underlying personality attribute or cultural norm. For instance, both types of activity might be related to a general propensity toward social activism which is unrelated to social status. This

[11] At an earlier stage in the analysis, scales were built with consistency checks on the SES dimension. Persons high on one status item and very low on another were deleted. This procedure increased correlations, but decreased the size of the valid sample, and was not, therefore, used in this analysis. It is especially noteworthy that while only about 10% of the original samples in the US, UK, Italy, and Mexico failed to meet the SES dimension consistency check, about *50%* of the German sample failed to meet it. This suggests the disruption of the social stratification system in Germany as a consequence, no doubt, of the historical upheavals. The strong wage position of workers in the post-war boom, following the leveling impact of World War II and the Third Reich, may also be a contributing factor. See the comments by Lewis J. Edinger, *Politics in Germany* (Boston: Little, Brown, 1968), pp. 19-30.

explanation implies that joining an organization, becoming active in it, and getting involved in politics are subsets of a more general social behavior which, for reasons of personality attributes, is more attractive to some citizens than others. It is an explanation stressing self-selection.

This is not the place to explore the merits of these alternative explanations—and the data available permit no direct test—but we do suggest that the former explanation has more merit than the latter. The self-selection hypothesis is weakened since we would expect a self-selective tendency to correlate with attitudes such as political efficacy and attentiveness. The absence of strong relationships between organizational involvement and these attitudes makes us suspicious of the self-selection explanation. The first explanation, that organizations are mobilizing their members, is intuitively more persuasive and has been supported by additional research. Data collected in the United States show that membership in organizations which have some relationship to public affairs and which have formal or informal discussions of politics at meetings, greatly increases the likelihood of being politically active. In contrast, members who belong to more apolitical organizations are only slightly more likely than non-members to participate in politics.[12]

II. FIVE INTERVENING VARIABLES AND PROVISIONAL HYPOTHESES ABOUT POLITICAL PARTICIPATION

An eight variable model is rather complex for adequate pictoral representation. Nonetheless, we felt that each of the intervening variables contributed to the theoretical richness of the model. Figure 1 presented a composite path diagram. The strongest path links in absolute terms are heavy arrows. "Strongest" means, here, more than five times the average standard error for all relationships (which is about .050). Somewhat weaker relationships, but still stronger than three times the average standard error in a majority of the nations, are shown as lighter arrows. Where there is a range of more than three standard errors (average) between highest and lowest coefficients, the arrow is shown with a broken line to indicate a less consistent relationship. (See the Key to Figure 1 for a more precise description.)

Table 2 shows the exact coefficients on which Figure 1 is based; Appendix I presents the corresponding standard errors. For a random sample, we would say that path coefficients which are more than double the standard error are statistically significant. However, given the problems in accurately estimating the real nature of the sample used in the five nations, we prefer simply to emphasize that the relationships are quite consistent, that some are relatively strong, and that others, such as the direct link between social status and participation, are always weak.[13]

Of course we are less confident about the ordering of the causal model as it pertains to the different attitudinal variables than we are about the independent → intervening → dependent ordering. There is, indeed, a high probability that a number of these attitude variables have an interactive relationship of the type disallowed by the conditions of the model. Thus any observations we make about the relative importance of these relationships must be suggestive rather than definitive.

Variables 1 and 2 in the model are, of course, social status and organizational involvement. These are the independent variables, as we have discussed. They are mildly related (except in the U.S. where the relationship is stronger). We assume that status is probably prior in the causal ordering: an individual's education, occupation, income, and life style tend to be established by his family background, and are less often a product than a cause of organizational involvement. But for analysis purposes, we treat these as the two independent "first cause" variables.[14] For that reason they are linked by a double

[12] This research, carried out by Sidney Verba and Norman Nie, shows that a great majority of all types of voluntary organizations in the U.S. have an interest in public affairs and members engage in political discussions; this accounts for the strong relationship between organizational involvement and participation even when not controlling for the type of organization. These findings are based on preliminary analysis of the U.S. data from the "Cross National Research Program in Social and Political Change."

[13] As is described in Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), pp. 509–525, the national samples were cluster samples and collected in somewhat different ways with different degrees of success. In Mexico, as noted, no interviews were collected in towns under 10,000. Given these differences and the fact that much of the sampling data was not available in our analysis tapes, we have not attempted to compute a sampling error—it could only be a guess—and prefer to rely on the strength and consistency of relationships, rather than significance tests presuming probability samples. See the comments on significance of correlation coefficients in Part I, Table 1, this REVIEW, 63 (June, 1969).

[14] See Duncan, *op. cit.*

TABLE 2. PATH COEFFICIENTS*

Eight Variable Recursive Model

Dependent Variable Equation	Country	B_1	B_2	B_3	B_4	B_5	B_6	B_7
$\chi_3 = B_{32}\chi_2 + B_{31}\chi_1 + B_{3u}R_u$	US	.345	.152					
	UK	.223	.195					
	GR	.169	.252					
	IT	.274	.314					
	MX	.190	.178					
$\chi_4 = B_{43}\chi_3 + B_{42}\chi_2 + B_{41}\chi_1 + B_{4v}R_v$	US	.302	.133	.344				
	UK	.337	.151	.269				
	GR	.258	.154	.333				
	IT	.347	.220	.248				
	MX	.287	.275	.219				
$\chi_5 = B_{54}\chi_4 + B_{53}\chi_3 + B_{52}\chi_2 + B_{51}\chi_1 + B_{5w}R_w$	US	.132	.000	.182	.228			
	UK	.123	.049	.160	.225			
	GR	.057	.000	.122	.298			
	IT	.000	.080	.144	.350			
	MX	.041	.000	.067	.088			
$\chi_6 = B_{65}\chi_5 + B_{64}\chi_4 + B_{63}\chi_3 + B_{62y2} + B_{61}\chi_1 + B_{6x}R_x$	US	.127	.125	.273	.242	.144		
	UK	.000	.174	.241	.184	.174		
	GR	.113	.186	.235	.307	.083		
	IT	.099	.138	.235	.264	.164		
	MX	.134	.108	.110	.319	.086		
$\chi_7 = B_{76}\chi_6 + B_{75}\chi_5 + B_{74}\chi_4 + B_{73}\chi_3 + B_{72}\chi_2 + B_{71}\chi_1 + B_{7y}R_y$	US	.055	.106	.030	.228	.135	.328	
	UK	.115	.056	.130	.284	.030	.221	
	GR	.053	.101	.120	.287	.072	.285	
	IT	.095	.126	.078	.319	.043	.272	
	MX	.119	.176	.136	.251	.068	.195	
$\chi_8 = B_{87}\chi_7 + B_{86}\chi_6 + B_{85}\chi_5 + B_{84}\chi_4 + B_{83}\chi_3 + B_{82}\chi_2 + B_{81}\chi_1 + B_{8z}R_z$	US	.086	.296	−.008	.048	.060	.168	.236
	UK	−.024	.295	.054	.110	.122	.063	.316
	GR	−.061	.295	.037	.017	.071	.210	.290
	IT	−.041	.250	−.021	−.022	−.079	.234	.434
	MX	−.003	.351	.029	.125	.100	.071	.206

χ_1 = Social Status
χ_2 = Organization Involvement
χ_3 = Citizen Duty
χ_4 = Political Information
χ_5 = Perceived Impact of Government
χ_6 = Political Efficacy
χ_7 = Political Attention
χ_8 = Political Participation

* See footnote 6 and relevant literature for the meaning of the regression equations. See Appendix to Part I, this REVIEW 63 (June, 1969), for creation of the variables from which the regression equations' standardized B's were computed.

arrow in Figure 1. We have noted the differences in the causal linkages from these two variables to participation. Each of the subsequent variables is drawn from the literature on attitudes and participation, and these variables can be linked to participation in a series of complementary hypotheses. Our data permit us to comment on these, and their relative importance in the causal chain specified by this model, although we stress that these must be considered provisional observations, not tests of the hypotheses.

Obligation to participate.—Variable 3 measures the sense of citizen duty to participate.

The scale was constructed from replies indicating the degree of active participation mentioned by respondents when enumerating the duties of a good citizen. Commentators on democratic participation have hypothesized that higher social status and organizational involvement expose the citizen to value systems approving of the act of participation.[15] Political participation as a value is taught in the schools, reiterated in mass media, and reinforced as a norm in general social interaction. The variable is interesting because prevalence of such participatory norms would not seem an intrinsic part of the developmental process, but rather a special outcome of conditions and political education efforts in particular societies.

The data in Table 2 suggest that citizen duty does, indeed, play a part in the propensity of citizens in these five nations to become involved in politics as they become more organizationally involved and better educated. However, there is very considerable variance from nation to nation, both in the strength of the relationship and in the linkages to the independent variables. In some nations this variable is more strongly linked to group involvement than status. This variation is, indeed, what we might expect from a political orientation related to particular patterns of socialization, current and historical. In each of our five nations from 10 to 22 per cent of the correlation between status and participation is explained by paths through civic duty. Civic duty operates almost purely as an intervening variable: its effect on participation is felt through subsequent links to information and efficacy. That is, motivation through sense of an obligation to participate increases one's tendency to get information about politics and feel confident in political expectations, but does not directly propel one into the political arena.

Information as a political resource.—Variable 4 measures citizen knowledge of national political leaders and high governmental affairs, basic indicators of political information. The developmental hypothesis suggests that higher status and organizational involvement expose citizens to communication stimuli of all kinds, informing them in general and, hence, about political events in particular.[16] Information in turn, it is presumed, increases citizens' sense of competence, promotes awareness of a stake in public policies, and provides a basic interpretative resource for following and understanding public affairs. This is a type of factor which would seem intimately linked to the developmental process, particularly to increases in general levels of education and growth of mass media, as an intervening attitudinal resource for mass participation.

The data tend to support this hypothesis quite firmly. Information is strongly related as a direct product of social status. Information does not directly increase participation, but does act strongly and consistently through sense of governmental impact, political efficacy, and, especially, attentiveness to politics. Basic information is the most consistently patterned of all the attitudes. It seems to be a fundamental prerequisite to the interest and awareness which stimulate participation. (We must recall at all times, however, the strong component of organizational involvement which did *not* operate through any of the attitudes, including information.)

In each nation about 1/3 of the correlation between status and participation was explained by paths passing through information. This has interesting implications for political stability and coalition consistency in developed nations, given the research showing strong relationships between information levels and consistency of vote, inter-relatedness of belief systems, and the like. The growth of a middle class, as political scientists since Aristotle have observed, provides an anchor of political stability. One linkage may be the development of basic information levels among citizens likely to participate consistently.

Perceived stake in society.—There is a long tradition of speculation about participation which suggests that the "better elements" in the society participate more frequently because they have more of a stake in the outcomes of political decisions. Related to this speculation is the observation that higher status and more organizationally involved citizens are closer to the "cen-

[15] See Almond and Verba, *op. cit.*, pp. 161–179; and Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley and Sons, 1960), pp. 105–106. For a general review, see Richard Dawson and Kenneth Prewitt, *Political Socialization* (Boston: Little, Brown and Co., 1969), especially chapters 9 and 10.

[16] Alker, *op. cit.*; Karl W. Deutsch, "Social Mobilization and Political Development," this REVIEW, 55 (September, 1961), 493–514; Daniel Lerner, *The Passing of Traditional Society* (New York: Free Press of Glencoe, 1958); and see the essays in Lucian W. Pye (ed.) *Communication and Political Development* (Princeton: Princeton University Press, 1963). It should be emphasized, however, that we are here dealing with very fundamental types of information, as captured by these responses naming party leaders and cabinet positions at the national levels. More detailed and sophisticated types of information might be expected to follow such factors as involvement and participation, rather than act as a basis for them.

ter" of society, and more likely to be in touch with government in their daily economic and social routines. Although we have no data to test the idea that higher status citizens actually perceive greater need to contact officials, there were questions asking citizens about the level of impact which, they felt, local and national governments had on their daily life.

Contrary to the speculations, perception of a general "stake" of this kind was not an important link between status or organizational involvement and participation. In most countries the link was negligible, and correlations spurious. Although generally related to political information, the sense of governmental impact (variable #5 in the model), had only weak links to attitudes further up or down the chain. At least as measured here, the hypothesis must be rejected.

Social competence and political efficacy.—Higher social status and organizational involvement, it has been argued, provide a citizen with a general sense of competence and importance; these feelings are transferred from the social to the political world. The socially advantaged citizen assumes that political events can be manipulated and controlled and this sense of political efficacy increases the likelihood he will participate in politics. Variable 6 is a sense of political efficacy, as measured by a number of indices, including the feeling that one understands political events, the perception that access channels exist in case of grievance, and the expectation of success if active participation is undertaken.[17]

Although we have no data on feelings of general social competence, it is clear that political efficacy does play a strong role in the relationship between social class and participation. What is striking is the importance of efficacy as an intervening variable in paths from the other intervening variables.

Efficacy does, however, show a great deal of variation from nation to nation. There are strong downward links to attentiveness in all cases, and very strong direct links to participation in two nations (Germany and Italy). We cannot explain the differences, however, which range from Italy, where half of the relationship between status and participation is explained by paths through efficacy, to Great Britain, where its role is a negligible 6%.

[17] As noted in Part I, footnote 15, efficacy is not a simple concept and other dimensions of it must be explored in other analysis. Efficacy as measured here is a sense of *personal* ability to cope with the political world. It seems to be a somewhat different concept from sense of approval or satisfaction that the political system is generally responsive.

One further point has bearing on the efficacy hypothesis. In the course of analysis we decomposed our dependent variable, political participation, and examined the causal model in terms of each of its components. All the general patterns remained the same, confirming our faith that the scale was built from a relatively consistent dimension, except that political competence was more important as a link to the more active forms of participation, such as contacting local officials, than it was in talking politics. Attentiveness, on the other hand, was far more strongly related to talking politics, although still powerful in all cases. Thus a sense of efficacy, it would seem, is a more important resource for initiating government contacts than for less active forms of political involvement. More complete findings of this path analysis by type of political activity are reported in Appendix II.

Political attentiveness.—The last variable in the attitude chain was political attentiveness, a scale built from whether citizens reported following political affairs in general and through specific mass media. A frequently heard hypothesis is that higher levels of social status and organizational involvement increase the general awareness of social happenings and their consequences. As we have seen, increased levels of information seem to provide a resource for such attentiveness. This general awareness spills over into political awareness, which provides a resource and stimulus for political participation.

Our data confirm the critical linking role of attentiveness to politics, although we do not have data to relate general social awareness to political attentiveness. The pattern suggests that status leads to acquisition of attitudes which prepare the individual for active political involvement when appropriate stimuli or needs arise. The consistency of the attentiveness variable as the final link in the chain emphasizes the way in which these attitude distributions indicate generally attentive "publics" which are likely to play a consistent political role.[18] Attentiveness paths always account for at least one-fourth of the relationship between status and participation, and range to a high of two-thirds in Italy.

The most consistently important single paths through the attitude variables are from status and involvement to citizen duty and, particularly, information, which then act on efficacy and attentiveness, which in turn affect probability of participation.

[18] See V. O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, 1961); and also Gabriel A. Almond, *The American People and Foreign Policy* (New Haven: Yale University Press, 1954).

The variation in the roles of these intervening variables should not obscure one overwhelming fact: they explain much of the influence of social status; they have far less explanatory power in linking organizational involvement and participation. We must go beyond this causal model in developing our understanding of these very important differences. A more general perspective on the amount of political participation being explained can perhaps be gained from Table 3. The increasing percentages given in the table are the squares of the coefficient of correlation or multiple correlation with participation for the various independent variables, or independent *plus* intervening variables. Social status alone, obviously, only explained about 9 percent of the total variance in participation. It is this causal relationship which has been most adequately explained in our analysis. The addition of group involvement explains an additional 18 percent of the variance in participation, a fact which is both a tribute to the strength of the variable and a challenge to further investigation, since about two-thirds of that relationship has not been explicated by our model. Addition of all attitude variables explains another 16 percent —the total model explaining about 43 percent of the variance in participation. It is, of course, unclear to what degree the total explanation could be increased through addition of further variables, and to what degree a ceiling is provided by problems in empirical survey research. In any case, the ability to explain close to half of the variance in a comprehensive scale of political participation and to relate the variables in a fashion which lends itself to great consistency in cross-national investigation gives considerable confidence in the basic analysis.

III. URBANIZATION AND LOCAL PARTICIPATION: THE CAUSAL LINKAGE PATTERNS

In Part I. of the analysis we showed that within each nation an individual's place of residence bore little relationship to his rate of political participation. For this reason we have disregarded size of place of residence as a variable in conducting our analysis of the attitudinal linkages between the social structural variables and political participation. As a double check on the findings about urbanization reported in Part I, we recomputed the causal model introduced in this part of the analysis, adding urban place of residence as Variable 1. The results were the same; the direct and indirect linkages were not altered by adding urbanization. We confirm the initial finding that knowing the size of the community in which a citizen lives adds nothing to

TABLE 3. AMOUNT OF PARTICIPATION EXPLAINED AS ADDITIONAL VARIABLES ARE ENTERED INTO THE CAUSAL MODEL

Independent Variables Added	Nation					Average Percent Explained	Average Percent Added
	US	UK	GER	IT	MX		
SES (r^2)	18%	3%	9%	8%	6%	9%	+ 9%
SES+Org.I (R^2)	32	24	26	26	28	27	+18
SES+Org.I.+Duty+Inf +Impact+Eff+Attent (R^2)	44	44	43	47	38	43	+16

Key: SES = Socio-economic Status
Org.I. = Organizational Involvement
Duty = Citizen Duty to Participate
Inf = Political Information
Impact = Perceived Impact of Government
Eff = Political Efficacy
Attent = Political Attentiveness
US = United States
UK = United Kingdom
GER = Germany
IT = Italy
MX = Mexico

our understanding of his general level of political participation.

Readers of Part I will recall that we carried the analysis of urbanization and participation one step further. We demonstrated that whereas the participation scale was unaffected by size of place of residence in our five nations, participation in local politics was somewhat influenced. Living in larger cities decreases the probability that the citizen will participate in local affairs. This finding has been tested again, but this time using the causal model. Where possible, the attitude measures were converted into indices having a local referent: sense of duty to participate in local affairs, perception of the impact of local government, sense of efficacy in local politics. The causal model, adding as independent variables the size of community in which the citizen lives and his length of residence there, was then recomputed. (See Appendix III for the path coefficients for the ten-variable recursive model of local participation.) Size of community is consistently *negatively* related to the attitude items and to actual local participation. Although the degree of negative influence is not great, accounting for about 2 percent of the variance in participation in local affairs; it is clear that moving to a larger community *reduces* the average citizen's sense of involvement, his feelings of efficacy, and his actual participation in local affairs.[19]

IV. SOCIAL STRUCTURE AND THE PARTICIPATION OF CLASS GROUPS[20]

Individuals vary in their political participation according to their social status and according to their organizational involvement, with the latter social characteristic having the stronger impact on political participation. This finding is consistent from one nation to another. Further, and because there is cross-national consistency, both social status and organizational involvement remain strongly related to political participation when we shift from the individual to the nation as a unit of analysis. That is, nations vary in their level of mass political participation according to the proportion of citizens in the middle and upper classes and according to the density and complexity of organizational life.

In addition, organizational involvement may represent an alternative channel for political participation for socially disadvantaged groups. The rural peasant, the industrial laborer, the disadvantaged black may become politically active through his organizational involvement even though he may otherwise lack the status resources for political participation. In these ways the class structure and the organizational structure of societies determine both the overall rate of participation and the relative amount of participation coming from various social groups.

We have argued that economic development changes both the class and organizational structures of a society. This is true, but other factors may also promote such changes. The class structure is, of course, intimately tied to the long-term development of human and capital resources. Governments may establish massive education programs and engage in forceful income redistribution. These efforts may well affect participation patterns. However, obviously major changes in the status structure, involving occupation, education, and income patterns, are extremely difficult to bring about. We suspect that the organizational structure may be susceptible to more direct and short-term manipulation. Assuming that our organizational correlations reflect causal patterns, rather than the operation of self-selection mechanisms (an assumption we feel is tenable), then the great influence of organizational structure on participation suggests important possibilities for political change.

Although there are few systematic and comparative studies of the process of organizational development, there are numerous examples of efforts by governmental and extra-governmental bodies to generate organizational structure. Religious groups, for example, have spawned an enormous variety of organizations. Governments have fostered—and impeded—the development of both political and non-political organizations such as farmers' associations, professional societies, trade unions, and youth groups. The European socialist movements and the accelerating actions of civil rights groups in the United States in the past few years are additional examples of group mobilization only partially related to economic development patterns. We

[19] Carrying out the equivalent operation for participation in national politics yields even less distinctive patterns. Direct linkages between place of residence and contacting national officials are virtually zero in each nation; the basic interactive patterns analyzed in the general model are still the overwhelming ones; only sense of national efficacy shows a significant—and still very slight—positive relationship to living in larger cities (and only in three nations of our five). The effect of urban residence on patterns of contacting national officials, in other words, is possibly measurable, but very, very weak. No more than one percent of the variance in the national participation item is affected by urban residence in any of the five nations.

[20] We are indebted to Mr. Jae-On Kim for help with some of the methodology required for our analysis in this section.

should not underestimate the complexity of the problems. The new African states have discovered the difficulties in penetrating a pre-mobilized economic structure even with a mass political party, as the numerous coups in recent years illustrate. And the history of socialist and labor movements suggest the frequency of middle class leadership in initial generation of working class movements. Nonetheless, the historical examples suggest a potential manipulability of the organizational structure, as well as a potential for mediating the effects of social class, which make it a central variable in the analysis of participant societies.[21]

The shape of the class structure, the extent of organizational involvement, and the relationship between social status and organizational membership are only partially determined by economic development levels. And these three factors have a complicated but identifiable impact on the overall rate of participation as well as on the amount of participation coming from the various social classes. In this section we explore how different mixtures of these factors affect the degree to which class groups will be under- or over-represented in the "stratum" of political participants in a society. An example illustrates what is meant by these terms as we employ them. Suppose that one-fourth of the citizens are active participants (by some explicated criterion) in the political life of their society. Suppose further that lower class peasants and industrial workers numbered half of the total society. Also suppose that a very limited proportion of this group were members of organizations. All of our previous analysis would therefore suggest that while this class group represented 50 per cent of the total society, it would probably represent a *much* smaller proportion of the 25 per cent who are active political participants. We would conclude that the peasants and workers were "under-represented" in the political stratum.

On the basis of the findings reported above and in Part I of this study, we can make some reasonable estimates about the effect on the level of mass political participation when changes occur in the society's class or organizational structure. We can also project or "simulate" the extent to which various class groups contribute more than or less than their share to the participant stratum. The basis of this analysis is the estimation of the participation level of the organizational members and non-members among the different class groups. This estimation is based on the matrix of correlations among the three variables of status, organizational membership, and political participation. For instance, of the citizens who are lower status but organizationally active, what proportion are likely to be politically active? Of the citizens who are middle class and not organizationally involved, what proportion will likely be political participants? And so forth. The changing size of these groups of citizens along with the strength of the association among the factors yield a projection of the changing participation patterns.

Once the correlation among the variables is known, the projections can be made by translating the correlation coefficients into percentage differences. This is by no means an exact process, but rather one of gross estimation —however, there are some guidelines in the statistical literature.[22] For our five nations the average correlation between social class and political participation is .30 and between organizational involvement and participation, about .50. The following estimates are indicated:

(a) With a correlation of .30 between social status and political participation, the higher social status groups in the society will have 15 to 25 per cent more political participants than the lower groups. (Assuming social status to be trichotomized and participation to be dichotomized.)

(b) With a correlation of .50 between organizational involvement and political participation, the organizationally involved citizens will have 30 to 40 per cent more political participants than the non-involved citizens. (Assuming organizational involvement and participation both to be dichotomized.)

[21] It may also be true that the development of different types of national industries may create differential needs for economic organizations depending upon industry requirements to coordinate production, transportation, marketing and the like. See, for example, Barrington Moore, Jr.'s discussion of the differential impact that varying types of national industry have on the social structure, in *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966).

[22] See for interpretation of percentage differences, Robert H. Somers, "A New Asymmetric Measure of Ordinal Variables," *American Sociological Review*, 27 (1962), 799–811. Also for interpretation of percentages for 2×2 tables as "proportional reduction of error" comparable in meaning to the square of the product moment r, see Jae-On Kim, "Predictive Measures of Ordinal Association;" unpublished manuscript. We are also indebted to Mr. Kim for helping us work out these percentage differences in our data.

Given these percentage differences, a return to the actual data provides us with a realistic base line—approximately 15% of those defined as lower class who are not members of any organization can be classified as politically active. This base line approximates the proportion who report discussing politics at least once a week or some more active type of participation. (See other items in the participation scale in the Appendix to Part I.) Using this base figure and the percentages differences defined above, we can estimate the rates of political participation for the various mixtures of social class and organizational involvement.[23]

Upper Class,
Organization Members = 70% Participants
Non-Members = 35% Participants
Middle Class,
Organization Members = 60% Participants
Non-Members = 25% Participants
Lower Class,
Organization Members = 50% Participants
Non-Members = 15% Participants

These estimates permit us to make two types of projections. First, we can estimate the overall increase in mass political participation when the class structure of society is altered or when the organizational life of society is altered. Table 4 presents illustrative analysis. Nation #1 has a class structure corresponding to that of a relatively underdeveloped country: 70 per cent of its citizens are lower class, 20 per cent are middle class, and 10 per cent are upper class. Futher, this nation has little in the way of organizational infrastructure. Only 15 per cent of the citizens belong to organizations. Nation #2 resembles the previous nation in its class structure, but it has a much richer group life—60 per cent of its citizens belong to organizations. Nation #3 is a "middle class" country; its class structure, shaped like a diamond, resembles that of a relatively industrialized society. Twenty-five per cent of its citizens are lower class, 50 per cent are middle class, and the remaining quarter are upper class. This nation, however, deviates from most "middle class" societies in that its group life is underdeveloped; only 15 per cent of its citizens belong to groups. Finally, Nation #4 has both a diamond-shaped class structure and a well-developed organizational life. The last column in Table 4 presents a projection of the proportion of citizens who are politically active in each of the nations.

According to the simulation, the middle-class and organizationally developed society (#4) has nearly twice as many citizens actively involved in political life as does the society with a class structure resembling that found in many non-industrialized nations and where there is little organizational infrastructure (#1). Thus we see, as shown in our five nations in Part I, that the number of citizens actively involved in political life varies according to the shape of the stratification system in the society and the complexity of its organizational infrastructure, with the latter characteristic showing the stronger relationship. (Nation #2 has more participants than Nation #3.)

Estimating participation levels of different groupings in society permits us to make a second set of projections as well. Altering the shape of society's class structure and its distribution of organizational members does more than change the absolute level of political participation. It also changes the *relative contribution* of various class groups to the political participant stratum. We have noted that (1) because social status and political participation are positively correlated, the lower classes are always under-represented among the political participants; and (2) that for the lower status citizen, his involvement in organizations can somewhat compensate for his lack of other resources and can provide a channel for political access. Moreover, because the impact of organizational involvement may overweigh that of social status in societies with

TABLE 4. ESTIMATING THE PARTICIPATION LEVEL OF SOCIETIES DIFFERING IN THEIR CLASS STRUCTURE AND DENSITY OF THEIR GROUP LIFE*

Nation	Given Social Conditions		Projected Participation
	Class Structure of Society: Proportion in Low, Middle, Upper Classes	Organizational Density: Proportion of Citizens Belonging to Organizations	Proportion of Citizens Who Are Politically Active
#1	70—20—10%	15%	25%
#2	70—20—10	60	40
#3	25—50—25	15	30
#4	25—50—25	60	46

* Correlation between class and participation is assumed to be .30. Correlation between organizational membership and participation is assumed to be .50. For explication of the projections in this table, see Appendix iv., below.

[23] Note how closely these projective estimates come to the actual data presented in Table 4 Part I of this article.

an extensive and complex group life, the relative disadvantage of the lower class citizens is less, providing that the distribution of organizations is equal among the social strata.

If we assume that different class groups have different political goals and interests, then the persistent under-representation of lower class groups is a politically significant fact. However, caution must prevail in drawing inferences, since there is no way to move directly from the degree of under-representation of a class group in the participation patterns to a statement about the distribution of social values. For instance, there may be limited unity on certain issues among citizens sharing a similar class position, or limited perception of shared class interests. Further, "active participant" as we use the term is an arbitrary category; there may be checks through electoral participation and the anticipations by leaders of electoral sanctions even when direct contacts are not made with the political leaders.[24]

Nonetheless, the systematic under-representation of the lower class groups is a base-line fact in all the participation relationships examined. It is, therefore, of some importance to find that the effect of organizational membership *could* alter patterns of disproportionate contribution by class groups to the participant stratum. Table 5 presents the data originally presented in Table 4 plus additional relevant materials. Again, it is based on simulating the different variables in order to show their effect on the relative representation of social class groups among the political participants. In Table 5, column I shows nations with two different class structures as previously discussed. Column II shows nations with two different organizational structures, either 15 per cent or 60 per cent of the citizens belonging to groups. Column III gives the stipulated relationship between social status and organizational membership. In the examples chosen we show nations in which there is no relationship between status and membership, (organizational memberships are distributed evenly throughout the status groups), and nations with a strong (.5) relationship between status and organizational membership, (upper class persons are much more likely than lower class persons to be organizationally involved). In column IV we show the computed class structure of the active participants in society; this is computed from the size of the status-organizational membership groups and the participation of each group as discussed previously. (See Appendix iv for methodological details.) The proportion of lower, middle, and upper class citizens among the active participants can be compared with their proportions in the total population shown in the first column. Finally, in column V, we show the difference between the lower class's proportion of political participants and its size as a group in the society, divided by its total size. This is an index of the under-representation of the lower class in nations differing in their class structure, organizational life, and strength of relationship between social status and organizational membership. All the index scores are negative since the positive correlation between status and political participation means that the lower class is always to some degree underrepresented.

The simulation presented in Table 5 underlines several themes already introduced during the discussion of the manner in which social status and organizational involvement provide alternative routes into the politically active stratum of society. We can conclude that:

(1) The higher the total organization level, the less the lower class is under-represented. Increasing organization level from 15 to 60 per cent reduces lower class under-representation scores by about one-third. (Compare lines 1 and 3, 2 and 4, 5 and 7, 6 and 8.)

(2) The higher the correlation between status and organizational membership, the more under-represented the lower class. A correlation of .5 increases under-representation in these cases by at least one-half. (Compare 1 and 2, 3 and 4, 5 and 6, 7 and 8.)

(3) The smaller the size of the lower class, the more it is, relatively, under-represented. The consistent magnitude of the distortions in participation rates weighs more heavily against a basically smaller group. Thus, other factors equal, the diamond-shaped system has a smaller lower class, but one whose participatory representation suffers relatively more from the relationship between status and participation. The effect in these cases doubles under-representation. (Compare 1 and 5, 2 and 6, 3 and 7, 4 and 8.)

[24] The need for caution in moving from analysis of social stratification, and even gross patterns of political participation, to assumptions about political decision-making patterns and outcomes has been amply demonstrated in the various studies of community power. See especially Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961); and Nelson W. Polsby, *Community Power and Political Theory*, (New Haven: Yale University Press, 1963). Factors shaping participation patterns must be incorporated as *one* element in such analysis.

TABLE 5. SOCIAL STRUCTURE AND CLASS GROUP PARTICIPATION: A SIMULATION

Simulation No.	Given Conditions			Projected Participation Patterns	
	I	II	III	IV	V
	Class Structure of Society: Proportion in Low, Middle, Upper Classes	Organizational Density: Proportion of Citizens Belonging to Organizations	Class-Organization Correlation	Class Structure of Politically Active: Proportion in Low, Middle, Upper Classes	Index of Underrepresentation for Lower Classes[a]
1	70% −20% −10%	15%	.0	59% −25% −16%	−16
2	70 −20 −10	15	.5	51 −28 −22	−27
3	70 −20 −10	60	.0	63 −23 −14	−10
4	70 −20 −10	60	.5	59 −25 −16	−16
5	25 −50 −25	15	.0	17 −50 −33	−32
6	25 −50 −25	15	.5	13 −50 −37	−48
7	25 −50 −25	60	.0	20 −50 −30	−20
8	25 −50 −25	60	.5	16 −50 −34	−36

[a] Computed by (1) subtracting the per cent of active participants in the lower class from the total per cent in the lower class, and (2) dividing the difference by the latter. For full explanation of the projections in this table, see Appendix IV, below.

The utility of the simulation analysis and the implications of the patterns reported in Table 5 will be clearer if we shift attention to the actual data from the five nations. That is, we can carry out the same analysis and show the degree of under-representation by the lower classes in each of the five nations we have used in this study. We will use cutting points which, with one exception, are identical from nation to nation to show the empirical data equivalent to that simulated above.[25] Table 6 presents the data.

The most striking pattern in Table 6 is the extent to which the lower class is under-represented among the politically active citizens in the United States; the lower class is more severely under-represented there than is true in any of the other nations. The simulations in Table 5 suggest the reasons for this (Note that the U.S. case corresponds roughly to simulation 8; Mexico to simulation 1.) The severe under-representation in the United States results first from the relatively *small size* of the lower class, which is confronted with a basic correlation between class participation which is higher than average. (Although it is not a powerful part of the phenomenon, the partial correlation between status and participation, controlling for organizational membership, remains higher in the U.S. than in the other nations. We have no explanation for this fact.) Secondly, while the high level of organization in the United States might have canceled out the effect of the relatively small class size (compare simulations 1, 5, 7), the high correlation between social class and organizational involvement (.44) eliminates the potential help offered by the organization structure. See Column III.

It seems likely that the high correlation between social class and organizational involve-

[25] Since our scales were derived to give intra-nation distributions of maximum utility, their validity for cross-national absolute comparisons is somewhat limited. Nor is it entirely clear how one should treat an indicator like "years of education" as a social status indicator on a cross-national basis. The meaning of such absolute measures, like the real buying power of formal income equivalents, depends considerably on the distribution of the commodity within the society. Nonetheless, the chosen cutting points fitted four of the societies in fairly plausible fashion. Actual distribution of education in the United States, for example, reported in the 1959 census was about 17% college educated, 48% 9–12 years of education, and 35% with 8 years or less. (Compare the class distribution in Table 6.) Only in Britain was a slightly different scale cutting point used—the division between lower and middle class was lowered by one level to show 46% in the lower class rather than 60%. This seemed a more plausible distribution in comparable terms, although it does not greatly affect the class analysis.

TABLE 6. SOCIAL STRUCTURE AND CLASS GROUP PARTICIPATION: FIVE NATIONS

Nation	Given Conditions			Overall Participation Rate: Per Cent Classified as Politically Active[a]	Projected Participation Patterns	
	Class Structure of Society: Proportion in Low, Middle, Upper Classes	Organizational Density: Proportion of Citizen Belonging to Organizations	Class-Organization Correlation		Class Structure of Politically Active Proportion in Low, Middle, Upper Classes	Index of Underrepresentation for Lower Classes[b]
United States	30%—45%—25%	55%	.44	46%	16%—47%—37%	−47
Britain	46 —38 —16	49	.31	45	32 —43 —25	−30
Germany	44 —41 —15	39	.21	40	41 —36 —23	− 7
Italy	54 —32 —14	27	.30	27	42 —39 —19	−22
Mexico	77 —16 — 7	20	.23	25	64 —25 —10	−17

[a] Being classified as "politically active" is roughly equivalent to discussing politics at least once a week, or more active involvement, as measured by our original scale items. Appendix B, Part I, reports this scale.

[b] Computed by (1) subtracting the per cent of lower class among active participants from the total per cent in the lower class, and (2) dividing the difference by the latter. See Appendix

ment in the United States reflects the fact that in the three European countries there existed historical efforts by political parties to mobilize the lower class and, especially in Germany, to create an organizational structure deliberately parallel to that which had traditionally evolved among the established strata. Even where there may have been a considerable subsequent disengagement of the parties from organizational domination, the effects of such mobilization may have continuing consequences. Lipset and Rokkan have suggested the importance of the political organizational structures initially established during the social mobilization of the society in setting the party alternatives for the society in subsequent years.[26] Their analysis may well apply to the initial formation of organizational infra-structures more generally. In Mexico, the political regime itself has engaged in a protracted attempt to create a lower class infrastructure. For various reasons, such deliberate attempts at group formation in the United States have been more limited, and particular socio-economic features may have also inhibited the development of lower class organizations.[27]

Explanation of these historical patterns is beyond the scope of our analysis. But it is at least clear from the American case that the growth of a lower class organizational structure is *not* part of the natural process of economic development. In fact, the consistent positive correlation between organizational involvement and social class points just in the opposite direction. As Olsen and other theorists of the costs of organizational development have suggested, the consequent "natural" evolution has limited the formation of mass organizations among groups with low resources.[28]

[26] Seymour M. Lipset and Stein Rokkan, *Party Systems and Voter Alignments* (New York: Free Press, 1967), pp. 50–56.

[27] A wide range of possible reasons for this failure of group mobilization of the American lower classes has been suggested. These include the presence of the frontier, the general belief in social mobility and economic progress, the tremendous linguistic, ethnic, and religious differences during the period of immigrations, and the lack of traditional social structures such as guilds. Others have suggested more specifically political factors. For example, Walter Dean Burnham, "The Changing Shape of the American Political Universe," this REVIEW, 59 (March, 1965), p. 24. A unified interpretation is obviously beyond the scope of the present discussion.

[28] Mancur Olson, *The Logic of Collective Action* (Cambridge: Harvard University Press, 1965). The organizational membership cutting points are the same from nation to nation; they correspond closely to the percentage of citizens in each nation reporting membership in one or more organizations (U.S. 57%; U.K. 47%; Germany 44%; Italy 30%; Mexico 24%), although our scale is composed of several items, rather than membership alone.

The evidence presented here, both actual data and the simulation, suggest a basis for analysis of trends in political participation and group representation. If, for instance, it proves to be the case, as recent data suggest, that the American lower class black population is developing a secondary group structure more rapidly than other similarly located groups in society, one should expect an emergent pattern of broader based representation in the political stratum—a movement from simulation 8 to simulation 7 in Table 5.[29] A more complete understanding of these processes, *and* of the reliability of the relationships under differing conditions, might enable us to foresee some of the outcomes of patterns of social change in various national settings.

However, although alerting us to conditions which affect the rates of political participation and the relative contribution of different social groups to the participant stratum, the simulation as presented ignores the issue of the "quality of participation." We saw in the previous section that persons mobilized into a politically active role through their organizational involvement need not share the attitudes normally thought conducive to "rational" political involvement. In the conclusion we attempt to sort out some of the issues connected with the costs and benefits of more equitably distributed access to the participant stratum.

V. SUMMARY AND CONCLUSIONS

Evidence from five nations indicates that the relationship between economic growth and increases in mass political participation is largely explained through two changes in the social structure of society: larger numbers of upper and middle class persons, in both relative and absolute terms; and the emergence of organized economic and social groups. Stating the relationship between economic development and mass political participation in terms of structural changes in society provides strong empirical correlations and leads to inclusive social theory. The frequently cited correlations between such individual traits as years of education or number of organizational memberships and the citizen's specific acts of political involvement can be subsumed into the more general theory advanced in this study.

At the national level economic development is associated with urbanization, with the growth of secondary groups, and with the expansion of the white-collar class. Each of these variables has been cited as producing mass political participation. We find, however, that at the individual level urban residence has little effect on participation. Living in larger cities has some *negative* impact on *local* participation, but the total amount of variance explained is slight. An even weaker relationship between urban residence and national participation seems purely a function of the concentration of status resources in urban areas in these systems.

There is less of a correlation between social status and organizational involvement than is sometimes supposed. In their impact on political participation, these characteristics clearly can operate independently of one another. In addition, social status and organizational involvement are differentially linked to participation by intervening political attitudes.

Social status tends to affect political participation through its impact on political attitudes and cognitions which, in turn, facilitate political activity. Although the findings are tentative, the data do suggest that the most important causal link is the creation of *attitudinal resources* that sensitize an individual to political messages and provide him the sense of competence needed to engage in political behavior. There is also evidence indicating that higher status exposes the citizen to learning situations in which the duty to participate is stressed. Acquiring this normative predisposition leads the citizen to acquire other attitudinal resources, such as information and competence, which further increase his probability of political participation. (There is, however, wide variance from nation to nation in the importance of an attitude of civic duty and we present these remarks very tentatively.) Finally, the data tend to reject the hypothesis that the well-to-do citizens participate more frequently in politics because they perceive a higher stake in the day-to-day conduct of political affairs. Correlations between such views and participation appear to be spurious.

Involvement in organizations can also produce the attitudinal resources which facilitate political participation. In some cases, at least, organizational involvement is a source of the sense of a normative obligation to participate (in Germany, a stronger source than status). However, in the main the extremely strong relationship between organizational involvement and political participation does not operate through the acquisition of attitudes and cognitions which facilitate participation.

The causal links connecting social status with political participation, links which create attitudinal resources enabling the individual to participate when opportunities occur, are a minor part of the causal impact of organizational involve-

[29] Anthony M. Orum, "A Reappraisal of Social and Political Participation of Negroes," *American Journal of Sociology*, 72 (July, 1966), 32–46.

ment. There is either some sort of self-selection effect which ushers the same citizens into both political activity and organized group life, or there is a strong role played by organizations in mobilizing group members for political activity. The latter interpretation is the more likely, although the two are not mutually exclusive. But such mobilization, if it occurs, does not necessarily result in or reflect higher levels of general political information or awareness. Apparently mobilization opens direct lines to participation, or provides attitudinal resources relevant to specific problems only. There might be, for instance, group-initiated political discussion, group-organized contacts with political authorities, or group-related political information relevant to a specific issue.

Implications—The findings reported in this research paper have implications more far-reaching than those thus far discussed. The findings are, of course, closely related to an understanding of some of the stresses and strains in mass democracies. A research report of this type is not the most useful vehicle for speculating about issues of democratic theory, but insofar as our data suggest some observations, we would be remiss to ignore them completely. Let us at least consider what the patterns may imply about one particularly critical problem: the paradox of mass involvement in political life.

First, the data confirm one well-known finding: Even in industrialized nations a majority of citizens do not participate very actively in politics and do not have the attitudinal resources which lead to citizen control of public policies.

The data support social theories which suggest that industrialization is a necessary condition for the establishment of mass involvement in democratic politics.[30] A key component in this theory is the observation that the tendency to get involved in politics, and the attitudes associated with involvement are not randomly distributed in society. Rather, these tendencies and attitudes tend to cluster in the middle and upper classes. Political life styles of citizens will not be markedly changed until extensive industrialization alters the status structure of society, and thereby increases the overall level of political information, attentiveness, efficacy, and so forth.

Second, the data suggest the critical role which the structure of organized group life may play in the overall level of mass political participation. It appears that the richness and complexity of organizational life might be altered somewhat independently of economic development. Deliberate governmental policies, for instance, can increase the number of citizens who are politically active. Mobilization parties in the single-party states of Africa are one example of how this might happen. Unionizing the labor force, even if comprised of peasant farmers, is another illustration. That is, although historically the growth of secondary groups has been associated with economic development, mobilization politics need not await industrialization. New stimuli to group organization may provoke new patterns of growth, although we observe that complexities and difficulties (beyond this analysis) have met such efforts in practice.

Organizational membership, then, could be a political resource for the lower classes. Were this resource equitably distributed, the criteria for pluralistic democracy advanced by Dahl, Key and others would be more nearly met.[31] That is, all social strata in the society would participate in those political processes which presumably lead to control of political leadership and through this control to influence over public policy. Group mobilization could attract into political life larger numbers of those persons who presently are political isolates. These citizens need not have the enabling antecedents, such as higher levels of education, now thought to be necessary conditions for political participation. Alterations in the organizational structure, then, can serve to correct the tendency for even the most democratically organized societies to allow a disproportionate amount of political influence to be exercised by the well-to-do.

These observations about the organization-related potential for increasing political participation should be advanced cautiously, however.

[30] For example, Seymour Martin Lipset, *Political Man* (Garden City: Doubleday and Co., 1960), pp. 27–63. Also see Philip Cutwright, "National Political Development," in Nelson W. Polsby, Robert A. Dentler, and Paul L. Smith, (eds.), *Politics and Social Life* (Boston: Houghton Mifflin Co., 1963), pp. 569–582. In his critical analysis of Lipset and Cutwright, Deane Neubauer argues that economic development may be a necessary condition for democracy, at least to some threshold, but that the relationship is not linear, and does not seem to hold within the subset of more developed systems he examines. See Deane Neubauer, "Some Conditions of Democracy," this REVIEW, 61 (December, 1967), pp. 1002–1009.

[31] Robert A. Dahl, *A Preface to Democratic Theory* (Chicago: University of Chicago Press, 1956); Key, *op. cit.*; and see the exchange between Jack Walker, "A Critique of the Elitist Theory of Democracy," and Robert A. Dahl, "Further Reflections on the 'Elitist Theory of Democracy,'" this REVIEW, 60 (June, 1966), 285–305, and Communications to the Editor.

For one thing, we reemphasize that in every nation examined social status and organizational involvement were somewhat correlated, though the relationship is not determinate. Indeed, it is important for the student of American politics to note that the distribution of organizational resources is more intertwined with the distribution of status resources in the United States than in any of the other four nations in the study. The natural growth of organizations, as Mancur Olson has suggested, seems to be one which places at a disadvantage very large potential groups when the individuals who might make up those groups have limited individual resources.[32] The cost to each individual of organization building is discouragingly great. It is probably no coincidence that those nations in which both political parties and governmental bureaucracies have been most active in encouraging secondary group formation show less upper class domination of the organized life of the society. What must be concluded is that processes of economic development will not automatically help redress class participation imbalances through the growth of secondary groups.

Another note of caution must be voiced as well. One cannot be certain, particularly on the basis of data presented here, about the quality of organization-based political participation. The individual mobilized by his group membership lacks the attitudes usually associated with political involvement. This may mean that the content of his effort to influence will be largely determined by the mobilizing leaders, a possibility suggesting that organizations are more the base of a "counter-elite" than a source of broader and more democratic patterns of political influence.

Whereas our findings suggest a potential for participatory democracy which is in optimistic contrast to most modern empirical studies of mass participation, these same findings raise again the specter of mass "irrationality" and "ignorant" involvement which has haunted democratic theorists since the Greeks. Organizations mobilizing their members and providing resources for their leaders present the possibility of large numbers of political activists who do not have the attitudes usually assumed to be associated with involvement in political life. Political information, a sense of civic duty, political attentiveness, and a sense of political efficacy are not preconditions for political participation among the organizationally involved.

Just what this implies is not entirely clear from the data at hand. While some modern American theorists have placed great stock in the moderating influence of norms of tolerance and accommodation, and of the gate-keeper role of those thoroughly socialized into recognition of democratic procedures and processes,[33] it is not evident that such norms are necessarily the property of elites rather than masses. If American data showing the informed substratum to be more tolerant and democratically oriented, although also more split on political issues, are correct, then group mobilization in this system is likely to be associated with higher levels of conflict and intolerance.[34] On the other hand, there is some evidence that in systems with a more ideological and conflict-ridden history, the better informed and more active citizens may possess belief systems into which deeply rooted hostility and suspicion of the opposition, as well as intransigent issue positions, are firmly integrated.[35] Such would seem to be the case in nations such as France, Austria, and Italy. In these circumstances, group mobilization of a certain type might draw into the system citizens oriented more pragmatically to specific issues and problems, who might press elites to abandon traditional attitudes of rigidity.

The problem is further complicated by the facts that (1) the relationship between basic attitudes, such as information and attentiveness, and the specific affective and normative content of opinions, has not been established; and (2) the possibility that under some conditions group activity itself may structure affective attitudes must be brought into the analysis. In nations where organized groups are linked to partisan camps, and few groups cut across party, class, and religious lines, then involvement itself may reinforce suspicions and hostilities. Thus, in nations such as Italy and Austria, where group members and leaders seem to be more distrust-

[32] Olson, *op. cit.*

[33] Dahl, *op. cit.*; and Key., *op. cit.*

[34] James W. Prothro and Charles M. Grigg, "Fundamental Principles of Democracy," *Journal of Politics*, 22 (1960), 276–294; Samuel Stouffer, *Communism, Conformity, and Civil Liberties* (New York: Doubleday and Co., 1955); Herbert McClosky, Paul J. Hoffmann, and Rosemary O'Hara, "Issue Conflict and Consensus among Party Leaders and Followers," this REVIEW, 59 (June, 1960), 406–427.

[35] See, for example, Duncan MacRae, Jr., *Parliament, Parties and Society in France, 1946–1958* (New York: St. Martin's Press, 1967.) Also, for suggestive data from a single community, G. Bingham Powell, Jr., *Fragmentation and Hostility in an Austrian Community* (Stanford: Stanford University Press, forthcoming).

APPENDIX I. STANDARD ERRORS FOR STANDARDIZED PATH COEFFICIENTS IN EIGHT VARIABLE RECURSIVE MODEL REPORTED IN TABLE 2

Dependent Variable	Equation	Country	Standard Error for:						
			B_1	B_2	B_3	B_4	B_5	B_6	B_7
X_2	$=B_{21}X_1+B_{2t}R_t$	US	.046						
		UK	.013						
		GR	.037						
		IT	.027						
		MX	.018						
X_3	$=B_{32}X_2+B_{31}X_1$ $+B_{3u}R_u$	US	.062	.062					
		UK	.015	.015					
		GR	.039	.039					
		IT	.031	.031					
		MX	.020	.020					
X_4	$=B_{43}X_3+B_{42}X_2$ $+B_{41}X_1+B_{4v}R_v$	US	.070	.063	.061				
		UK	.016	.016	.016				
		GR	.041	.043	.043				
		IT	.034	.035	.038				
		MX	.021	.021	.021				
X_5	$=B_{54}X_4+B_{53}X_3$ $+B_{52}X_2+B_{51}X_1$ $+B_{5w}R_w$	US	.069	—*	.071	.081			
		UK	.019	.016	.017	.021			
		GR	.045	—*	.049	.055			
		IT	—*	.038	.040	.039			
		MX	.024	—*	.022	.025			
X_6	$=B_{65}X_5+B_{64}X_4$ $+B_{63}X_3+B_{62}X_2$ $+B_{61}X_1+B_{6x}R$	US	.077	.063	.073	.084	.060		
		UK	—*	.016	.018	.019	.017		
		GR	.045	.044	.051	.060	.045		
		IT	.040	.037	.041	.052	.036		
		MX	.024	.023	.022	.029	.019		
X_7	$=B_{76}X_6+B_{75}X_5$ $+B_{74}X_4+B_{73}X_3$ $+B_{72}X_2+B_{71}X_1$ $+B_{7y}R_y$	US	.078	.064	.080	.088	.061	.090	
		UK	.019	.017	.019	.023	.017	.020	
		GR	.046	.046	.054	.062	.045	.057	
		IT	.041	.038	.043	.055	.038	.051	
		MX	.024	.023	.022	.032	.019	.027	
X_8	$=B_{87}X_7+B_{86}X_6$ $+B_{85}X_5+B_{84}X_4$ $+B_{83}X_3+B_{82}X_2$ $+B_{81}X_1+B_{8z}R_z$	US	.077	.065	.079	.092	.062	.100	.091
		UK	.019	.017	.019	.024	.017	.021	.023
		GR	.046	.046	.054	.068	.045	.063	.071
		IT	.041	.038	.043	.061	.037	.055	.060
		MX	.024	.024	.023	.033	.019	.028	.032

* E level too low to be entered in equations. No standard error computed.

APPENDIX II. COMPARISON OF DIRECT PATH LINKAGES IN SIX VARIABLE MODEL USING PARTICIPATION SCALE, TALKING POLITICS, AND CONTACTING LOCAL OFFICIALS AS DEPENDENT VARIABLES

Sample	Direct Path	Dependent Variable		
		Talking Politics	Contacting Local Officials	Participation Scale
US	Status→Dep Var	.119	.015	.089
	Involvement→	.022	.209	.297
	Information→	.017	.031	.055
	Efficacy→	.209	.186	.176
	Attentiveness→	.333	.156	.248
UK	Status→Dep Var	.000	−.051	−.007
	Involvement→	.136	.121	.303
	Information→	.121	.114	.143
	Efficacy→	.100	.092	.100
	Attentiveness→	.366	.169	.330
Germany	Status→Dep Var	.008	.023	−.058
	Involvement→	.157	.072	.299
	Information→	.018	−.048	.043
	Efficacy→	.146	.230	.244
	Attentiveness→	.440	.199	.304
Italy	Status→Dep Var	−.029	.176	−.045
	Involvement→	.102	.081	.246
	Information→	.000	−.100	−.045
	Efficacy→	.104	.225	.211
	Attentiveness→	.606	.037	.426
Mexico	Status→Dep Var	.077	−.009	−.011
	Involvement→	.070	.110	.358
	Information→	.107	.030	.145
	Efficacy→	.066	.137	.091
	Attentiveness→	.408	.109	.194

APPENDIX III. PATH COEFFICIENTS FOR TEN VARIABLE RECURSIVE MODEL OF LOCAL PARTICIPATION X_1 = SIZE OF COMMUNITY

Dependent Variable	Country	B_1	B_2	B_3	B_4	B_5	B_6	B_7	B_8	B_9
X_2	US	−.022								
	UK	.130								
Length of	GR	−.042								
Residence	IT	.031								
	MX	.007								
X_3	US	.145	−.116							
	UK	.056	−.266							
Social	GR	.165	−.115							
Status	IT	.167	−.088							
	MX	.119	−.019							
X_4	US	−.080	.110	.462						
	UK	.031	.039	.323						
Organization	GR	−.040	−.026	.217						
Involvement	IT	−.073	.060	.321						
	MX	−.007	.063	.229						
X_5	US	.008	−.024	.287	.154					
	UK	−.017	.047	.168	.184					
Local Citizen	GR	−.061	−.002	.177	.237					
Duty	IT	.017	−.011	.204	.300					
	MX	.008	.067	.177	.160					
X_6	US	−.022	−.022	.340	.142	.288				
	UK	−.021	−.063	.349	.171	.202				
Political	GR	.183	−.020	.237	.185	.254				
Information	IT	−.053	.007	.382	.231	.206				
	MX	.008	−.007	.380	.266	.180				
X_7	US	.068	.004	.043	.065	.121	.156			
	UK	−.023	.070	.104	.007	.149	.117			
Impact of	GR	−.007	.050	.036	.015	.050	.221			
Local Gov.	IT	−.115	−.021	.040	.014	.133	.273			
	MX	−.079	.129	.005	.011	.065	.078			
X_8	US	−.076	.063	.126	.150	.225	.160	.108		
	UK	−.022	.059	.018	.155	.139	.100	.164		
Political	GR	−.168	.119	.123	.132	.160	.159	.025		
Efficacy	IT	−.090	−.004	.179	.170	.170	.155	.094		
	MX	.038	.021	.125	.121	.051	.270	.080		
X_9	US	.081	.031	.084	.118	.060	.310	.106	.223	
	UK	.035	−.040	.123	.075	.145	.336	.006	.156	
Attentiveness	GR	.030	.067	.060	.139	.134	.356	.080	.161	
to Politics	IT	−.011	.023	.113	.136	.139	.389	.014	.147	
	MX	.046	−.017	.091	.182	.137	.262	.058	.209	
X_{10}	US	−.047	.030	.045	.192	−.058	.061	.033	.183	.174
	UK	−.056	.001	.054	.124	.020	.107	.080	.075	.171
Local Partici-	GR	−.095	−.021	.031	.069	.040	−.033	.126	.202	.158
pation	IT	−.031	−.021	.173	.075	.031	−.064	−.057	.195	.066
	MX	−.037	.020	−.003	.110	.001	.033	.132	.127	.097

APPENDIX IV. PROCEDURES USED IN COMPUTING THE TABLES (4, 5, 6) IN THE ANALYSIS OF SOCIAL STRUCTURE AND THE PARTICIPATION OF CLASS GROUPS

In our discussion of the representation of various class groups among the political participants in a society, we present three tables of simulated and empirical data. The following description of the computation of these tables should be compared to the analytic discussion in the text.

Table 4 and 5: Simulated Data. The following step-by-step analysis applies to the Columns in Table 5. For Table 4, use steps 1, 2, 4, 5, and 6 only.

(1) Columns I–III are the conditions stipulated for the society whose participation pattern is to be simulated.

(2) From these three conditions it is necessary to compute the size of the six class-organization groups in the society (i.e. proportion of low class-uninvolved, low class-involved, middle class-uninvolved, middle class-involved, upper class-uninvolved, upper class-involved.)

(3) If the correlation between status and organizational membership is .0, then the organizational memberships—whose absolute number in the society is given in Column II—are distributed evenly through the class groups. The size of the group is multiplied by .15, .60, or whatever the proportion of memberships in the system, and this gives the size of the involved members' proportion in each class group. The uninvolved are the difference between the involved proportion and the original size of the group.

(4) If the correlation is .50, then our estimates (See footnote 14) suggest that the difference between the number of organization members in the upper class and the number in the lower class should be about 35%. The following equation can be solved for the percentage of organization members in each class group, given this range of difference and the stipulated level of organizational membership in the society:

$Ax+By+Cz=100\ M$

$z=x+35$, $y=x+17$; as simultaneous equations

where:

A is the proportion of lower class persons in the society

B is the proportion of middle class persons in the society

C is the proportion of upper class persons in the society

M is the proportion of organization members given

x is the proportion of organization members in the lower class; y is the proportion of organization members in the middle class; z is the proportion of organization members in the upper class.

A, B, C, and M are stipulated by the conditions. Other simulations for alternative correlations between status and organization could, of course, be carried out using estimates of the type noted in footnote 22—which determine the relationships between x, y, and z in the above equations.

(5) The proportion of each sub-group in the society is multiplied by the participation rates for that sub-group, shown in the text and originally estimated from the average cross-national correlation coefficients.

(6) The number of participants in each sub-group is summed to get the participation rate for the society. (See Table 4.)

(7) The numbers in the sub-group are compared to the total participation rate to show the proportion of participants made up of lower, middle, and upper class individuals. (See Column IV in Table 5).

(8) The difference between lower class proportion in the society and lower class proportion of political participants (from columns I and IV) is divided by the size of the class group (from Column I) to get the index of under or overrepresentation of the group. (See Column V in Table 5.)

Table 6: Empirical Data from Five Nations.

For Table 6 we use, of course, empirical data from the surveys in the five nations to determine the size and participation within the class-organization groups. As noted in footnotes in the text, similar cutting points on the various scales were used for all five nations, with one exception. The group sizes and participation rates for the United States are shown below as an example.

Size of Class-Organization Groups in United States		*Participation in Each Group*
Low class, uninvolved	20%	14.5%
Low class, involved	11%	43.5%
Middle Class, Uninvolved	21%	32.5%
Middle Class, involved	24%	60.0%
Upper Class, uninvolved	5%	40.8%
Upper Class, involved	20%	75.5%
Total	101%	

Using this data, the calculations can be made as above, steps 5–8, or the figures in the Table can be calculated from the data more directly.

URBANIZATION AS AN AGENT IN LATIN AMERICAN POLITICAL INSTABILITY: THE CASE OF MEXICO*

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I. INTRODUCTION

A large descriptive and conjectural literature has evolved during the past decade suggesting that rapid urbanization—conceived primarily as a massive demographic shift from country to city—constitutes an important agent of political instability in much of Latin America, favoring the growth of alienation and radicalism.[1] This effect is frequently attributed to the frustration of migrant expectations for economic improvement and social mobility in the large cities, or to the processes of personal and social disorganization allegedly inherent in the migration experience. Although field researchers have long suspected the inadequacy of such theory, derived largely from the urbanization experiences of advanced Western nations, for explaining political instability in Latin America, there have been few systematic attempts at testing the various propositions and unconfirmed generalizations advanced by the "urban instability-crisis-and-chaos" theorists. This paper attempts to explore, if not to test, some of the empirical implications of the general theoretical conceptions of the urbanizing process and its socio-political consequences as developed in recent social science literature on Latin America. Mexico is selected for analysis both by virtue of its extremely rapid rate of rural-urban migration in recent years and because of the opportunities afforded by the availability for Mexico of detailed survey data from a number of independent sources to examine systematically a wide range of theoretically relevant variables and relationships posited in the urbanization literature. While in certain areas (as indicated below) the particular nature of the Mexican political system and developmental pattern may render these findings imperfect as predictors for other parts of Latin America, the analysis presented here should serve to identify the major inadequacies of existing theory and illustrate the need for new conceptual models which can encompass both stabilizing and destabilizing concomitants of rapid urban concentration in a developing nation.

* The author is indebted to Richard R. Fagen, Robert A. Packenham, and Barry S. Rundquist, all of Stanford University, for comments on an earlier draft of this paper.

[1] Overviews of a large part of this literature are provided in Richard M. Morse, "Recent Research on Latin American Urbanization," *Latin American Research Review,* 1 (Fall, 1965), 35–74; William Mangin, "Latin American Squatter Settlements: A Problem and a Solution," *Latin American Research Review,* 2 (Summer, 1967), 65–98; and Wayne A. Cornelius, Jr., "The Political Sociology of Cityward Migration in Latin America: Toward Empirical Theory," in Francine F. Rabinovitz and Felicity M. Trueblood (eds.), *Latin American Urban Annual* (Vol. I; Beverly Hills, Calif.: Sage Publications, forthcoming). Most discussions of Latin American urban affairs by social scientists and historians reflect a substantial intellectual debt to "mass society" theorists such as William Kornhauser and to the early "Chicago School" of urban sociology, especially the ideas of Robert E. Park and Louis Wirth. See W. Kornhauser, *The Politics of Mass Society* (New York: Free Press, 1959); R. E. Park, "Human Migration and the Marginal Man," *American Journal of Sociology,* 3 (May, 1928), 881–893; and L. Wirth, "Urbanism as a Way of Life," in A. J. Reiss (ed.), *Louis Wirth on Cities and Social Life* (Chicago: University of Chicago Press, 1964). For detailed analyses of the Chicago School formulations see Robert E. L. Faris, *Chicago Sociology, 1920–1932* (San Francisco: Chandler, 1967); and R. N. Morse, *Urban Sociology* (New York: Praeger, 1968), pp. 15–38, 160–72.

II. THEORY

For purposes of analysis it is possible to reduce current urbanization theory to three basic themes or clusters of interrelated propositions, which may be stated and schematized as follows:

(1) *Urbanization breeds economic frustration among the migrant population,* which aspires high but fails to participate in the material rewards of urban society. This "frustration gap" between high expectations and the ability to fulfill them results both from the inadequate premigration preparation of the migrants and the inability of urban government and private enterprise to accommodate them. Migrants are deficient in both the skills to earn a living in their new environment and the education needed to learn the

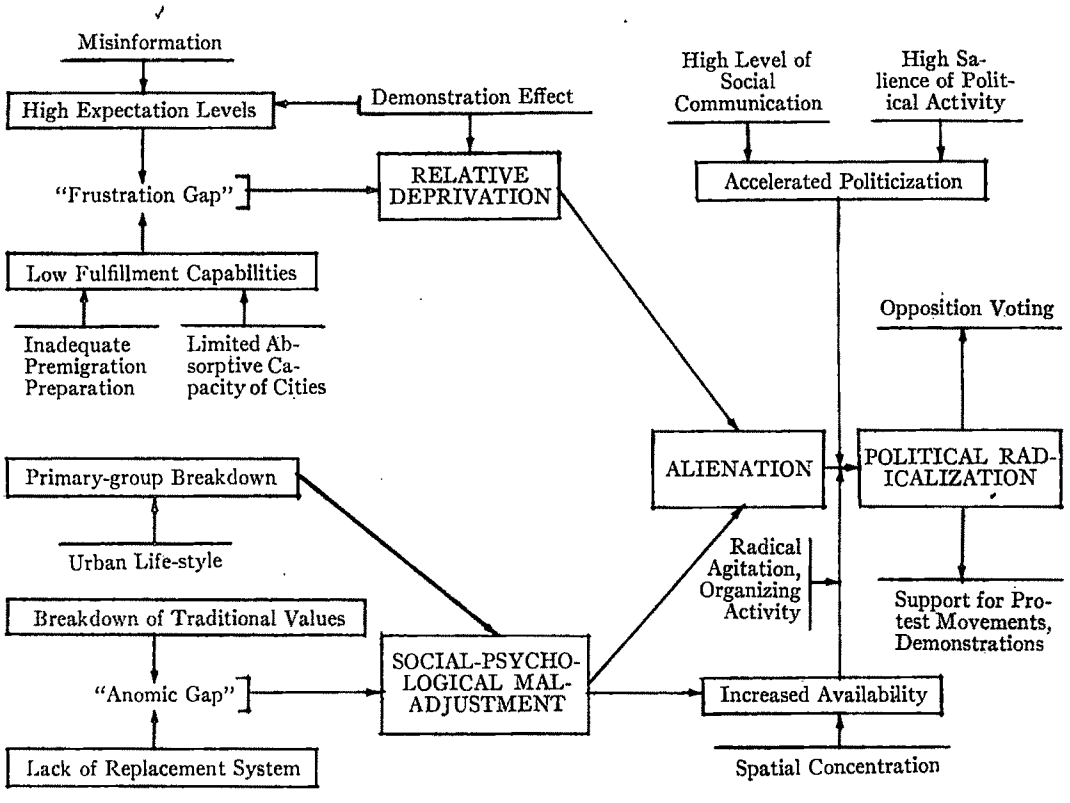


FIGURE 1. Migrant assimilation in Latin American urban environments: theory

skills; upward mobility is sharply restricted.[2] Moreover, the accelerated population input into cities makes it impossible for the public and private sectors to provide adequate housing, education, and other essential urban services. Thus for the majority of migrants, the city brings thwarted mobility expectations and objective living conditions often worse than those endured in the countryside. And the "demonstration effect" of concentrated buying power and highly conspicious forms of consumption in urban areas makes the migrants increasingly conscious of their marginal role in society. Cognitive mechanisms of relative deprivation are set into operation.[3] Frustrated material expectations and feelings of relative deprivation lead to alienation and increase the mi-

[2] Cf. Lawrence R. Alschuler, *Political Participation and Urbanization in Mexico* (Ph.D. dissertation, Northwestern University, 1967; Ann Arbor, Mich.: University Microfilms, 1968, No. 68-3156), p. 97; Leonard Cardenas, Jr., "Trends and Problems of Urbanization in the U.S.-Mexico Border Area" (unpublished paper presented at the Conference on Urbanization in the U.S.-Mexico Border Region, University of Texas at El Paso, June, 1968), p. 4; and R. H. Fitzgibbon, "Political Implications of Population Growth in Latin America," *The Sociological Review,* 11 (February, 1967), p. 41.

[3] The relative deprivation concept is relied upon frequently in research on political alienation in Latin American countries. It is argued that even though many migrants to the cities arrive with a relatively low level of expectations and perceive a relative increase in environmental satisfaction as they compare their present status with their rural past, these early feelings of relative reward give way—perhaps after ten or fifteen years of urban residence—to relative deprivation. Migrant consciousness of economic marginality in the city supposedly increases over time. Such arguments are incorporated into the conceptual framework developed in Gláucio A.D. Soares, "Desarrollo económico y radicalismo político," in Joseph A. Kahl (ed.), *La industrialización en América Latina* (Mexico City and Buenos Aires: Fondo de Cultura Económica, 1965), pp. 516-559; Soares, "The Politics of Uneven Development: The Case of Brazil," in S. M. Lipset and Stein Rokkan (ed.), *Party Systems and Voter Alignments: Cross-Na-*

grant's propensity to engage in radical political activity.[4]

(2) *Migrants experience major difficulties in adjusting socially and psychologically to the urban environment.* Such difficulties originate in culture conflicts, the disorganization of previous living habits and customs, personal "identity crises," and primary-group breakdown.[5] Large nuclear families and kinship ties tend to disintegrate in urban society with its new standards, economic demands, and mobility patterns. The traditional social controls on deviant behavior may be weakened.[6] Ruralistic norms and modes of social interaction are found to be either irrelevant or harmful under urban conditions. Orthodox religious values are undermined, accompanied by a sense of normlessness, insecurity, and felt need for social reintegration by entry into new protective groupings. There results an "anomic gap,"[7] a time lag between the abandonment of the traditional value system and the acceptance of a replacement system. During this period the migrant is particularly vulnerable to recruitment into extremist political movements which seek to satisfy his psychological need for reintegration. Personal anomia and feelings of social isolation may persist long after the migrant is assimilated economically into his urban surroundings, threatening a chronic susceptibility to manipulation by demagogic political leaders.[8]

(3) *Urbanization, increased awareness of government and politics, and mobilization of radical opposition force go hand in hand.* Exposure to the urban environment with its higher level of social communication, intense competition among diverse social and economic interests, and salience of organized political activity accelerates the politicization of migrants and creates conditions for mass involvement in direct political action. Migrant frustrations and anxieties stemming from problems of assimilation into the urban environment are translated into support for opposition political parties and aggressive protest movements against established urban governments.[9] We may expect the anti-system, protest vote in a given area or locality to increase in response to high rates of in-migration.[10] Large influxes of migrants tend

tional Perspectives (New York: Free Press, 1967), at pp. 488–489; and G. Soares and R. L. Hamblin, "Socio-economic Variables and Voting for the Radical Left: Chile, 1952," this REVIEW, 61 (December, 1967), pp. 1055–1056. For other discussions and applications of the relative deprivation concept in recent studies of urban disorder and civil strife, see Ted Gurr, "Urban Disorder: Perspectives from the Comparative Study of Civil Strife," *American Behavioral Scientist,* 11 (March-April, 1968), 50–55; Gurr, "A Causal Model of Civil Strife: A Comparative Analysis Using New Indices," this REVIEW, 62 (December, 1968), 1104–1124; and Don R. Bowen, *et al.,* "Deprivation, Mobility, and Orientation Toward Protest of the Urban Poor," *American Behavioral Scientist,* 11 (March-April, 1968), 20–24.

[4] Cf. Margot Romano Yalour de Tobar and Edith Soubie, "Marginalidad y alienación en la clase obrera," *Cuadernos del Centro de Estudios Urbanos y Regionales* (Argentina), No. 9 (1967), pp. 129–132.

[5] Cf. Héctor Martínez, "Tensión y conflicto en el proceso de formación de la nacionalidad peruana," *Aportes* (France), 7 (January, 1968), p. 62; and Martin C. Needler, *Latin American Politics in Perspective* (Rev. ed.; Princeton, N.J.: Van Nostrand, 1968), pp. 63–64.

[6] Cf. Martin C. Needler, "Political Implications of Urbanization in Mexico" (unpublished paper presented at the Conference in the U.S.-Mexico Border Region, University of Texas at El Paso, June, 1968), p. 15.

[7] The term is from Soares and Hamblin, *op. cit.,* p. 1054. Cf. J. Medina Echavarria and Philip M. Hauser, "Rapporteurs' Report," in P. Hauser (ed.), *Urbanization in Latin America* (New York: International Documents Service, 1961), p. 54.

[8] Cf. Mauricio Gómez Mayorga, *et al., La superurbanización caótica* (Mexico, D. F.: Prensa Universitaria, 1963), p. 4; Gerardo Andújar, "Migración urbano-rural y autoritarismo político," *Revista Paraguaya de Sociología* (Paraguay), No. 6 (1966); and Jorge Giusti, "Rasgos organizativos en el problador marginal urbano latino-americano," *Revista Mexicana de Sociología* (Mexico), 30 (January-March, 1968), pp. 73–75.

[9] Cf. Kenneth F. Johnson, *Urbanization and Political Change in Latin America* (Ph.D. dissertation, University of California at Los Angeles, 1963; Ann Arbor, Mich.: University Microfilms, 1964, No. 64–2244), pp. 138, 339–340; K. F. Johnson, "Causal Factors in Latin American Political Instability," *Western Political Quarterly,* 17 (September, 1964), p. 442; Ben G. Burnett and K. F. Johnson, *Political Forces in Latin America: Dimensions of the Quest for Stability* (Belmont, Calif.: Wadsworth, 1968), p. 519; and Dankwart Rustow, *A World of Nations: Problems of Political Modernization* (Washington, D.C.: Brookings Institution, 1967), p. 245.

[10] Cf. K. F. Johnson, *The Guatemalan Presidential Election of March 6, 1966* (Washington, D.C.: Institute for the Comparative Study of Political

> to exacerbate the destabilizing forces already at work in major cities, inviting extreme responses from either governments or populations or both. Radical agitation among the increasingly politicized urban masses, made less patient by the visible abundance of the city and more powerful by their concentration, becomes one of the greatest long-term threats to orderly development within a non-authoritian framework.[11]

It should be emphasized that the above propositions are not deduced from an integrated, formal theory of urban migration and political behavior in Latin America which is elaborated or adhered to *in toto* by the writers cited in connection with them. They are, however, reflective of a general theoretical orientation persisting strongly in discussions of the socio-political implications of rapid urbanization in the region. This orientation places heavy stress on the personally disorienting, socially disintegrative, and politically disruptive consequences of the urbanizing process.

III. METHODOLOGY

We have little direct information on attitudinal and behavioral consequences of Latin American urbanization which would enable rigorous testing of the foregoing set of propositions on a cross-national basis. However, by secondary analysis of available data from field surveys, the national census, and national election returns, we can at least approximate this goal within the context of an individual country. In the analysis of the Mexican case which follows, I shall draw primarily upon survey data on urban political culture in Mexico originally gathered for Gabriel A. Almond and Sidney Verba's pioneering cross-national study, *The Civic Culture*.[12] The relatively small number of retrospective questions in the main survey instrument used in this study, as well as the lack of a control group of non-migrant Mexicans still residing in rural areas to which the urban sample might be compared, impose certain limitations on the use of these data for an investigation of the urbanization process. Nevertheless, they still represent the most important body of systematic data on the urban Mexican's cognition and evaluation of, and sense of participation in, politics and government.[13] For purposes of the present analysis, a migrant sample consisting of 467 adult respondents born in rural villages or localities of less than 5,000 population, and who had migrated to urban centers of 10,000 or over by the time of the survey, was isolated from the universe of 1297 Mexican cases analyzed in *The Civic Culture*.[14] (Some 68 respondents interviewed in the Kahl survey in Mexico were also found to conform to our definition of migrant

Systems, 1967), p. 18; Soares and Hamblin, *op. cit.*, p. 1062; and Soares in Kahl, *op. cit.*, pp. 553-555.

[11] Cf. Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, Conn.: Yale University Press, 1968), pp. 281–283; Rex D. Hopper, "Research on Latin America in Sociology," in Charles Wagley (ed.), *Social Science Research on Latin America* (New York: Columbia University Press, 1964), p. 265; Paul Kelso, "A Developing Democracy," in Russel C. Ewing (ed.), *Six Faces of Mexico* (Tucson, Ariz.: University of Arizona Press, 1966), p. 164; Alan Angell, "Party Systems in Latin America, *Political Quarterly*, 37 (July-September, 1966), pp. 313, 316; and Philip L. Wagner, "Political Implications of Rapid Urbanization in Caribbean Countries" (unpublished paper presented at the 57th Annual Meeting of the American Association of Geographers, Miami Beach, Fla., 1962).

[12] Princeton, N.J.: Princeton University Press, 1963. See also the detailed interpretation of the Mexican survey results provided in Robert E. Scott, "Mexico: The Established Revolution," in Lucian W. Pye and Sidney Verba (eds.), *Political Culture and Political Development* (Princeton: Princeton University Press, 1966).

[13] Another useful source of survey data on attitudinal consequences of Mexican urban development, though lacking a specifically political focus, is Joseph A. Kahl's study, *The Measurement of Modernism: A Study of Values in Brazil and Mexico* (Austin, Texas: University of Texas Press, 1968). Occasional reference will be made in this paper to the results of a secondary analysis performed on this data, obtained through the International Data Library and Reference Service, University of California, Berkeley.

[14] The actual total of interviews was 1,008, which was upweighted to 1,297 to compensate for undersampling of Mexico City, done in order to allow for greater interior city coverage. (A complete description of the Mexican sample is provided in *The Civic Culture*, Appendix A, pp. 514–516.) My use of a place-of-birth criterion in defining the migrant population has certain obvious limitations. For example, we have no indication in a given case of the stages of the migratory process or of the respondent's stage in his life cycle when he arrived in the big city. Thus between the time of departure from the community of origin and the time of arrival in the city where interviewed, a migrant may have lived in several different communities of varying size, perhaps arriving in the interview city with a relatively high degree of anticipatory socialization into urban living pat-

status, and will hereafter be referred to as the *Measurement of Modernism* sample.[15]) The modal respondent in our *Civic Culture* sample is over 35 years of age and has resided in a city of over 100,000 population for ten or more years. Females are overrepresented in our sample by a factor of 1.2 (the ratio of percent female migrants to percent females in the 1960 Mexican census). This figure is roughly comparable to the slight degree of female predominance in lifetime migratory streams in the Mexican nation as a whole in recent decades, as well as in migration patterns of other Latin American countries. Such findings reflect the continuing attractiveness of urban opportunities for female employment in commerce, industry, and services.[16]

Before turning to an examination of the social and economic characteristics, attitudes, and behavior patterns exhibited by members of our migrant sample, it may prove useful to orient the discussion by providing a brief overview of the process of urban concentration in Mexico in recent years. Some knowledge of the nature, rate, and magnitude of the process is important to an assessment of the contextual basis for political radicalism and instability in the nation's major urban centers.

IV. DEMOGRAPHY

There is no lack of evidence to support the contention that Mexico is rapidly becoming one of the most highly urbanized nations in the Latin American region. According to recent estimates by the Inter-American Development Bank, Mexico's current rate of urban growth is among the three highest in the region.[17] Although both urban and rural populations have increased sharply in Mexico, between 1950 and 1960 the rural population expanded at a rate of 1.6% per year while the urban population grew at a rate of 6.1% per year.[18] The proportion of Mexico's population in cities of 10,000 or over rose from 12% in 1900 to 38% in 1960; and it

terns (Cf. Anthony and Elizabeth Leeds, "Brazil and the Myth of Urban Rurality: Urban Experience, Work, and Values in 'Squatments' of Rio de Janeiro and Lima" [unpublished paper presented at the Conference on Urbanization and Work in Modernizing Societies, St. Thomas, Virgin Islands, November, 1967], pp. 17-18; and Arthur J. Field (ed.), *Urbanization and Work in Modernizing Societies* [Detroit, Mich.: Glengary Press, 1967], pp. 72–73). Analysis of survey data gathered in specific Mexican metropolitan areas does indicate, however, that a majority of migrants to such cities grew up in nearby rural areas and came directly to the big city with no extended intermediate steps. It has been suggested that for Mexico the model of "staged" migration might be an adequate description of the earlier moves from rural villages to large urban centers, but that once communication links are formed by the first movers from village to metropolis, succeeding waves of migrants do not necessarily have to follow in the same steps but may make a single move directly to the large urban center (see Jorge Alberto Harth Deneke, "The Colonias Proletarias of Mexico City: Low Income Settlements at the Urban Fringe," [unpublished M.A. thesis, M.I.T., 1966], p. 92). Support for this hypothesis comes from a number of anthropological and demographic studies of cityward migration in Mexico, including Oscar Lewis, "Urbanización sin desorganización," *América Indígena* (Mexico), 17 (July, 1957), p. 238; Douglas S. Butterworth, "A Study of the Urbanization Process Among Mixtec Migrants from Tilaltongo in Mexico City," *América Indígena,* 22 (July, 1962), p. 261; and Jorge Balán, *et al.* (eds.), *Movilidad social, migración, y fecundidad en Monterrey metropolitano* (Monterrey, Mexico: Centro de Investigaciones Económicas, Universidad de Nuevo León; and Population Research Center, University of Texas, Austin, 1967), pp. 73–75. It is therefore reasonable to assume that the primary place of urban socialization for most members of our migrant sample was the city where they were interviewed. The "missing" data on prior urban experience and its consequences for the assimilation process in large cities may well be non-existent.

[15] Note that subsequent references to "migrants" interviewed in the Kahl survey pertain only to those respondents meeting our criteria for definition of migrant status and not those employed by Kahl in his original analysis of the data (see *The Measurement of Modernism, op. cit.,* p. 26).

[16] See Zulma Carmen Camisa, "Effects of Migration on the Growth and Structure of Population in the Cities of Latin America," in United Nations, Department of Economic and Social Affairs, *Proceedings of the World Population Conference, 1965* (New York, 1967) (E/CONF.41/5), Vol. IV, pp. 408–411.

[17] See the data reproduced in Irving L. Horowitz, "Electoral Politics, Urbanization, and Social Development in Latin America," *Urban Affairs Quarterly,* 2 (March, 1967), Table 2, p. 10.

[18] Raúl Benítez Zenteno, "La población rural y urbana en México," *Revista Mexicana de Sociología,* 24 (September, 1962), 689–703. The best general survey of recent urban growth in Mexico is provided in Luis Unikel, "El proceso de urbanización en México: Distribución y crecimiento de la población urbana," *Demografía y Economía* (Mexico), 2 (1968) 139–283. Official Mexican use of the

is estimated that by 1980, nearly 70% of the population will be classified as urban.[19] Mexico's high rate of natural population growth (about 3.8% per annum in 1967 and 1968) has, of course, been an important factor in urban expansion. Of far greater importance, however, is the movement of population from the countryside into the cities, which reached torrential proportions in the 1950-1960 period, when over 2.4 million persons participated in the exodus from rural life.[20] Many left in search of greater occupational and educational opportunities in the cities; many more because of intolerable living conditions and population pressure on depleted rural land.[21] It is not surprising to find that most inter-state migration has originated in the six centrally-located states comprising the Mexican Bajío, a semi-arid agricultural region where population pressure on the land has been most severe.[22] Some 52% of the respondents in our migrant sample drawn from the *Civic Culture* survey were born in the Bajío region.

Long-distance migration has been concentrated overwhelmingly in a few large streams running from the centrally-located states to the Mexico City (Federal District) metropolitan area and the North Pacific (Baja California) region.[23] During the 1950-1960 period the population of the Federal District grew to over five million inhabitants; and growing at an annual rate of 7.2%, its population is expected to top the seven million mark by 1970.[24] The impact of migration on the capital's expansion can be gauged from the fact that over 37% of the people in the District in 1960 had migrated from elsewhere in the nation since 1950.[25] This influx contributed substantially to the truly spectacular expansion of the *"colonia proletaria"* zones, located for the most part on the periphery of Mexico City proper and extending beyond its legal limits. During the past two decades these marginal settlements expanded at an average rate of 46% per annum, while the core districts of the city grew at an annual rate of 2%. In 1966 it was estimated that nearly two million people—over one third of the population of Mexico City—made their homes in the *colonias proletarias*.[26]

V. ECONOMIC ASSIMILATION

There is some evidence that material deprivation in Mexican urban areas is not as serious as might be expected, given the massive influx of new residents from the countryside. Between 1940 and 1960, for example, the percentage of Federal District residents without access to public sanitation increased by only 18.9%, while the District's population grew by 177.1%.[27]

figure 2,500 as the breaking point between urban and rural is considered to be highly unrealistic by most students of Mexican demography. The cutting point was adopted by Mexico from U.S. census practice without due regard for differing conditions in the smaller and less developed country. Most Mexican localities of less than 10,000 people are essentially rural communities with few commercial or other urban functions and services. Thus for purposes of this paper, the "urban" population will be conceived as including all those residents of localities of 10,000 or more population.

[19] Nacional Financiera, *50 años de revolución en cifras* (Mexico, D. F., 1963), p. 40; and Raúl Benítez Zenteno and Gustavo Cabrera Acevedo, *Proyecciones de la población de México, 1960-1980* (Mexico, D. F.: Banco de México, S. A.; Investigaciones Industriales, Oficina de Recursos Humanos, 1966), p. 58.

[20] Gustavo Cabrera, "La migración interna en México, 1950-1960," *Demografía y Economía*, 1 (1967), p. 357.

[21] For data-based analyses of motivations for rural-urban migration in recent Mexican experience, Balán, *et al., op. cit.*, pp. 107-159; Butterworth, *op. cit;* and Enrique Valencia, *La Merced: Estudio ecológico y social de una zona de la ciudad de México* (Mexico, D. F.: Instituto Nacional de Antropología e Historia, 1965; Serie Investigaciones No. 11), pp. 243-258

[22] Cf. Julio César Olivé Negrete and Beatriz Barba de Piña Chán, "Estudio de las clases sociales en la ciudad de México: experiencias con un grupo obrero," *Anales del Instituto Nacional de Antropología e Historia* (Mexico), 14 (1962), pp. 237-238.

[23] See Cabrera, *op. cit.*, pp. 312-60; and Robert P. Stevens, "Algunos aspectos de la migración interna y urbanización en México, 1950-1960," *La geografía y los problemas de población* (Unión Geográfica Internacional, Conferencia Regional Latinoamericana), 1 (1966), 65-72.

[24] G. Luna, "Megalopolis Trends in Mexico," *Ekistics* (Greece), 24 (July, 1967), 15-20.

[25] Oliver Oldman, *et al., Financing Urban Development in Mexico City* (Cambridge, Mass.: Harvard University Press, 1967), pp. 7 ff.

[26] Deneke, *op. cit.*, p. 32.

[27] James W. Wilkie, *The Mexican Revolution: Federal Expenditure and Social Change since 1910* (Berkeley: University of California Press, 1967), p. 231. There are some fragmentary data from Mexican government surveys indicating that service deprivation is relatively more acute in the capital's peripheral *colonia proletaria* zones (see Deneke, "The *Colonias Proletarias* of Mexico City," *op. cit.*, pp. 73-74), where over 70% of the adult residents are first-generation migrants. However, survey data gathered by academic researchers indicate that a majority of *colonia* dwellers do

Thus for perhaps a majority of migrants to the capital, the move from country to city resulted in an improvement in the level of services and other material amenities to which they had access. This contention is supported by the results of an analysis of a sample of the 1960 Mexican census in the Federal District, which showed that, contrary to expectation, migrant residents do not differ significantly from the urban-born population with respect to standard of living indicators taken from the census data (e.g., availability of running water in households, sewage facilities, ownership of radios and household appliances).[28] Widespread service deprivation and deteriorating overall conditions of life for the in-migrant population are more often assumed than demonstrated. Confounding evidence takes on added significance in light of an analysis of aggregate data on urbanization and political development in the Latin American region, which found that "while ideological and psychological factors play a role in assuring or undermining stability, they are keyed to the real level of material services [provided] to urban residents."[29]

The migrant's decision to remain in the urban center to which he has moved must be based on a modicum of perceived satisfaction with the urban environment. Whether or not this measure of satisfaction derives from the migrant's subjective, retrospective comparison of his present situation with his rural past, or from an actual improvement in objective conditions of living which he has experienced in the post-migration period, his intention to remain in the city serves as one indicator of success in meeting the basic challenges of economic assimilation. The data from the *Civic Culture* survey suggest a far more widespread satisfaction with his new environment than the migrant is normally credited with. Among those respondents in our migrant sample with less than two years of residence in the city where interviewed, 41% express a definite intention to remain there; and after four years of urban residence this figure rises to over 80%. A relatively low rate of remigration (i.e., return to the countryside as a negative response to the urban environment) is suggested by these data.[30]

The quality of a migrant's economic assimilation is obviously a factor of critical importance in determining whether he will later develop a "frustration gap" leading to alienation or political radicalism. It is reasonable to assume that successful assimilation depends in large part upon the migrant's capacity to display salable productive skills. This in turn depends upon his premigration preparation: his basic education and work experience. Among the newly-arrived migrants in our *Civic Culture* sample (those with under two years of urban residence), 70% had attained some level of primary education (up to six years of schooling); and among the migrants as a whole, 69% had achieved a primary education or higher. It would thus appear that the bulk of migrants arriving in Mexican urban centers possess at least the basic educational equipment which would enable them to learn productive skills in the post-migration period. The data contained in Table 1 provide some indication of their success in acquiring such skills over time in the city. Of importance are the relatively large proportions of skilled workers, artisans, office workers, and small

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have access to basic urban services (see Olivé Negrete and Barba de Piña Chán, *Anales*, 14 (1962), *op. cit.*, p. 232).

[28] See Harley L. Browning, "Urbanization and Modernization in Latin America: The Demographic Perspective," in Glenn H. Beyer (ed.), *The Urban Explosion in Latin America* (Ithaca, N.Y.: Cornell University Press, 1967), p. 91.

[29] Francine F. Rabinovitz, "Urban Development and Political Development in Latin America," in Robert T. Daland (ed.), *Comparative Urban Research: The Administration and Politics of Cities* (Beverly Hills, Calif.: Sage Publications, forthcoming, 1969). Respondents in Kahl's *Measurement of Modernism* survey were asked to comment on the adequacy of social welfare services available to them in the Federal District. As the following data indicate, migrant evaluations were significantly more positive than those of metropolitans (the latter defined as respondents living in Mexico City who had been living in large cities at the age of 10):

	Migrants (N = 68)	*Metropolitans* (N = 247)
Very good	31%	22%
Fair	52	47
Poor	18	32

(Migrant/metropolitan differences are significant here at the .05 level. Controlling for age does not alter the observed relationship between migrant status and positive evaluation of services.)

[30] Migrants to the capital city in the *Measurement of Modernism* survey were asked whether the move had made their lives better or worse. Some 71% of these respondents reported an improvement in living conditions as a result of cityward migration, and 76% stated that they would not consider remigration. Among migrants to the Monterrey metropolitan area surveyed in the Monterrey Mobility Study, 92% reported satisfaction with their decision to migrate, citing improved living conditions as the most important source of satisfaction: Balán, *et al.*, *op. cit.*, pp. 124–28.

TABLE 1. COMPARATIVE OCCUPATIONAL DISTRIBUTIONS OF MIGRANTS, URBAN-BORN, AND MEXICAN WORK FORCE

Occupation	Economically Active Migrants by Length of Urban Residence (in years)					All Economically Active Migrants	Urban-born[a]	Mex. Work Force[b]	Ratio: % Migrants to % Work Force
	under 2	2–4	5–9	10–19	20, over				
Unskilled worker[c]	32%	17%	25%	23%	16%	21%	11%	15%	1.4
Skilled worker, Artisan	21	46	31	38	38	36	44	11	3.3
Office worker	21	17	20	17	9	15	21	6	2.5
Agricultural	11	10	2	4	10	7	4	54	.13
Small Tradesman	11	5	17	14	22	16	12	9	1.8
Professional, Technician	5	5	5	3	2	3	6	4	.75
High Management, Business	0	0	0	2	3	2	2	1	2.0
Total[d]	101%	100%	100%	101%	100%	100%	100%	100%	
(N)	(38)	(41)	(64)	(114)	(125)	(380)	(710)		
	$\chi^2 = 44.00$, $p < .05$					$\chi^2 = 27.74$, $p < .001$			

[a] This category, as employed in this and subsequent tables, includes those respondents in the *Civic Culture* sample for Mexico born in localities of over 10,000 population.

[b] Data is for the total economically active population in 1960. Computed from International Labour Office, *Yearbook of Labor Statistics* (Geneva, 1966), pp. 168–69. Migrant and native-born distributions include data on husband's occupation reported by economically inactive, female respondents.

[c] Mexican work force percentages for skilled and unskilled workers are estimated on the basis of data included in official census categories suggesting skill level.

[d] Totals in this and subsequent tables may not add to 100% due to rounding.

tradesmen among those migrants with ten or more years of urban residence. Moreover, there is some evidence here to challenge the common assumption that the vast majority of migrants arrive in the city bereft of any useful skills and destined only to swell the ranks of the unemployed. Indeed, only 32% of the recently-arrived (under two years' urban residence), economically-active migrants in our *Civic Culture* sample may be classified as unskilled workers. When we compare the migrants as a group with the urban-born, we find about twice as many migrants as urban-born classed as unskilled workers; but differences in the distributions of cases among the remaining occupational categories are not large.[31] Comparison of the occupational distribution of all economically-active migrants with the distribution for the entire Mexican work force shows skilled workers and artisans to be over-represented among the migrants by a factor of 3.3; white-collar (office) workers by a factor of 2.5; and higher managerial personnel by a factor of 2.0. The movement of migrants into these higher status occupational groups, as well as the sharp decline in migrants classed as unskilled workers as length of urban residence increases, suggest that whatever the handicaps from which migrants suffer upon arrival in the city they are not so severe as to constitute a major obstacle to occupational mobility.[32] Kahl's survey data also indicate that urban migrants in Mexico show a tendency toward slightly more upward mobility than the native-born populations of the cities to which they move, although there is substantial varia-

[31] For comparable survey findings from specific metropolitan areas, see Harley L. Browning and Waltraut Feindt, "Diferencias entre la población nativa y la migrante en Monterrey," *Demografía y Economía*, 2 (1968), 183–204; and Donald R. Case, "Differentiation among Manual Workers in Mexico City" (unpublished M.A. thesis, University of Texas at Austin, 1967), pp. 71–73, 96–106.

[32] The same conclusion is suggested by the data from the Monterrey Mobility Study. See especially Jorge Balán, "Are Farmers' Sons Handicapped in the City?" *Rural Sociology*, 33 (June, 1968), 160–74.

tion in mobility rates among occupational status categories.[33]

The *Civic Culture* data do not permit examination of the migrants' urban employment history or rate of job turnover. However, Table 2 shows the level of unemployment among employable migrants in our sample at the time of the survey. We find that the proportions of unemployed migrants and urban-born respondents are virtually the same. Although the level of unemployment among migrants is slightly higher than that among the total population of employable Mexicans, the latter figure conceals severe underemployment in rural areas, which, as we have noted, constitutes one of the major "push" factors in rural-urban migration.[34] Although no accurate underemployment data are available for urban areas, we can expect the problem there to be alleviated by the expansive capacity of the commercial and industrial sectors of the economy. In the Mexican case, absorption of increasingly large numbers of migrants into the major metropolitan areas has occurred under conditions of rapid and sustained industrial growth and relatively high structural mobility rates.[35] While apparently failing to contribute to higher real income levels for working-class Mexicans, this pattern of economic expansion is not without its implications for political stability and the moderation of demands upon governmental structures.[36]

TABLE 2. COMPARATIVE UNEMPLOYMENT LEVELS AMONG EMPLOYABLE MIGRANTS, URBAN-BORN, AND ALL EMPLOYABLE MEXICANS

Labor Force Participation Status	Employable Migrants	Employable Urban-born	All Employable Mexicans, 1960[a]
Unemployed	1.8%	1.3%	1.6%
Employed	98.2	98.7	98.4
Total	100.0%	100.0%	100.0%
(N)	(218)	(380)	
	$\chi^2 = 0.02$	(N.S.)[b]	

[a] Percentages computed from México, Secretaría de Industria y Comercio, Dirección General de Estadística, *VIII Censo general de población, 1960: Resumen general* (Mexico, D.F., 1962), Table 22, p. 362.1.

[b] In this and subsequent tables, the designation (N.S.) indicates that differences between migrant and urban-born respondents are not, by chi-square test, significant at or beyond the .05 level of significance.

[33] See Joseph A. Kahl, "Social Stratification and Values in Metropoli and Provinces: Brazil and Mexico," *América Latina* (Brazil), 8 (January-March, 1965), p. 31; and Kahl, *Measurement of Modernism, op. cit.*, pp. 175, 182–183.

[34] Mexican government statistics for 1959 indicate that over half of the nation's five million agricultural workers were employed for only 145 days or less during the year (cited in U.S. Department of Labor, Bureau of Labor Statistics, *Labor in Mexico* [Washington, D.C., 1963; BLS Report No. 251], p. 40).

Another objective measure of the quality of economic assimilation among urban migrants is family income level. The distribution of incomes for our *Civic Culture* sample of migrant respondents presented in Table 3 shows a larger proportion of migrant families than urban-born families falling into the lowest income bracket. This may be attributable to the larger proportion of migrants employed in personal services, small crafts industry, and very small-scale retail

[35] See Jorge Balán, "Migrant-Native Socioeconomic Differences in Latin American Cities: A Structural Analysis," *Latin American Research Review,* 4 (Spring, 1969), p. 21; and José Luis Reyna, "Some Patterns of Occupational Mobility: The Mexican Case," *Social Research,* 35 (Autumn, 1968), 540–564.

[36] Cf. Gino Germani, "Social and Political Consequences of Mobility," in Neil J. Smelser and S. M. Lipset (eds.), *Social Structure and Mobility in Economic Development* (Chicago: Aldine, 1966), pp. 391–394; and W. R. Armstrong and T. G. McGee, "Revolutionary Change and the Third World City: A Theory of Urban Involution," *Civilisations* (Belgium), 18 (1968), 353–376. Official statistics show that between 1950 and 1957, the income of the poorest 20% of the Mexican population decreased in both relative and absolute terms: Ifigenia M. de Navarrete, "Income Distribution in Mexico," in Enrique Pérez López, *et al., Mexico's Recent Economic Growth* (Austin: University of Texas Press, 1967), pp. 166, 170. The data on migrant perceptions of upward mobility and job satisfaction provided by the *Measurement of Modernism* survey are of particular interest in this connection. Migrants were asked whether they had gotten farther than they expected (in terms of occupational or career objectives), or not as far. Obtained responses are as follows:

Farther than respondent expected	72%
Equal with what respondent expected	23
Not as far as expected	5

In addition, some 72% of the same respondents reported that they held jobs which were "good" or "fine" in their estimation.

TABLE 3. COMPARATIVE FAMILY INCOME LEVELS OF MIGRANTS, URBAN-BORN, RURAL, AND ALL MEXICAN FAMILIES

Average Monthly Income in Mexican Pesos[a]	% of Migrant Families[b]	% of Urban-Born Families	% of All Mexican Families[c]	% Rural Families[d]
499 or under	56	34	61	73
500–999	22	28	26	19
1000–1499	13	16	10	8[f]
1500–2499	6	10	2	
2500–4999	3	7	1[e]	
5000 or over	1	5		
Total	101%	100%	100%	100%
(N)	(422)	(736)		
	$\chi^2 = 66.30$, $p < .001$			

[a] At the time of the surveys which provided income data for this table, Mexican pesos could be exchanged for U. S. dollars at a rate of 12.50 per dollar. This is the current exchange rate.

[b] Data for both migrant and non-migrant (urban-born) families reported in the *Civic Culture* survey are for the time period June–July, 1959.

[c] Calculated from Secretaría de Economía, Dirección General de Estadística, Departmento de Muestreo, *Ingresos y egresos de la población de México, en le mes de octubre de 1956: investigación por muestreo* (Mexico, D. F., 1958), pp. 76, 98, 119, 141, and 162. Time period for the data is October, 1956.

[d] Data from Pablo González Casanova, *La Democracia en México* (Mexico, D. F.: Editorial ERA, 1967), Table LXV, p. 277. Time period is 1961–1962.

[e] Includes all reported incomes above 2500 pesos per month.

[f] Includes all reported incomes above 1000 pesos per month.

trade, where wage levels have not kept pace with those in other sectors of the urban economy.[37] Comparison of the distribution of migrant incomes with those of all Mexican families shows that 23% of the migrant families fall into the higher income brackets (1000 or more pesos per month), as against the national proportion of only 13%. And the incomes of migrant families are clearly superior to those of families in the rural areas from which they come, where just 8% of the incomes fall into the upper-income groupings.

Whether or not we perceive the objective economic basis for the oft-hypothesized "frustration gap" characterizing the situation of the migrant, there is yet need to examine his subjective evaluation of that situation and the prospects for improvement. The data presented in Table 4 show a positive expectation of achieving family economic improvement among 57% of the cases in our *Civic Culture* migrant sample. This compares favorably with the level of positive expectations of improvement among the urban-born population.[38] Certainly there is little here to support the notion of widespread and deep frustration of expectations which is basic to much writing on urbanization and political instability in Latin America. Our data also indicate a curvilinear relationship between migrant expectations of improvement and length of urban residence, with the highest level of positive expectations occurring among those with five to nine years of residence in the city. It might be inferred from this that migrant expectations upon arrival in the city and in the early post-migration period are relatively low; that greater awareness of opportunities and the general affluence of urban society creates high, probably unrealistic expectations during the 5–9 year period of residence; that in subsequent years frustration of such inflated goals leads the migrant to adjust his aspirations closer to the possible. Thus our migrant data lend support to a general proposition regarding the attitudinal consequences of persisting deprivation: "In the very long run men's expectations about the goods and conditions of life to which they are entitled tend to adjust to what they are capable of attaining."[39]

What is the relationship between objective economic condition and subjective mobility expectations among the in-migrant population? Our data from the *Civic Culture* sample show a slight, positive relationship (Pearson $r = .14$, significant at the .002 level) between objective condition (indexed by family income level) and expectations of improvement. But even among the most objectively deprived, as indicated by family incomes of less than 500 pesos per month (U.S. $40), some 53% of the migrants have expectations of future economic improvement. This finding leads us to hypothesize that even for

[37] Cf. Denis Lambert, "The Accelerated Urbanisation of Latin America and the Formation of a Tertiary Refuge Sector," *Civilisations* (Belgium), 15 (1965), 158–174, 309–325, 477–492; Fernando H. Cardoso and José Luis Reyna, "Industrialization, Occupational Structure, and Social Stratification in Latin America," in Cole Blasier (ed.), *Constructive Change in Latin America* (Pittsburgh: University of Pittsburgh Press, 1968), pp. 22–44; and Reyna, *Social Research*, 35 (1968), *op. cit.*, pp. 542–543.

[38] Comparable data from the *Measurement of Modernism* survey are as follows:

(Expectation)	(N = 68) *Migrants*	(N = 249) *Metropolitans*
Improve	87%	90%
Stay the same	9	9
Get worse	4	1

[39] Gurr, *op. cit.*, p. 1109. Further corroborating evidence for Mexico City is reported in Olivé Negrete and Barba de Piña Chán, *op. cit.*, pp. 235, 255–56.

TABLE 4. EXPECTATIONS OF FUTURE ECONOMIC IMPROVEMENT AMONG MIGRANTS AND URBAN-BORN[a]

Expectation	Migrants by Length of Urban Residence (in years)					All migrants	Urban-born
	under two	2–4	5–9	10–19	20 or more		
Improve	57%	54%	65%	58%	54%	57%	64%
Stay the Same	27	25	18	15	12	17	16
Get Worse	11	6	8	14	14	12	9
Don't Know	5	15	8	13	20	14	10
Total	100%	100%	99%	100%	100%	100%	99%
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
	$\chi^2 = 20.79$, $p < .05$					$\chi^2 = 8.93$, $p < .05$	

[a] Item: "How will the economic situation of your family change in the next ten years?"

those individuals whose premigration training and experience is such that it significantly impedes social and economic mobility, city life brings satisfactions of a non-objective nature (e.g., opportunities for individual ownership of a homesite on the urban periphery) which help considerably to offset perceptions of relative deprivation and hold promise of future betterment.[40] Present and projected rates of Mexican economic expansion suggest that the goals of many aspirants from the lower-income strata of the newly-arrived, in-migrant population—as well as children of migrants with longer urban residence—may meet frequent disappointment. Nevertheless, the migrants' persistent non-perception of structural obstacles to vertical mobility seems supportive of the existing socio-political order.

VI. SOCIO-PSYCHOLOGICAL ADJUSTMENT

The gap between mobility expectations and concrete socio-political orientations is not bridged directly, however. There are a number of variables of a socio-psychological nature which may also figure prominently in the calculus of support or non-support. All such variables relate to the alleged difficulties of migrants in making a successful mental adjustment to the urban setting. The deterioration of nuclear family and extended family bonds is postulated in much of the literature as one inescapable consequence of integration into the urban-industrial culture. As early as 1952, however, Oscar Lewis presented evidence that, at least in the case of Mexico, strong family and kinship ties were wholly compatible with urban life and industrialization.[41] Lewis's subsequent work in this area, based on a much wider sample of migrant families, served to corroborate these early observations and showed that migrant family life in Mexico City remained quite stable, and that extended family ties increased rather than decreased. He found that the average size of nuclear families in the city was at least as large or somewhat larger than in the migrants' native village.[42] Still more recent field studies of migrants to Mexico City have shown that migra-

[40] For a contrasting analysis, based on survey data for Mexico City only, in which urban experience is found to be essentially unrelated to subjective dimensions of living conditions, see Clifford Kaufman, "Urbanization and Political Involvement: Limited Reflections on the Case of Mexico City" (unpublished paper presented at the Annual Meeting of the American Political Science Association, Washington, D.C., September, 1968), pp. 6–10. On migrant satisfactions deriving from expanded opportunities for home ownership in marginal settlement zones, see Deneke, "The *Colonias Proletarias* of Mexico City," *op. cit.*, pp. 77–78, 108; William L. Flinn, "The Process of Migration to a Shantytown in Bogotá," *Inter-American Economic Affairs*, 22 (Autumn, 1968), p. 88; William P. Mangin, "Mental Health and Migration to Cities: A Peruvian Case," in D. B. Heath and R. N. Adams (eds.), *Contemporary Cultures and Societies of Latin America* (New York: Random House, 1965), p. 549; and Talton F. Ray, *The Politics of the Barrios of Venezuela* (Berkeley: University of California Press, 1969), pp. 154–156.

[41] Oscar Lewis, "Urbanization Without Breakdown: A Case Study," *Scientific Monthly*, 75 (July, 1952), 31–41.

[42] Oscar Lewis, "La cultura de la vecindad en la ciudad de México," *Actas del III Congreso Internacional de Americanistas* (2 vols.; San José, Costa Rica, 1959), Vol. I, pp. 387–402.

tion is increasingly by family group and have failed to detect any significant deterioration of the family or kinship group in the post-migration situations.[43] My data strongly support these findings. The average nuclear family size of migrants in the *Civic Culture* sample is over 5.1 persons, with 35% of the migrant families having seven or more members. The distribution of migrant nuclear family sizes is essentially comparable to that of the urban-born and of all Mexican families according to the 1960 census. Thus we find little empirical basis for the notion of widespread primary-group breakdown as a significant factor contributing to migrant disorientation and maladjustment in the cities.

A potentially more important factor, more difficult to measure and interpret, is that of anomie: the lack of stable cognitive orientations to a changed situation. In terms of classical folk-urban social theory, anomic response to urban life may take the form of a significant decline in religious participation, a high divorce rate, a loss of both trust and a sense of cohesion in interpersonal relations. All such phenomena are indicative of a breakdown of traditional norms, value systems, and social controls, with an attendant potential for embracing extremist ideologies or movements. Thus such demographic statistics as percentage of population divorced in a heavily Roman-Catholic Latin American country have been used as indicators of breakdown in traditional value systems and predictors of the percent voting for the radical left.[44] No clear pattern emerges from our data on anomic response among migrant and urban-born populations in Mexico. Table 5 shows that the migrants as a whole tend to be slightly more an-

[43] See Butterworth, *op. cit.*, pp. 266–267; and Balán, *et al., op. cit.*, pp. 128–151.

[44] Cf. Soares and Hamblin, *op. cit.*, p. 1055.

TABLE 5. INDICATORS OF ANOMIC RESPONSE TO URBAN ENVIRONMENT

Indicator	Migrants by Length of Urban Residence (in years)					All migrants	Urban born
	under two	2–4	5–9	10–19	20 or more		
Frequency of Participation in Religious Services	%	%	%	%	%	%	%
Weekly or more often	73	76	80	79	80	78	74
Once in a while	24	23	17	21	16	20	23
Only on holidays	2	0	3	0	3	2	2
Never	0	0	0	0	1	1	2
(N)	(41)	(49)	(69)	(131)	(153)	(443)	(795)
						$\chi^2 = 5.44$ (N.S.)	
Divorce Rate	0	5	7	1	5	4	2
(N)	(32)	(40)	(57)	(102)	(115)	(346)	(628)
						$\chi^2 = 3.64$ (N.S.)	
Social Trust							
Most people can be trusted	23	27	28	20	33	27	30
You can't be too careful	68	62	69	75	58	66	66
Depends, Don't Know	9	12	3	4	9	7	4
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2 = 5.20$ (N.S.)	
Social Cohesion							
People are more inclined to help others	9	19	22	15	11	15	15
More inclined to help themselves	84	77	74	80	81	79	79
Miscellaneous, Don't Know	7	4	4	5	7	6	6
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2 = 0.20$ (N.S.)	

omic than the urban-born on two of the four indicators provided by the *Civic Culture* survey. The differences, however, are not statistically significant. The contention that "migration may produce anomie, but only in the long run," since it may take considerable time for migrants to lose traditional norms and values carried over from rural life,[45] is not confirmed here. In fact, the data indicate that participation in religious observances, social trust, and sense of social cohesion tend to increase among migrants with longer residence in the city.[46]

What specific politically-relevant attitudes might we expect to be associated with anomic response to the urban environment? Although this question has never been explored adequately in survey research involving Mexican or other Latin American populations,[47] we would, on theoretical grounds, expect an anomic person to be confused and bewildered by events in politics and government; to exhibit feelings of political ineffectiveness or impotence; and to display cynical attitudes toward the men and practices governing society and the state and their responsiveness to his needs.[48] Four items were selected from the *Civic Culture* interview schedule in an attempt to identify and measure some of these attitudinal correlates of anomie among members of our migrant sample.[49] Agreement with items 1 and 2, a "not-at-all-likely" response for item 3, and a "no" response for item 4 in our Table 6 could be interpreted as anomic. The data suggest that, in general, migrants are more inclined toward this type of response than urban-born Mexicans, although once again most differences between the two populations are slight. Only with respect to feelings of political cynicism (as measured by item 4) do migrants score significantly higher on the anomie index than urban-born respondents.

VII. POLITICIZATION

Even if one were to accept the prevailing orthodoxy that rapid urbanization provides a basis for political radicalization and instability through the creation of what we have defined as the "frustration gap" and the "anomic gap," still other variables of an explicitly political nature would have to be examined before attempting to assess the stabilizing or destabilizing consequences of the urbanizing process for developing political systems. Levels of migrant politicization, for example, are crucial in determining the conditions of stability or instability in rapidly expanding urban areas. Before people engage in political action, before they are moved to protest, articulate grievances or make demands upon governmental structures, they must be at least minimally politicized.[50] As we have noted, much of the recent developmental literature assumes a high level of politicization among urban populations, even among those elements whose

[45] *Ibid.*, p. 1062; and Soares in Lipset and Rokkan, *op. cit.*, p. 489.

[46] Lewis also found that the religious life of Tepoztecan migrants to Mexico City appeared to be "at least as vigorous" and "more Catholic and disciplined" than in their native village (Oscar Lewis, "Further Observations on the Folk-Urban Continuum and Urbanization with Special Reference to Mexico City," in Philip M. Hauser and Leo F. Schnore (eds.), *The Study of Urbanization* [New York: Wiley, 1965], p. 495). Cf. Bryan R. Roberts, "Protestant Groups and Coping with Urban Life in Guatemala City," *American Journal of Sociology*, 73 (May, 1968), 753–767.

[47] See, however, the limited data on anomie and attitudes toward political participation among lower-class urban dwellers in Buenos Aires, reported in Margot Romano Yalour de Tobar and Maria Magdalena Chirico, "Clase obrera, anomia, y cambio social," *Cuadernos del Centro de Estudios Urbanos y Regionales* (Argentina), No. 9 (1967), 1–39.

[48] See Charles D. Farris, "Selected Attitudes on Foreign Affairs as Correlates of Authoritarianism and Political Anomie," *Journal of Politics*, 22 (February, 1960), p. 53; and Herbert McClosky and John H. Schaar, "Psychological Dimensions of Anomy," in Heinz Eulau (ed.), *Political Behavior in America* (New York: Random House, 1966), p. 483.

[49] Two of these items (listed as items 1 and 2 in Table 6) were also included as measures of political efficacy in the Survey Research Center's 1956 study of the U.S. electorate. For U.S. data and interpretations, see Angus Campbell, *et al.*, *The American Voter* (New York: Wiley, 1960), pp. 515–519.

[50] In this discussion we will adhere to the concept of politicization developed in the work of Daniel Goldrich, who conceives of the politicization process as "a continuum ranging from lack of perception of the revelance of government to one's life, through perception of it, to active involvement in politics" (Goldrich, "Toward the Comparative Study of Politicization in Latin America," in D. B. Heath and R. N. Adams (eds.), *Contemporary Cultures and Socieiies of Latin America* [New York: Random House, 1965], p. 361). "This concept of politicization has two dimensions: one, the perception of the personal relevance of government, is perceptual [or cognitive]; the other, political activity, is overt and behavioral" (Goldrich, *Sons of the Establishment* [Chicago: Rand McNally, 1966], p. 11).

TABLE 6. ATTITUDINAL CORRELATES OF ANOMIE

Item	Migrants by Length of Urban Residence (in years)					All migrants	Urban-born
(N)	−2 (44)	2–4 (52)	5–9 (72)	10–19 (137)	20+ (162)	(467)	(830)
1. "Politics and government are so complicated that the average man cannot really understand what's going on."	%	%	%	%	%	%	%
* Agree	86	64	68	78	64	71	67
Disagree	0	27	25	18	28	22	26
Depends, misc.	14	10	7	4	8	8	7
						$\chi^2=2.70$ (N.S.)	
2. "People like me don't have any say about what the government does."							
* Agree	71	67	76	80	74	75	76
Disagree	27	29	19	16	22	21	21
Other, DK	2	4	4	4	4	4	4
						$\chi^2=0.06$ (N.S.)	
3. "If you made an effort to [influence some government decision], how likely is it that you would succeed?"							
Very likely	14	4	7	10	10	9	5
Somewhat likely	27	29	44	46	28	36	43
* Not at all likely	55	56	33	39	48	45	43
Don't know	5	12	15	6	14	11	8
						$\chi^2=15.02$, $p<.01$	
4. "Suppose there were some question that you had to take to a gov't office [would you receive equal treatment?]"							
Yes	27	25	29	38	32	32	45
* No	59	67	64	56	58	60	47
Depends, DK	14	8	7	6	10	8	8
						$\chi^2=22.15$, $p<.001$	

* Anomic response category.

political consciousness has been awakened only recently through cityward migration. Thus we find references to the threat of "teeming urban populations . . . so highly politicized that they have become in a sense loaded revolvers pointed at the responsible governments and on the verge of being triggered off at the slightest provocation."[51] Both the assumption of uniformly high urban politicization levels and of increased political cognition as a result of migration to the big city appear largely untenable, however, in the Mexican case. While some studies have detected a slight correlation between migration and

[51] Lucian W. Pye, "The Political Implications of Urbanization in the Development Process," in *United States Papers Prepared for the United Nations Conference on the Application of Science and Technology for the Benefit of the Less Developed Areas* (7 vols.; Washington, D.C.: U.S. Government Printing Office, 1962), Vol. VII, p. 87.

heightened identification with the nation-state as a political entity,[52] they have failed to demonstrate any appreciable increase in migrant familiarity with, or interest or participation in, anything even resembling "national affairs," or even in popular programs of urban-based, national political groups.[53] Oscar Lewis found that among residents of two Mexico City *vecindades* (high-density, centrally-located, "conventional" slums) political apathy and lack of involvement in both formal and informal associations were just as common among persons who had been in the city for over 30 years as among recent arrivals.[54] Once again our data corroborate Lewis's observations. In Table 7 we find that among both migrant and urban-born populations less than half of the respondents have attained even minimal levels of politicization; and that on all nine indicators, the migrants as a whole exhibit lower cognition than the urban-born. On most indicators the migrants show considerable improvement with longer residence in the city, a pattern which persists with chronological age, sex, and education controlled. But even among those with 20 or more years of urban residence, 67% have failed to develop any sense of the personal relevance of the national government; 69% perceive no effect of local government; and only 13% regularly follow political and governmental affairs. The proportion of migrants who pay any attention to national election campaigns actually declines over time from 51 to 44 percent. Thus we find that, in terms of the taxonomy employed in the original analysis of these data, most migrants fall into the category of "parochials," i.e., those citizens manifesting little awareness of, or interest in, the national political system.[55]

Examination of the data on several behavioral indicators of politicization (Table 8) reveals essentially the same pattern. On all but one indicator (political party membership) the migrants show slightly lower participation frequencies than the urban-born population, but in most cases the differences do not prove statistically significant. On just one indicator (voting) do migrants show appreciably higher levels of political activity with longer residence in the city. Even after 20 years of urban residence, however, only 54% of the migrants eligible to vote in the 1958 election did so.[56] And when controls for age, sex, and education are introduced, we find educational attainment to be far more important in determining participation levels on this and other indicators than either length of urban residence or the other control variables.[57] Thus the general trends emerging from our analysis of migrant politicization in the Mexican context lend little support indeed to the common conception of the in-migrant populations of Latin American cities as increasingly politicized, "available masses." Even if differences in cognition levels between new arrivals and long-time urban residents are in fact

[52] See, for example, Guy T. Ashton, "Early Adulthood and Mexican National Identity: Consequences of Migrations by Yucatec Adolescent Shoemakers," *América Indígena,* 27 (April, 1967), 301–316; Butterworth, *op. cit.,* p. 265; and Lelio Mármora, "Marginalidad y conciencia nacional en grupos migrantes," *Aportes* (France), No. 7 (January, 1968), 29-46. Cf. Frederick W. Frey, "Socialization to National Identification among Turkish Peasants," *Journal of Politics,* 30 (November, 1968), pp. 943–944.

[53] At least one analyst has suggested that urban migration promotes competitive party politics in Mexico because of the increased access to individuals resulting from urban clustering, which "permits and encourages transmission of political values to persons who formerly were untouched by political propaganda" (Johnson, *Urbanization and Political Change in Latin America, op. cit.,* pp. 99–100). Although similar arguments have been advanced in subsequent writings (see, for example, Burnett and Johnson, *Political Forces in Latin America, op. cit.,* p. 51), we are not aware of any data which demonstrates empirically the purported linkage between growing strength of opposition parties in Mexico and increased support from *in-migrant* populations now in a situation of "political availability."

[54] Lewis, in Hauser and Schnore, *op. cit.,* p. 496.

[55] Cf. Almond and Verba, *op. cit.,* pp. 17–18; and Gabriel A. Almond and G. Bingham Powell, Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown, 1966), p. 53.

[56] The national turnout of eligible voters in the 1958 general election was 49.4%. This was the first such election in which both men and women voted. More recent cross-national survey research in other Latin American countries also fails to reveal any marked increase in participant orientations among in-migrant populations with longer exposure to the urban environment. In Argentina and Chile, for example, longer urban residence was found to be *negatively* related to higher participation frequencies (unpublished research, Project on the Social and Cultural Aspects of Economic Development, Department of Social Relations, Harvard University, 1969).

[57] This finding is reflective of those reported in Almond and Verba, *op. cit.,* especially pp. 379–387, and elsewhere in the literature of political development with respect to education as the most accurate predictor of participation levels.

TABLE 7. POLITICIZATION INDICATORS: COGNITIVE

Item	Migrants by Length of Urban Residence (in years)					All Migrants	Urban-born
	−2	2–4	5–9	10–19	20+		
1. Perceived effect of national government on daily life:	%	%	%	%	%	%	%
Great effect	2	2	3	4	6	4	8
Some effect	20	14	32	24	28	25	24
None	78	84	65	72	67	71	68
(N)	(41)	(51)	(66)	(130)	(153)	(441)	(808)
						$\chi^2=7.25$, $p<.05$	
2. Perceived comprehension of national and international issues:							
Excellent	2	2	0	0	1	1	2
Fair	0	4	4	6	8	6	8
Poor	33	44	41	44	43	42	49
None	64	50	54	50	49	52	42
(N)	(42)	(52)	(68)	(134)	(160)	(456)	(805)
						$\chi^2=13.28$, $p<.01$	
3. Perceived effect of local government on daily life:							
Great effect	0	0	3	7	9	6	8
Some effect	7	16	26	23	22	21	26
None	93	84	71	70	69	74	66
(N)	(43)	(50)	(70)	(131)	(152)	(446)	(805)
						$\chi^2=7.67$, $p<.05$	
4. Perceived comprehension of local or regional issues:							
Excellent	5	2	3	2	3	3	7
Fair	0	10	7	13	13	11	15
Poor	34	40	44	51	48	46	49
None	61	48	46	34	35	41	29
(N)	(44)	(52)	(70)	(137)	(158)	(461)	(824)
						$\chi^2=25.34$, $p<.001$	
5. Has some conception of possible channels for influencing local government decisions*	55	58	70	69	61	63	67
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2=2.14$ (N.S.)	
6. Has some conception of possible sources of political information*	55	54	50	56	49	53	61
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2=8.02$, $p<.01$	
7. Follows accounts of political and governmental affairs:							
Regularly	5	12	4	15	13	11	17
Occasionally	27	37	47	40	37	39	40
Never	68	52	49	45	50	50	43
(N)	(44)	(52)	(72)	(133)	(161)	(462)	(819)
						$\chi^2=9.78$, $p<.01$	

* Detailed affirmative response.

TABLE 7—(*Continued*)

Item	Migrants by Length of Urban Residence (in years)					All Migrants	Urban-born
	−2	2–4	5–9	10–19	20+		
8. Attention paid to national election campaigns:							
Much	21	12	12	14	8	12	15
Little	30	35	31	41	36	36	41
None	49	53	57	45	56	52	45
(N)	(43)	(51)	(65)	(131)	(159)	(449)	(815)
						$\chi^2=6.21$, $p<.05$	
9. Able to identify one or more of the major ideals or goals of the Mexican Revolution	59	58	65	99	80	79	98
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2=34.81$, $p<.001$	

TABLE 8. POLITICIZATION INDICATORS: BEHAVIORAL

Item	Migrants by Length of Urban Residence (in years)					All Migrants	Urban-born
	under two	2–4	5–9	10–19	20 or more		
1. Membership in politically relevant groups or organizations[a]	%	%	%	%	%	%	%
	2	8	14	14	8	10	11
(N)	(44)	(51)	(70)	(132)	(162)	(459)	(821)
						$\chi^2=0.25$ (N.S.)	
2. Political party membership	7	17	6	11	4	8	7
(N)	(44)	(52)	(71)	(135)	(158)	(460)	(825)
						$\chi^2=0.60$ (N.S.)	
3. Voted in last (1958) general election (if eligible)	43	52	46	57	54	52	58
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2=3.88$, $p<.05$	
4. Ever done anything to try to influence a local government decision:							
Often	0	0	0	0	1	0	1
Once or twice, a few times	0	2	3	11	4	6	5
Never	100	98	98	89	95	94	94
(N)	(43)	(50)	(71)	(135)	(158)	(457)	(821)
						$\chi^2=4.14$ (N.S.)	
5. Frequency of discussion of public affairs with other people:							
Nearly everyday	5	6	1	3	2	3	3
Once a week	5	4	3	7	2	4	4
Time to time	23	27	31	25	28	27	34
Never	68	64	65	66	69	67	59
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(825)
						$\chi^2=7.13$ (N.S.)	

[a] Organizations "in any way concerned with governmental, political, or public affairs;" which "take stands on or discuss public issues or try to influence government actions."

attributable to longer exposure to the urban environment, such differences do not necessarily imply a greater readiness to engage in articulation of demands or otherwise to seek access to the political decision-making process. Hence the importance of dealing separately with cognitive and behavioral dimensions of the politicization process among urban migrants.[58]

VIII. LEGITIMACY ORIENTATIONS

It may be argued, however, that urbanization as an agent in Latin American political stability or instability should not be assessed primarily in terms of its effects on politicization levels. More important as a threat to the stability of the political system is the ideological radicalization of urban politics; the development of nonsupportive, alienated attitudes and orientations toward the system among the migrant masses, due either to the nature of the assimilation experience or to factors intrinsic to the urban environment itself. Such feelings could be intensified and exploited by extremists or dissident political groups for the purpose of undermining the legitimizing argument for the system. The empirical questions to which these arguments lead are (1) the extent to which nonsupportive attitudes are distributed through the populace; and (2) the extent to which such feelings are actually translated into behavior contributing to conditions of political instability. In a one-party-dominant system of the Mexican type, such behavior may take the form of support for opposition parties or movements committed to major alterations of political structure or rules of the game, as well as for overt acts of political violence.

Our data on legitimacy orientations of migrants (Table 9) do not suggest any widely-shared attitude potential which could provide the basis for political radicalism and instability in Mexican urban areas. The migrant respondents in general demonstrate a relatively high level of system affect; in most cases higher than urban-born respondents. Some 85% of all migrants believe that the ideals and goals of the Mexican Revolution have already been realized or that people are still working to realize them; only 10% believe that the government party's policies or activities would "probably" endanger the national welfare.[59] The data for items four and five do show a significant decline in system affect among migrants with respect to local and national government outputs, as length of urban residence increases; yet even among those with 20 or more years of residence in the city, 85% continue to believe that Revolutionary goals and ideals have been achieved or are still being pursued actively, and just 10% believe that the official party's actions would eventually endanger the nation's welfare.[60]

It has been suggested that an estimate of the prospects for stability or instability in Latin American countries might be arrived at through simultaneous attention to two key dimensions of national systems, legitimacy and politicization.[61]

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[58] Cf. Daniel Goldrich, *et al.*, "The Political Integration of Lower Class Urban Settlements in Chile and Peru," *Studies in Comparative International Development*, 3 (1967–1969), pp. 14–15; and Daniel Pécaut, "The Urban Working Class," in Claudio Véliz (ed.), *Latin America and the Caribbean: A Handbook* (New York: Praeger, 1968), pp. 677–678.

[59] A similar pattern emerges from the data on system affect provided by the *Measurement of Modernism* survey. The two relevant items and obtained response distributions are as follows:

	Migrants (N = 68)	*Metropolitans* (N = 249)
(1) Mexico is much better off today because we had the Revolution	79%	77%
The Revolution hasn't solved our problems	21	23
(2) Political parties that exist now are providing adequate solutions for the problems of Mexico:		
Always	21%	19%
Sometimes	62	53
Rarely	13	19
Never	4	10

[60] These results are consistent with Almond and Verba's original finding that Mexicans in general are more alienated from governmental output than from the system itself, especially as the latter relates to and embraces the goals and symbolism of the Mexican Revolution: see *The Civic Culture, op. cit.*, pp. 495–496; and Sidney Verba and Gabriel Almond, "National Revolution and Political Commitment," in Harry Eckstein (ed.), *Internal War* [New York: Free Press, 1964], pp. 205–232). As Kahl has noted, this ambivalence is explained in part by the general cynicism pervading Mexican political culture. "Since a Mexican expects everyone to be out for himself, he is not outraged by self-seeking politicians and union leaders. Nor would he anticipate that things would be very different if the political system were changed. Therefore he does not blame the Revolutionary institutions for the chicanery of his fellow citizens, which he sees merely as a reflection of the Mexican temperament. . . . He believes in a government whose representatives he distrusts": *Measurement of Modernism, op. cit.*, p. 116.

[61] See Daniel Goldrich, "Toward an Estimate of

TABLE 9. LEGITIMACY ORIENTATIONS

Item	Migrants by Length of Urban Residence (in years)					All Migrants	Urban-Born
	−2	2–4	5–9	10–19	20+		
1. Regards political system as chief object of personal pride as a Mexican:	%	%	%	%	%	%	%
	41	23	25	31	33	31	28
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)
						$\chi^2 = 1.32$ (N.S.)	
2. Evaluation of extent to which the ideals and purposes of the Mexican Revolution have been realized[a]							
Ideals have been realized	26	36	38	17	19	23	21
People still working to realize them	53	46	56	65	66	62	58
Ideals have been forgotten	16	18	3	10	10	10	16
Don't know	5	0	3	8	5	5	5
(N)	(19)	(22)	(32)	(89)	(83)	(245)	(552)
						$\chi^2 = 4.12$ (N.S.)	
3. "Do you think that (the official party's) policies and activities would ever seriously endanger the national welfare?"							
Probably happen	11	14	9	10	10	10	12
Might happen	36	38	20	31	25	29	28
Probably not happen	39	36	39	35	39	37	43
Don't know	14	12	33	24	26	24	17
(N)	(44)	(50)	(70)	(135)	(158)	(457)	(826)
						$\chi^2 = 9.57$, $p < .05$	
4. Evaluation of extent to which national government's activities improve conditions in the country[b]							
Tend to improve	—*	—*	71	43	52	50	63
Sometimes improve, sometimes don't	—*	—*	8	20	11	15	22
Better off without them	—*	—*	21	37	36	35	15
(N)	(7)	(8)	(24)	(35)	(44)	(118)	(252)
						$\chi^2 = 18.67$, $p < .001$	
5. Evaluation of extent to which local government's activities improve conditions in the area[c]							
Tend to improve	—*	—*	75	45	42	49	50
Sometimes improve, sometimes don't	—*	—*	15	26	28	25	24
Better off without them	—*	—*	10	29	30	26	27
(N)	(3)	(6)	(20)	(38)	(43)	(110)	(260)
						$\chi^2 = 0.05$ (N.S.)	

[a] Among those cognizant of the ideals and purposes of the Mexican Revolution.
[b] Among those perceiving effect of the national government on daily lives.
[c] Among those cognizant of effect of local government on daily lives.
* Too few cases.

These two dimensions can be conceived as ranging from support through acquiescence to opposition; and from non-politicization through moderate politicization to high politicization. Assessing significant groups of people according to their position within these dimensions would then indicate potential sources of political stability or instability.[62] The matrices appearing as Tables 10–11 order the data for our migrant sample in this manner. Important in an assessment of the potential for instability stemming from urbanization in Mexico is the fact that the largest group of migrants according low legitimacy to the political sytem (the "opposed," clustered in cell *C* of the matrices) are also among the least politically aware and involved (the "non-politicized"). Thus, while these individuals may harbor basic political dissatisfactions, their attitudes would not appear to be immediately threatening to system stability. Of equal significance is the fact that no more than five out of over 340 cases cluster in cell *A* of the matrices, where we would expect to find those most predisposed to radical political activity.

IX. POLITICAL RADICALIZATION

In an effort to assess the extent to which non-supportive legitimacy orientations and feelings of alienation among migrants are translated into actions, or advocacy of actions, which might contribute to political instability, we may examine patterns of support for opposition parties and movements. Kenneth F. Johnson has documented several cases among "numerous open expressions of citizen discontent with Mexican public policy in urban areas," focused on such matters as excessive taxation and service deprivation.[63] He regards as one indicator of political radicalism among the materially-depressed urban masses their support for such "independent civic protest movements against established urban regimes . . . [movements which have] preferred to remain aloof from any of the established political parties."[64] However, the data for our migrant sample presented in Table 12 suggest a very low level of potential support for such movements among the in-migrant population. Only 9% of the migrants regard protest demonstrations as the most effective way of influencing government decisions; some 37% believe this form of political action to be the least

TABLES 10–11. LEGITIMACY-POLITICIZATION MATRICES[a]

Legitimacy Orientation[b]	Politicization (Cognitive)[c]					
	Highly Politicized		Moderately Politicized		Non-Politicized	
Opposed	*A*	5	*B*	9	*C*	31
Acquiescent	*D*	6	*E*	52	*F*	72
Supportive	*G*	4	*H*	34	*I*	129

($N = 342$; $\chi^2 = 30.99$, $p < .001$)

Legitimacy Orientation[b]	Politicization (Behavioral)[d]					
	Highly Politicized		Moderately Politicized		Non-Politicized	
Opposed	*A*	0	*B*	12	*C*	35
Acquiescent	*D*	7	*E*	48	*F*	76
Supportive	*G*	6	*H*	64	*I*	100

($N = 348$; $\chi^2 = 29.23$; $p < .001$)

[a] Based on data from the *Civic Culture* migrant sample only. Cell frequencies represent actual numbers of cases. "Don't know" cases have been excluded from the matrices.

[b] As measured by a summative index constructed from responses to items 2 through 5 in Table 9, and validated through inter-item correlations. Respondents were assigned a score of 0, 1, or 2 on each item, from least to most legitimacy accorded the system. Summing scores across all items in the index, a respondent's total score may range from 0 to 8. A score of 0–2 is coded as "opposed;" a score of 3–5 as "acquiescent;" and one of 6–8 as "supportive."

[c] Measured by a summative index constructed from items 1, 2, 3, 4, 7, and 8 in Table 7. Respondents were assigned a score of 0, 1, or 2 on each question, from lowest to highest level of political awareness indicated by the response. Summary scores range from 0 to 12. A score of 0–3 is coded as "non-politicized;" a score of 4–8 as "moderately politicized;" and one of 9–12 as "highly politicized."

[d] Measured by a summative index constructed from items 4 and 5 in Table 8. Respondents were assigned a score of 0, 1, or 2 on each item, from lowest to highest level of political involvement indicated. Sums of scores range from 0 to 4. A score of 0 is coded as "non-politicized;" a score of 1–2 as "moderately politicized;" and one of 3–4 as "highly politicized."

the Probability of Social Revolutions in Latin America: Some Orienting Concepts and a Case Study," *The Centennial Review*, 6 (1962), 394–408.

[62] Goldrich, *Sons of the Establishment, op. cit.*, pp. 13–15.

[63] Johnson, *Urbanization and Political Change in Latin America, op. cit.*, p. 113.

[64] *Ibid.*, p. 111.

TABLE 12. POLITICAL RADICALIZATION INDICATORS

Indicator	Migrants by Length of Urban Residence (in years)					All Migrants	All Urban-born	Alienated Migrants[c]	Alienated Urban-born[c]
	under two	2–4	5–9	10–19	20 or more				
Opposition party preference[a]	%	%	%	%	%	%	%	%	%
PAN	6	5	2	0	5	3	9	15	22
PP	0	0	0	1	0	0	0	0	1
Other	0	0	0	2	0	1	1	1	2
(N)	(32)	(39)	(64)	(101)	(140)	(376)	(670)	(40)	(90)
						$X^2=6.86$, $p<.05$		$X^2=0.9_4$ (N.S.)	
Opposition party voting[b]	—*	11	0	4	15	10	17	8	9
(N)	(19)	(27)	(33)	(78)	(87)	(244)	(472)	(106)	(232)
						$X^2=5.38$, $p<.05$		$X^2=0.00$ (N.S.)	
Regards protest demonstrations as the *most* effective method of influencing a government decision	5	17	14	11	4	9	10	9	7
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)	(170)	(355)
						$X^2=0.01$ (N.S.)		$X^2=0.47$ (N.S.)	
Regards protest demonstrations as the *least* effective method of influencing a gov't decision	59	23	26	38	40	37	51	43	52
(N)	(44)	(52)	(72)	(137)	(162)	(467)	(830)	(170)	(355)
						$X^2=21.66$, $p<.001$		$X^2=2.19$ (N.S.)	

[a] Among those expressing a party preference. The PAN (Partido de Acción Nacional) is Mexico's principal right-wing opposition party; the PP (now the PPS, or Partido Popular Socialista) is its counterpart on the left.

[b] For the candidate of the PAN in the 1958 presidential election, among those voting. For all practical purposes, "opposition voting" in 1958 was limited to the PAN, since the PP and another splinter party, the PARM, supported the official party's candidate for president. The Communist Party's candidate received fewer than 10,000 write-in votes.

[c] Those respondents scored as "opposed" on the legitimacy orientation index employed in Tables 11–12.

* Too few cases.

effective method. Even among politically alienated migrants, the same proportion of respondents perceive demonstrations as the most effective tactic, and 43% consider them the least effective.

With respect to preference for opposition political parties, we find once again a very low level of migrant support. Only 3% of all migrant respondents in the *Civic Culture* sample express a preference for an opposition party; 10% report voting for the opposition candidate in the 1958 presidential election. These migrant proportions are somewhat lower than those for the urban-born population. There is no large increase in migrant support for opposition parties as length of urban residence increases. Thus it would appear, on the basis of survey data, that the increased political availability of large segments of the populace now concentrated in the nation's major urban centers has not been effectively translated into support for groups opposed to incumbent national leaders. On the basis of the same evidence it might also seem reasonable to conclude that the stability of the electorate and the ability of the ruling party to control the political life of individual states or localities would not be closely tied to the volume of migration into such political units. However, aggregate electoral and demographic data show an increasingly strong, positive correlation between net in-migration into these entities and frequency of opposition voting (Spearman rank coefficient = .28 for the 1958 presidential election; .40 for the 1961 congressional elections; and .60 for the 1964 presidential election).[65] Yet in such cases as Baja California Norte and the Fed-

[65] Computed for all federal entities experiencing a net gain in population due to in-migration in the 1950-1960 period, from electoral data reported in González Casanova, *La democracia en México, op. cit.*, Table 46, p. 249, Table 55, p. 258, and Table 57, pp. 262–63; and net in-migration data presented in Cabrera, *Demografía y Economía,* 1 (1967), *op. cit.*, Table 12, Col. 2, p. 357. This trend does not appear when urbanization is indexed by simple percentage change in proportion of state population residing in rural areas: see Barry Ames, "Bases of Support for Mexico's Dominant Party" (unpublished paper, Dept. of Political Sci-

eral District, which have experienced the highest rates of both in-migration and opposition voting, it is doubtful that the relative potency of migration in determining electoral outcomes has been nearly as great as the aggregate indices suggest. In Baja California, for example, level of economic development, distance from the national capital and proximity to the United States, historical antecedents, localized issues of political corruption and abuse of power, the quality of state and local governmental outputs, and the dynamism and political skill of opposition leadership, are all key factors to be considered in attempting to account for the high incidence of opposition voting in the state's major urban centers.[66] Moreover, there would appear to be a strong possibility that patterns of partisanship among urban migrants are dependent in large measure upon the political coloration of the city in which they take up residence.[67] While his rural and/or small-town socialization insures that the migrant is far from a *tabula rasa* upon arrival in the big city, he is frequently, as Hobsbawm has observed, an individual with "no prior commitment—or even potential commitment—to any version of urban or national politics, or indeed to any beliefs which could form the basis of such politics."[68] Under these circumstances, a migrant vote against the official party's candidate for president or congressman may represent more a conformist adaptation to the dominant political opinion of the host community than a protest reaction against the hardships of adjustment to city life. If the ineptitude, non-responsiveness, or corruption of the official party regime at the local level has aroused widespread dissatisfaction, the probability of opposition voting among migrants as well as all other elements in that locality to fill elective positions at all levels of the system is greatly enhanced. The disparity between the apparently strong relationship of in-migration to opposition voting in such states as Baja California Norte and the Federal District as reflected in the aggregate data, and the much weaker association between migrant status and opposition voting reflected in our national survey data underscores the hazards of the so-called "ecological" or "aggregative" fallacy in writing and theorizing on Latin American urbanization and its socio-political consequences. Lacking better data, it may seem efficacious to attempt inferences about the behavior of micro units (such as migrants) using aggregate data from macro units (such as states).[69] But the need anticipated by some researchers for testing the results of macro-level, ecological-correlational analysis of the association between urbanization and political attitude and activity patterns against sample survey data, is confirmed here.[70]

X. CONCLUSION

Two general impressions which emerge strongly from the foregoing examination of the Mexican case are (1) a sense of the extraordinary complexity of the urbanization phenomenon itself; and (2) the inadequacy of most conventional theory for dealing with the phenomenon in research on political instability in a Latin American context. As the results of field studies undertaken in a number of countries begin to accumulate, it becomes increasingly evident that there is a high degree of within-region variation with respect to patterns and consequences of urbanization. It would be inaccurate to regard the outcomes flowing from such a process as tending essentially toward stability or instability in the political order, from either a long- or short-term perspective.[71] Certainly in the Mexican case

ence, Stanford University, 1968, pp. 23–24).

[66] For enlightening commentary on the corruption issue and recent political trends in Baja California Norte, by an ex-governor of the state now expelled from the official party due to his heretical utterances during the 1964 national election campaign, see Braulio Maldonado Sández, *Baja California: Comentarios políticos* (Mexico, D.F.: Costa-Amic, 1960).

[67] The analysis of urban political behavior in Brazil presented in Frank P. Sherwood, *Institutionalizing the Grass Roots in Brazil: A Study in Comparative Local Government* (San Francisco: Chandler, 1967), especially pp. 28-30, points toward essentially the same conclusion with respect to that country.

[68] E. J. Hobsbawm, "Peasants and Rural Migrants in Politics," in Claudio Véliz (ed.), *The Politics of Conformity in Latin America* (New York and London: Oxford University Press, 1967), p. 60.

[69] One recent study of political consequences of urbanization in Mexico utilizing such an aggregate data approach is Alschuler, *op. cit.*

[70] See Myron Weiner, "Urbanization and Political Protest," *Civilisations* (Belgium), 17 (1967), p. 50; and Soares and Hamblin, *op. cit.*, p. 1064.

[71] Cf. Huntington, *op. cit.*, p. 283. As Kalman Silvert has suggested, there is need to abandon the conventional view that "the mere fact of an urban agglomeration means a specific set of things for the persons who live in that agglomeration" in favor of a more probabilistic viewpoint, "that urban conglomerations in any part of the world can lead to a very broad variability of human reactions" (Silvert, "Seminar Summary," *Proceedings* of the Seminar on Social Science and Urban

there can be little doubt that rapid urbanization has contributed to conditions of material scarcity, overcrowding, and service deprivation in the big cities, while straining the absorptive capacity of both the public and private sources of employment. Yet it would appear that the rate of demand creation among the urban masses, and therefore the magnitude of the collective "frustration gap," is not as great as has been assumed. Thus given sustained economic expansion and a modicum of improvement in real income levels, the fact of discrepancy between rates of urbanization in the nation and the capacity of commerce and industry to provide adequate employment opportunities at the rate of in-migration may not be crucial to the stability of the political system.[72] Maintenance of system support will depend upon expansion of governmental performance capabilities, at all levels, and on the success of the ruling party-government apparatus in coöpting leaders emerging from the newly urbanized population into supportive political roles.[73] In any event, we find little empirical evidence to support the standard conception of the city as essentially a radicalizing environment. Nor does there appear to be an empirical basis for frequent perceptions of imminent threat to existing authority structures stemming from the rapid influx of migrants to cities. In Mexican *colonias proletarias*, as in the *favelas* of Brazil and the *barriadas* of Lima,[74] contradictions between the migrant's values regarding the city as a place of economic opportunity and his actual situation seem more likely to lead to cynical criticism of the "malfunctioning" of authority structures and institutions (due to self-seeking politicians, corrupt and unresponsive bureaucrats, etc.), than to a political questioning of the legitimacy of such structures. Finally, we find that there is little or no difference between migrants and urban-born individuals on many indices of social condition, political attitudes, and behavior patterns; and that the ratio of migrants to non-migrants is probably one of the less critical factors accounting for urban political phenomena. It would appear that a similar pattern of relationships obtains in some other Latin American countries undergoing rapid urbanization, even though these nations for the most part are not characterized by the conditions of general political stability and economic progress which mark the Mexican case. Evidence reported from such diverse countries as Brazil, Chile, Guatemala, Peru, and Venezuela indicates that even though first and second generation migrants frequently comprise a large proportion of the electorates they join, their presence in the urban center does not seem to result in any significant radicalization of politics in their new place of residence.[75] Nor do there appear to be conspicuous differences between migrants and city-born residents with respect to

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Development in Latin America, Jahuel (Aconcagua), Chile, April, 1968, forthcoming).

[72] Parallel conclusions are reached with reference to other Latin American political systems in Robert H. Dix, *Colombia: The Political Dimensions of Change* (New Haven, Conn.: Yale University Press, 1967), pp. 40–41; and Daniel Goldrich, "Peasants' Sons in City Schools: An Inquiry into the Politics of Urbanization in Panama and Costa Rica," *Human Organization*, 23 (Winter, 1964), 328–333.

[73] As Almond and Verba have observed, "The Mexican system may be described as living off its capital of system affect. Unless the output performance of the system can match the aspirations of the citizens (and what is relevant is not the objective level of output, but the evaluation of its adequacy by the citizens), then the Mexican pattern . . . may have within it the seeds of instability": *The Civic Culture, op. cit.*, p. 497.

[74] See Leeds and Leeds, "Brazil and the Myth of Urban Rurality," *op. cit.*, pp. 31, 35; and William P. Mangin, "Poverty and Politics in Cities of Latin America," in Warner Bloomberg, Jr., and Henry J. Schmandt (eds.), *Power, Poverty, and Urban Policy* (Vol. II: *Urban Affairs Annual Reviews;* Beverly Hills, Calif.: Sage Publications, 1968), pp. 419–420.

[75] See François Bourricaud, "Lima en la vida política peruana," *América Latina* (Brazil), 7 (October-December, 1964), 89-95; Ernst Halperin, "The Decline of Communism in Latin America," *Atlantic Monthly*, 215 (May, 1965), 65–70; George F. Jones, "Urbanization and Voting Behavior in tinctive mix of economic pressures, social con-Venezuela and Chile, 1958–1964" (unpublished M.A. thesis, Stanford University, 1967); Mangin, *Latin American Research Review*, 2 (1967), pp. 82–85; Lisa Redfield Peattie, *The View from the Barrio* (Ann Arbor: University of Michigan Press, 1968), pp. 139–40; Sandra Powell, "Political Participation in the Barriadas [of Lima, Peru]: A Case Study" (unpublished paper, Dept. of Political Science, San Francisco State College, 1969); José Arthur Rios, "El pueblo y el político," *Política* (Venezuela), (February, 1960), 11–36; Bryan Roberts, "Politics in a Neighborhood of Guatemala City," *Sociology* (London), 2 (May, 1968), 185–204, and *idem*, "Poverty and Politics in Guatemala City" (unpublished paper presented at the Annual Meeting of the American Anthropological Association, Seattle, Wash., November, 1968). Nevertheless, the radicalization conception cannot

several key socioeconomic attributes and attitudinal patterns.[76]

If some kind of migrant/non-migrant dichotomy does not prove useful in analyzing the conditions which tend toward alienation and instability in a given area, then what tack should investigators take in trying to deal with the urbanization phenomenon and its impact on Latin American political systems? It would appear that specific metropolitan areas and metropolitan subunits and their varying patterns of interaction with larger political environments provide the most promising focus of attention for research in this field. The migrant population of a large city contains a large and disparate array of social types, who may react in very different ways to the same set of basic social and economic conditions, or who may in fact be exposed to very different types of conditions imposed by urban settlement patterns. Even geographically contiguous "squatter settlements" on the periphery of the central city may differ sharply in the degree to which they and their inhabitants are integrated into urban social and political life. As several investigators have argued recently, settlement-zone integration should be viewed as a significant variable, subjecting migrants to a distinctive mix of economic pressures, social controls, and political influences.[77] Some settlements

simply be dismissed as mere folklore lacking *any* empirical referent in Latin American experience, past, present, or future. As Germani and others have noted, the conventional wisdom would appear to have a rather high degree of fit with the Argentine situation in the Perón era (see Gino Germani, "The Transition to a Mass Democracy in Argentina," in Arpad von Lazar and R. R. Kaufman (eds.), *Readings in Latin American Politics* (Boston: Allyn and Bacon, 1969), pp. 138–140; and Andújar, *Revista Paraguaya de Sociología*, 6 (1966), *op. cit.*); and it may also be accurately descriptive of some aspects of the situation in Santo Domingo during the 1965 Dominican revolt (see José A. Moreno, *Barrios in Arms: Revolution in Santo Domingo* (Pittsburgh: University of Pittsburgh Press, forthcoming), Chap. 5). Moreover, the evidence is as yet too fragmentary to discount the applicability of the radicalization proposition to second and third generation in-migrants, even though the data which have been reported thus far do not reveal any significant radicalization of the second generation in terms of support for extremist parties, frustration of socio-economic expectations, or diminished confidence in existing socio-political institutions: see Goldrich, *Human Organization*, 23 (1964), *op. cit.*, pp. 332–333; and Adolfo Gurrieri, *Situación y perspectivas de la juventud en una población urbana popular* (Santiago de Chile: United Nations, Economic Commission for Latin America, 1965; Doc. E/LACCY/BP/L.2), pp. 16-18, 24–27. Yet with respect to newly-arrived migrants and first generation in-migrants in general—the groups to which the commonly-accepted notions of "explosive potential" are most frequently applied—theorists do indeed "have a burden of proof that there is an empirical reality that corresponds to their abstractions": Kahl, *Measurement of Modernism, op. cit.*, p. 97.

[76] See Balán, *Latin American Research Review*, 4 (1969) (summarizing survey data from Brazil, Argentina, Chile, Mexico, and El Salvador), *op. cit.*; Guillermo Briones, "Movilidad ocupacional y mercado de trabajo en el Perú," *América Latina*, 6 (July-September, 1963), 63–76; Bertram Hutchinson, "Urban Social Mobility Rates in Brazil Related to Migration and Changing Occupational Structure," *América Latina*, 6 (July-September, 1963), 47–61; Leeds and Leeds, "Brazil and the Myth of Urban Rurality," *op. cit.*, pp. 16–17; Héctor Martínez, "Las migraciones internas en el Perú," *Aportes* (France), 10 (October, 1968), pp. 144–145; and William P. McGreevey, "Causas de la migración interna en Colombia," in Centro de Estudios sobre Desarrollo Económico (CEDE), *Empleo y desempleo en Colombia* (Bogotá: Ediciones Universidad de los Andes, 1968), p. 215. For a general discussion of the methodological inadequacies of the migrant/non-migrant dichotomy for social science research on Latin America, see Harley L. Browning and Waltraut Feindt, "Natives vs. Migrants: A False Dichotomy?" (unpublished paper, Population Research Center, University of Texas at Austin, 1967).

[77] Elizabeth R. Leeds, "Political Complementarity of *Favelas* with the Larger Society of Rio de Janeiro" (unpublished paper presented at the 37th International Congress of Americanists, Mar del Plata, Argentina, September, 1966; forthcoming in Anthony Leeds (ed.), *The Favelas of Rio* (Austin: University of Texas Press for the Institute of Latin American Studies); Lloyd H. Rogler, "Slum Neighborhoods in Latin America," *Journal of Inter- American Studies*, 9, (October, 1967), 507–528. Several forthcoming survey studies of lower-class urban settlements will provide extensive data on the operation of this variable in the Mexican context. These include Susan Eckstein, "Social, Economic, Political, and Cultural Integration of the Urban Poor in Mexico City" (title approx.; Ph.D. dissertation in progress, Columbia University); Charles Ornelas, "Land Tenure Sanctions, and Politicization in Mexico City" (Ph.D. disserta-

are afflicted with recurrent waves of tension, antagonism, and conflict; others show evidence of no such stress.[78] Much systematic research focused on these ecological variations and their relationship to rates and modes of politicization and demand creation will be necessary if we are to arrive at meaningful generalizations regarding the importance of urbanization as an agent in Latin American political instability.

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tion in progress, University of California, Riverside); and Wayne A. Cornelius, Jr., "Political Correlates of Migrant Assimilation in Mexican Urban Environments" (Ph.D. dissertation in progress, Stanford University).

[78] The work of Oscar Lewis and John F. C. Turner also suggests the need for examination of differential structural and contextual properties of lower-class urban settlement zones in Latin American cities. In his well-known anthropological study, *The Children of Sánchez* (New York: Vintage Books-Random House, 1963, pp. xii-xviii), Lewis describes sharp economic and socio-psychological contrasts between two slum neighborhoods in Mexico City which he studied in depth. Similarly divergent sets of characteristics and functions are ascribed by Turner to the "bridgehead" and "consolidation" settlements of metropolitan Lima: "Uncontrolled Urban Settlement: Problems and Policies," *International Social Development Review* (United Nations), 1 (1968), pp. 116–120, and *idem, The Squatter Revolution: Housing as a Vehicle for Social Change* (Cambridge: MIT Press, forthcoming). See also the conceptualization of "slums of hope" and "slums of despair" in Charles J. Stokes, "A Theory of Slums," *Land Economics*, 38 (August, 1962), 187–197.

PARTY COMPETITION AND WELFARE POLICIES IN THE AMERICAN STATES*

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Recent comparative, quantitative studies of state politics conclude that party competition and other political variables have little or no impact on important state policies such as per pupil expenditure, old age assistance, unemployment compensation, and aid to dependent children.[1] These are rather unexpected and disturbing conclusions for they disconfirm relationships predicted by some of the most important theoretical formulations concerning democratic politics. Before re-examining the bases for these conclusions, a review of theory is in order.

I. THEORY

V. O. Key, Jr., set the context for examining the effects of political variables on state policies.[2] He stressed the importance of two-party competition, or bi-factionalism in one-party states, as a determinant of policy. Key sees the degree of party competition as crucial because it reflects the extent to which politics is organized or unorganized. Party competition by producing some semblance of an organized politics lessens the difficulty of lower status groups in sorting out political actors and issues, thereby enabling them to promote their own interests more effectively. Since state social welfare policies are undoubtedly relevant to the interests of "have-nots," we can utilize state expenditures in this area as a measure of the success that these groups have enjoyed. Key's formulation, then, would lead to a simple two variable model:

$P = e_1$
$S = k_1P + e_2$
Where P is inter-party competition, S is expenditure on social welfare, and e_1 and e_2 represent error or variables left out of the system, and k_1 is a constant.

This model simply states that, all other things being equal, increasing competitiveness will produce increased social welfare expenditures.

Lockard's analysis of policy in the New England states brought some refinements to Key's model.[3] Lockard argued that states with complex economies would be more likely to have competitive parties since the diversity of economic interests would provide a context within which two partyism could flourish. He also noted that this diversity makes it difficult for a few powerful interests to dominate policy-making. The addition of Lockard's thinking to the basic model is straightforward:

$D = e_1$
$P = k_1D + e_2$
$S = k_2P + e_3$
Where D is the level of economic development and all other symbols are the same as above.

This model states that there is a developmental sequence between the three variables; a high level of economic development yields inter-party competition which in turn produces the level of welfare expenditures.

The more recent literature rejects the developmental sequence predicted by the Key-Lockard observations.[4] Utilizing multiple and partial correlational analysis, or their non-parametric analogues, this recent literature finds that when socio-economic factors are held constant, the impact of party competition largely disappears,

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[1] See, especially Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *Journal of Politics,* Vol. 25 (1963), pp. 265–289 and Thomas R. Dye, *Politics, Economics and the Public* (Chicago: Rand McNally and Co., 1966).

[2] V. O. Key, Jr., *Southern Politics* (New York: Vintage Books, 1949), p. 307.

[3] Duane Lockard, *New England State Politics* (Princeton, N.J.: Princeton University Press, 1959), see, especially, pp. 336–337.

[4] Dawson and Robinson, *op. cit.*, and Dye, *op. cit.*

while the effects of socio-economic development do not vanish when party competition is held constant. From these findings these researchers conclude that party competition does not serve as an intervening variable between socio-economic development and policy. They conclude that party competition and policy are both dependent on socio-economic development. Their revised model then is:

$$D = e_1$$
$$P = k_1D + e_2$$
$$S = k_2D + e_3$$

All symbols are the same as above.

This model states that any association between competition and social welfare expenditures is spurious. There is no causal connection between competition and welfare, rather they are both caused by socio-economic development.

This model is certainly unexpected in that the theoretical formulations, if not the conclusions, of these recent studies are perfectly consistent with those of Key and Lockard. The framework common to these studies is that of Easton's systems analysis which stresses the importance of environment, process, and outputs.[5] Economic and social development are treated as aspects of the environment, party competition is categorized as an aspect of the political process, and governmental expenditures are conceptualized as the outputs of the system. Easton's notion that aspects of the political process are a consequent of the environment and serve to translate factors of that environment into policy outputs is also common to these efforts. These research designs, then, began with the hypothesis that political factors such as party competition act as intervening variables between the socio-economic environment and output measures. This hypothesis is the same as that embodied in the Lockard model above. That this hypothesis appears to be disconfirmed, raises serious questions concerning assumptions regarding the importance of political variables in explaining policy outcomes.

Yet, if we return to examine the theoretical notions underlying the predictions from Key and Lockard, it may still be possible to find grounds upon which to reexamine the relevance of political variables. First, it seems clear that Key's thinking would not lead us to expect that party competition, or any other political variable, would serve as an important explanatory variable for all types of public policies. While this statement may seem trivially true, it leads to an important question. What kinds of policy would party competition be important for? Key's answer seems to depend on what he thought are the politically significant questions; questions of the sort that may have become lost in recent literature.

> The significant question is, who benefits from political disorganization? Its significance is equalled only by the difficulty of arriving at an answer. There probably are several answers, depending on the peculiar circumstances in each case. Politics generally comes down, over the long run, to a conflict between those who have and those who have less. . . . It follows that the grand objective of the haves is obstruction, at least of the haves who take only a short-term view. Organization is not always necessary to obstruct; it is essential, however, for the promotion of a sustained program in behalf of the have-nots, although not all party or factional organization is dedicated to that purpose.[6]

Party competition, then, may not have an appreciable impact on some types of policies, but Key's discussion would lead us to specify the conditions under which it would have its greatest effect: the have-not oriented policies. If party competition serves to provide political organization useful to the have-nots in their struggle with the haves, then party competition would be most intimately related to those policies which are central to that struggle. Key's status conflict formulation enables us to discard the simplistic hypothesis that party competition is important for explaining policy in general. We would hypothesize that party competition would differentially explain policies as a function of the centrality of the policy area to the have, have-not struggle. For example, party competition may not be important for many kinds of education policies. Although they may belong to a social welfare dimension, education policies fall much less clearly in this dimension than other more controversial and more have-not oriented policies such as aid to dependent children.[7]

If we expect the explanatory strength of party competition to vary with the type of policy, it seems but a short theoretical leap to expect similar variation in the importance of other characteristics of the political system. In fact it is possible to consider variables which are most important in exactly those situations in which variables such as party competition are least important. What we are thinking of here

[5] David Easton, "An Approach to the Analysis of Political Systems," *World Politics*, 9 (1957), 383–400.

[6] Key, *loc. cit.*

[7] See, Ira Sharkansky and Richard I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," in this issue of this REVIEW.

are the perceptual and attitudinal distributions of the individuals who occupy the important political roles in state systems. Thus as Lockard has pointed out competition may place rather rigid electoral restraints upon the kinds of policy appeals that can be made.[8] However, where these external restraints do not exist, or are weakest, the greatest room would seem to exist for individual variation in policy-making. In these situations, our level of explanation would remain meager as long as we restrict our measurements to non-individual based variables.

Of course such variables may never be gathered systematically for any but a few states at any one point in time. In this paper, we wish to merely raise the possibility of the importance of such variables. The explanatory value of such variables in other areas of political research emphasizes the fact that very little effort has been expended at rounding out the measurement of the many political variables that may intervene between state environments and policy.[9] From these additional theoretical considerations we can derive the following model:

$$D = e_1$$
$$P_1 = k_1D + e_2$$
$$P_2 = k_2D + e_3$$
$$P_3 = k_3D + e_4$$
$$\cdot$$
$$\cdot$$
$$\cdot$$
$$P_n = k_nD + e_{n+1}$$
$$k_{n+1}P_1 + k_{n+2}P_2 + \ldots + k_{2n}P_n + e_{n+2}$$

Where $P_2, P_3, \ldots P_n$ are political variables other than party competition which intervene between socio-economic development and social welfare policy.

This model states that there are "n" number of political variables all of which serve to transform aspects of the environment into public policy.

We of course do not know if this model best fits any data. However, we can use it to make predictions about what would happen if it were descriptive of the policy making process and we measured only one or a few of the intervening variables in any one study. If, for example, we measured only P_1, then the model would be:

$$D = e_1$$
$$P_1 = k_1D + e_2$$
$$S = k_2P_1 + k_3D + e_3$$

In this model social welfare policies are the result of two forces, both of which originate in socio-economic development. One is transmitted through the intervening variable party competition and the other appears as a direct effect of development because we have ignored the additional intervening political variables.

In review, the evidence and theoretical formulations we have considered suggest three primary models which have some kind of relevance to the issues in this literature. The first we shall label the Key-Lockard model because we derived it from their work and it calls for a developmental sequence between the three types of variables. The second we shall call the Dawson-Robinson model because those authors were the first of a series to postulate a spurious relationship between competition and social welfare policy. The third model depends on our reworking of these theoretical strains and can be thought of as a hybrid of the others. This model predicts effects from both party competition and socio-economic development with party competition's effect as part of a developmental sequence originating in development.

II. METHODOLOGY

If we can derive three different models from the existing theoretical formulations, the next step is to select a methodology that enables us to choose between them empirically. Severe difficulties arise at this point, however, because our theory has to some extent outpaced conventional research techniques. For this reason, we will develop the logic of a cluster of tests which will help to alleviate these problems.

Normally in a test of a developmental sequence like the Key-Lockard model $j \rightarrow k \rightarrow i$ we control for the hypothesized intervening variable (k) to determine whether the original relationship between the independent (j) and dependent (i) variables remains. In making this test we are attempting to choose between the developmental sequence model and another model in which the test variable does not intervene and effects from the independent variable remain. This is the case because we would not control for the hypothesized intervening variable unless there were zero-order relationships between the three variables in the first place. On the other hand, when controlling in a test for spuriousness such as the Dawson-Robinson model would require, we normally partial for the in-



[8] Lockard, *op. cit.*

[9] For example, see Warren E. Miller and Donald E. Stokes, "Constituency Influence in Congress," this REVIEW, 57 (1963), 45–56.

dependent (j) variable which hypothetically accounts for the original relationship between two dependent (k and i) variables. In making this test, we are attempting to choose between the spuriousness model and another in which an effect remains between the latter two variables. Once again we proceed with the test only after determining whether zero-order relationships exist between all three variables.

In both tests, we are attempting to choose between the hypothesized model and a second. However, in this instance, we are attempting to choose between models associated with the two tests: developmental sequence and spuriousness models. At this point, the conventional testing procedures becomes inadequate.

If we use partial correlation coefficients, or some analogous technique, often we can not distinguish between these models. In such controls, we are attempting to hold "constant" that variation in other variables which is due to the control variable. We can conceptualize what is involved in these procedures by thinking of residual variation after the control. In the developmental sequence test, we hold constant variation in the independent and dependent variables which is related to the control variable. The relationship which remains after the control, therefore, is the association between the residuals in both variables. Thus if the variation in the dependent variable which is due to the independent variable is entirely transmitted through the intervening variable, then, the residual relationship should be zero in the pure case. If the residual relationship is of a magnitude to make us think that it does not really depart from that of the original relationship, usually we would reject the developmental sequence model. However, this does not mean that we can then infer the spuriousness model, because it is not the only alternative model.

Similarly, if we make the spuriousness test, we examine the residuals (which are not related to the control variable) between the two variables under test. If a relationship remains after the control, we may want to reject the spuriousness model, but this test alone does not allow us to infer that a developmental sequence exists.

Moreover, even if we use the two tests in combination often we will be unable to distinguish between the two models. The reason for this is that when we proceed with the test for spuriousness, we will observe a reduction in the relationship between the two variables under test when either model fits the data. If the relationship is that of a developmental sequence, when we control for spuriousness we remove variation due to the most independent variable in the sequence. Therefore, the controlled relationship between the remaining variables is due to their residuals which are unrelated to the control variable. If the relationship is that of a developmental sequence, we will be examining the correlation between residuals that are left over after variation due to the initial variable in the sequence has been removed. In this case, we would not expect a very high partial relationship because with the first causal variable in the sequence removed, we are left with relationships that are primarily outside the sequence. In other words, we will observe reductions in the partial relationship with this kind of control when the relationships are *either* spurious *or* developmental.[10] As a result we can not use this test to distinguish between the two models. This is the procedure utilized in the recent literature.

The advice when either of these models is a possibility is to partial using unstandardized regression coefficients rather than partial correlation. Since these coefficients are unstandardized they are unaffected by the reduction in variation due to controlling for background factors. Therefore, if we test for spuriousness when a developmental sequence exists, the magnitude of the partial regression coefficient will remain unchanged and we will not improperly infer spuriousness.

However, this strategy also breaks down when in addition to the developmental sequence and spuriousness models we also have our third hybrid model as a possibility. Thus, if both a direct effect from the independent variable and a developmental sequence through an intervening variable exists, the behavior of the regression coefficients will be affected by a control for spuriousness. In this situation the magnitude of the regression coefficient between the intervening and dependent variables will be reduced because we will be subtracting from that association any relationship due to the direct effect of the independent variable. Therefore, if we make this test, we may confuse the spurious with the hybrid model because we will observe a reduction in the regression coefficients in both cases. Due to the complexity of our hybrid theory, the introduction of our model to the set of possibilities for testing represents a second breakdown in the straight-forward methodology of causal inference.

To overcome this second difficulty, we propose that partial regression coefficients be utilized with controls for both spuriousness and de-

[10] For the argument in this and the subsequent paragraph, see, Hubert M. Blalock, Jr., *Causal Inferences in Non-Experimental Research* (Chapel Hill: U. of North Carolina Press, 1964), pp. 83–87.

TABLE 1. BEHAVIOR OF REGRESSION COEFFICIENTS UNDER TESTS FOR SPURIOUSNESS AND DEVELOPMENTAL SEQUENCE AND MODEL TO BE INFERRED

Inference	Test for: Spuriousness	Developmental Sequence
Spuriousness	Reduction	No Reduction
Developmental Sequence	No Reduction	Reduction
Hybrid	Reduction	Reduction

velopmental sequences—a combination of controls which might lead to improper inferences if partial correlations are used, as discussed above. If a test of spuriousness, on significant zero-order regression coefficients, does not yield a reduction in the regression of party competition on a welfare policy, we may safely infer the most appropriate model is that of a developmental sequence. On the other hand, if the test does produce a reduction in the regression coefficient, we are still faced with the problem of choosing between the spuriousness and hybrid models for both models predict a reduction.[11]

We have a basis for inference, however, if we also test for a developmental sequence. Both the hybrid and developmental sequence models predict a reduction in the regression of the environment on the policy, while the spuriousness model predicts no reduction.[12] In other words, only when we observe a reduction in the regression coefficient under *both* tests should we infer the hybrid model. When the spuriousness and developmental sequence tests yield a reduction in one case but not in the other, we are in a position to choose between the other two models. Table 1 summarizes this discussion.

Yet a third difficulty remains. If we focus on the *magnitude* of the regression coefficients and use the significance test most commonly associated with regression (the F test), we would often conclude that our controls resulted in regression coefficients which are not significant. The reason for this is that this test, like correlation, is related to the amount of variation in our variables. If we happened to control for a background variable, as in the test for spuriousness, we would be reducing that variation perhaps to the point of making the partial regression not significant.[13] Normally, we avoid this outcome by being careful not to control for background variables. However, when two of the theoretical models under test require controlling for background variables, we must confront this difficulty.

The proposed solution in this situation is to make use of the fact that our theory predicts a *change* in the magnitude of the regression coefficients. Thus the null hypothesis is that the magnitudes will remain the same even after the control. Because of this, we can utilize confidence limits within which the magnitudes could have varied on a chance basis. Our research problem then is not just whether the controls reduced their magnitudes, but whether they reduced them by an amount greater than they could have by chance, given a significance level of, say, .05. If the partial is smaller than the corresponding lower confidence limit we conclude that there has been a significant reduction, if it is not smaller, we conclude that we cannot reject the null hypothesis.

III. ANALYSIS

Key's notions of variation in the relevance of party competition to policy, according to the centrality of the policy to the conflict between the haves and have-nots, led us to select four state expenditure policies for analysis—per pupil expenditure, old age assistance, unemployment compensation, and aid to dependent children.[14] All of these policies can be thought of as belonging to the same dimension of social welfare liberalism, broadly defined. Important differences exist, however, in the degree to which they are defined in the United States as embodying a welfare dimension. In other words, they vary in how central they are to the conflict between haves and have-nots. Thus, although educational policy is very important as an arena of conflict between the haves and have-nots, it includes rewards for both groups and has not been defined generally as a policy specifically designed for the poor, at least in recent times. Aid to dependent children, on the other hand, is designed explicitly to provide funds for the poor and is frequently defined as a "dole." Old age assistance and unemployment compensation, defined as policies designed to assist those who have worked long and hard before retirement or who are *temporarily* unemployed, would seem to occupy something of a middle ground between education and aid to dependent children.

[11] See mathematical appendix below.

[12] *Ibid.*

[13] Hubert M. Blalock, Jr., *Social Statistics* (New York: McGraw-Hill Book Co., 1960), p. 355.

[14] Data for per pupil expenditures and per receipt payments for old age assistance, unemployment compensation, and aid to dependent children taken from *The Book of the States: 1962–63* (Chicago: Council of State Governments, 1963). Data for Hawaii and Alaska, not included.

TABLE 2. REGRESSION COEFFICIENTS BETWEEN INTER-PARTY COMPETITION AND FOUR SOCIAL WELFARE POLICIES IN THE AMERICAN STATES, WITH CONTROLS FOR FOR FOUR SOCIO-ECONOMIC DEVELOPMENT FACTORS

	Regression Coefficients					
	Simple b	Lower Confidence Limit	Partial			Non-Agricultural Occupations
			Education	Urbanization	Income	
Per Pupil Expenditure Reduction	1.878	1.391	1.402* — *	1.257 (30%)	.218 (88%)	1.176 (37%)
Old Age Assistance Reduction	.319	.207	.129 (60%)	.262* — *	.233* — *	.357** — **
Unemployment Compensation Reduction	.132	.098	.086 (35%)	.101* — *	.058 (56%)	.100* — *
Aid to Dependent Children Reduction	.859	.636	.685* — *	.728* — *	.581 (32%)	.766* — *

* Not significant at .05 level.

** Control slightly, but not significantly, increases its value.

The test for spuriousness between party competition[15] and each of the four policies by controlling for each of four socio-economic factors is shown in Table 2. In every policy area, at least one control variable significantly reduces the regression coefficient between party competition and the policy. The regression for per pupil expenditure is reduced significantly by income, non-agricultural occupations, and urbanization. The regressions for old age assistance, unemployment compensation, and aid to dependent children are significantly reduced by education, education and income, and income, respectively. As discussed in the methodology section above, we are now in a position to reject the Key-Lockard pure developmental sequence model, because it predicts no reduction in the regression coefficients. On the other hand, we are not in a position to choose between the spuriousness and hybrid models, because they both predict a significant reduction.

Before proceeding to the test for a developmental sequence, we would like to note two other implications of Table 2. First, the socio-economic factor that reduces the regression coefficient the most for each policy area, income for per pupil expenditure, education for old age assistance, income for unemployment compensation, and income for aid to dependent children, varies considerably in how much reduction it produces. Thus, income reduces the regression for per pupil expenditure by eighty-eight percent (88%), while income only reduces the regression for aid to dependent children by thirty-two percent (32%). The reductions for old age assistance and unemployment compensation, sixty percent (60%) and fifty-six percent (56%), fall between these two poles. In other words, the extent to which we can reject the model at issue may correspond to the rough ranking of the centrality of these issues to the have, have-not struggle.

Second, in every policy area, there is at least one socio-economic variable which does not significantly reduce the regression of party competition on the policy. Education does not signifi-

[15] Our measure of party competition is drawn from Austin Ranney, "Political Parties," in Herbert Jacob and Kenneth Vines (eds.), *Politics in the American States: A Comparative Analysis* (Boston: Little, Brown and Co., 1965), pp. 64–65, which combines competition for governor, the lower house, the upper house, and percent of divided executive-legislative control into one overall index of party competition. Our indices of wealth (per capita income), urbanization (percent urban), education (median school year completed), and percent in nonagricultural occupations for 1960–61 were drawn from Bureau of the Census, *Statistical Abstract of the United States: 1962* (Washington: U.S. Government Printing Office, 1962).

cantly reduce the regression coefficient for per pupil expenditure. Urbanization and income do not affect the regression coefficient for old age assistance. Urbanization and non-agricultural occupations do not reduce the regression for unemployment compensation. Three variables, education, urbanization and non-agricultural occupations fail to have an effect on aid to dependent children. This suggests that all four variables are not always equally relevant as control variables for all policy areas, as implied by previous research.[16] The theoretical implications of this are clear. We must have a clear notion of what it is that we are controlling for and why.

This finding also emphasizes that we must be very careful when we utilize multiple controls. For example, since income is the only socio-economic variable to significantly reduce the regression coefficient between party competition and aid to dependent children, to control for the other three socio-economic factors may lead to unwarranted inferences. The variables which do not significantly reduce the effect of party competition may still be significantly related to that variable. When we control their effects in a multiple-partial correlation analysis, we are further reducing the variation in the political variable which may artificially reduce the party competition-dependent variable correlation. Such gross use of controls, then not only obscures what may be important theoretical differences between these socio-economic factors, but also may lead to unwarranted inferences of spurious relationships due to "over-controlling."

Table 3 shows the tests for a developmental sequence. Controlling for party competition fails to produce a significant reduction in the regression of income on per pupil expenditure and education on old age assistance (almost significant, $p = .08$), though the other three socio-economic factors are sharply reduced in their effects. In the other two policy areas, however, the control for party competition significantly reduces the regression coefficients of all four socio-economic

[16] See Dye, *op. cit.*

TABLE 3. REGRESSION COEFFICIENTS BETWEEN FOUR SOCIO-ECONOMIC DEVELOPMENT FACTORS AND FOUR SOCIAL WELFARE POLICIES IN THE AMERICAN STATES, WITH A CONTROL FOR INTER-PARTY COMPETITION

	Regression Coefficients			Reduction
	Simple b	Lower Confidence Limit	Partial b	
Per Pupil Expenditures				
Education	44.464	30.350	17.483	(61%)
Urbanization	3.722	2.757	2.489	(33%)
Income	.164	.140	.152*	—*
Non-Agricultural Occupations	12.008	8.801	7.103	(41%)
Old Age Assistance				
Education	9.478	6.674	6.998*	—*
Urbanization	.485	.240	.228	(53%)
Income	.020	.012	.008	(60%)
Non-Agricultural Occupations	1.286	.452	−.076**	—**
Unemployment Compensation				
Education	3.350	2.425	1.701	(49%)
Urbanization	.224	.150	.125	(44%)
Income	.010	.008	.007	(30%)
Non-Agricultural Occupations	.738	.496	.320	(57%)
Aid to Dependent Children				
Education	19.559	12.936	6.378	(67%)
Urbanization	1.238	.709	.524	(58%)
Income	.056	.039	.025	(55%)
Non-Agricultural Occupations	4.134	2.411	.942	(77%)

* Not significant at .05 level.

** Actually changes the sign for the regression coefficients indicating reduction greater than 100 percent.

variables with aid to dependent children and unemployment compensation.

The combination of the two tests suggests that the spurious model is the better inference for per pupil expenditure and old age assistance. The test for spuriousness produced a significant reduction in the regression coefficients, while the test for a developmental sequence did not.

The more appropriate inference, in aid to dependent children and unemployment compensation would seem to be the hybrid model. A significant reduction in the regression coefficient between party competition and these two policies in the test for spuriousness is matched by a significant reduction in the regression coefficients of the four socio-economic factors in the test for a developmental sequence.

IV. CONCLUSION

Our analysis leads us to several major conclusions. First, our examination of the methodology utilized in previous research into the effects of political variables on state expenditure policies convinces us that the spurious model now predominant in state politics literature rests on shaky empirical foundations. Reliance on correlational analysis can lead to unwarranted causal inferences.

Second, only for per pupil expenditure and old age assistance does the spurious model appear to be the more appropriate inference. Per pupil expenditure policies, especially, seems to be virtually a pure case of spuriousness as a function of the wealth of a state.

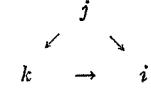
Third, because we have not found sufficient support for the relatively simple models in the areas of aid to dependent children and unemployment compensation our hybrid model appears as an alternative. Although it is not possible to estimate the exact magnitude of the impact of party competition if this is the appropriate model, it does indicate that this political variable does have an impact. Rejection of the spuriousness model then does suggest that even if there are direct effects from the environment party competition still serves as an intervening variable.

Fourth, as might have been predicted by Key, we tend to have different models with different policies. The extent to which we can reject the spuriousness model seems to vary as a function of the centrality of the policy to the struggle between the haves and have-nots. Our inferences therefore are consistent with the theory that given the advantages possessed by the haves, the organization, continuity, and visibility of alternatives provided through inter-party competition is important for the capacity of have-nots to attain policies in their own interests.

Finally, we think several research directions are suggested by our analysis. The model outlined above which suggested that there may be n number of intervening political variables implies that we need to place great stress on conceptualizing and measuring relevant political variables. To the extent that this model is the appropriate one we will continue to have trouble estimating the effects of the few political variables measured. The variation between models across the few policies considered in this analysis also suggests that we need to place as much, if not more stress on conceptualizing and measuring policy outputs beyond expenditures, especially outputs relevant to the struggle between haves and have-nots.[17]

APPENDIX

As Blalock points out if we make the test for spuriousness (control by the background variable) when the appropriate model is a pure developmental sequence the regression coefficients will be unaffected, i.e., the simple and partial coefficients will be equal. However, if we make the same control when the appropriate model is the hybrid case the coefficient will be reduced. We can prove this assertion by examining the hybrid model below. In addition to the developmental sequence $j \rightarrow k \rightarrow i$ we also have a direct effect of j on i. Employing the equation for partial regres-



sion coefficients and the fact that $(bjk)\ (bkj) = r^2jk$ we can set bij equal to $(1-r^2kj)\ (bij \cdot k) + (bik)\ (bkj)$. When we make the test for spuriousness we then have:

$$1. \quad b_{ik \cdot j} = \frac{bik - (bij)(bjk)}{1 - r^2jk}$$

$$2. \quad = \frac{bik - [(1-r^2kj)(bij \cdot k) + (bik)(bkj)]bjk}{1 - r^2jk}$$

$$3. \quad = \frac{bik - (bjk)(1-r^2kj)(bij \cdot k) - (bik)(bkj)(bjk)}{1 - r^2jk}$$

$$4. \quad = \frac{bik - (bjk)(1-r^2kj)(bij \cdot k) - (bik)(r^2kj)}{1 - r^2jk}$$

[17] See Donald J. McCrone and Charles F. Cnudde, "On Measuring Public Policy," in Robert E. Crew, Jr. (ed.), *State Politics: Readings on Political Behavior* (Belmont, California: Wadsworth Publishing Co., 1968), pp. 523–530 for an attempt to measure a qualitative output of state systems—anti-discrimination laws.

5. $$=\frac{bik-(bik)(r^2kj)-(bjk)(1-r^2kj)(bij\cdot k)}{1-r^2jk}$$

6. $$=\frac{bik(1-r^2kj)-(bjk)(1-r^2kj)(bij\cdot k)}{1-r^2jk}$$

$$bik\cdot j$$

7. $$=bik-(bjk)(bij\cdot k)$$

In other words the value of the partial regression coefficient will be that of the simple after it has been reduced by the effects of the other two links. If Blalock's pure developmental sequence were actually the appropriate model, $bij\cdot k$ would equal zero and according to equation #7 $bik\cdot j$ would equal bik, i.e. no reduction.

We can similarly show that the developmental sequence test (control for an intervening variable) when this model is the appropriate one will also result in a reduction in the coefficient. In that case we set bik equal to $(1-r^2jk)\ (bik\cdot j)+(bij)\ (bjk)$ and when we make the test we have:

$$bij\cdot k$$

1. $$=\frac{bij-(bik)(bkj)}{1-r^2kj}$$

2. $$=\frac{bij-[(1-r^2jk)(bik\cdot j)+(bij)(bk)](bkj)}{1-r^2kj}$$

3. $$=\frac{bij-(bkj)(1-r^2jk)(bik\cdot j)-(bij)(bjk)(bkj)}{1-r^2kj}$$

4. $$=\frac{bij-(bkj)(1-r^2jk)(bik\cdot j)-(bij)(r^2kj)}{1-r^2kj}$$

5. $$=\frac{bij-(bij)(r^2kj)-(bkj)(1-r^2jk)(bik\cdot j)}{1-r^2kj}$$

6. $$=\frac{bij(1-r^2kj)-(bkj)(1-r^2jk)(bik\cdot j)}{1-r^2kj}$$

$$bij\cdot k$$

7. $$=bij-(bkj)(bik\cdot j)$$

Once again the value of the partial will be that of the simple after reductions due to the other two links. On the other hand, if the purely spurious model is the appropriate one in this test we know that $bik\cdot j=0$ which means that according to equation #7 $bij\cdot k=bij$. In other words the control will not reduce the value of the coefficient.

DIMENSIONS OF STATE POLITICS, ECONOMICS, AND PUBLIC POLICY*

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I. INTRODUCTION

A question often posed by students of American state politics is: "Do state political systems leave a distinctive imprint on patterns of public policy?" Prior to recent years the nearly automatic response of political scientists was an unqualified "yes." More recent research has led to a qualified but increasingly confident "no."[1]

Several recent publications have explored relationships between various indices of state politics, socio-economic characteristics, and public policy. The general conclusion has been that central features of the political system such as electoral and institutional circumstances do not explain much of the variation in policy. There are occasionally high correlations between individual measures of voter turnout, party competitiveness, or the character of state legislatures and some aspects of governmental spending. But these political-policy correlations seem to disappear when the effect of socioeconomic development is controlled.

These are disturbing findings. They have not gone unchallenged.[2] But the challenges, rather than reassuring those who have asserted the relevance of parties, voting patterns, and government structures, have demonstrated that the burden of proof now rests on those who hypothesize a politics-policy relationship. The problem has not been resolved.

Part of the problem may rest on the conceptualization and measurement of the central variables. Electoral balance or alternation in office is not "inter-party competition," except in the most mechanical sense. Compare Massachusetts' loose-knit party structure to the centralization of Connecticut's. "Party competition" is not the same as "party organization." And party competition, voting habits, and patterns of apportionment fall far short of being equivalents of "political systems." We have been too simple-minded in our measurement of "politics" and "policy." Concepts are, hopefully, ever more closely approximated by the things we use to denote them. We propose to advance further in the approximation of "politics," "economics," and "policy" as they are used in comparative state research. As an added payoff, we offer some clarification of their interrelationships.

Our results confirm some and challenge other findings of recent research. Because we deal with factors and not isolated variables, we can speak with improved precision of which *dimensions* of policy respond to what *dimensions* of politics and economics. Our findings show that different social and economic characteristics have different relevance for policies, and their relevance varies between substantive areas of policy. Furthermore, central features of state politics are important for some policies, even when socioeconomic variation is controlled.

* Separate grants to each of the authors from the Social Science Research Council Committee on Governmental and Legal Processes provided financial support for the work that is reported here.

[1] Richard E. Dawson and James A. Robinson, "Interparty Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265–289; Thomas R. Dye; *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966); Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW, 60 (March, 1966), 73–82. One of the present authors has employed a different conception of "political" characteristics and his results qualify those of the authors listed above. See Ira Sharkansky, *Spending in the American States* (Chicago: Rand MacNally, 1968).

[2] See Sharkansky, *op. cit.*, plus James A. Wilson, *City Politics and Public Policy* (New York: Wiley, 1968), Chapter 1; and Duane Lockard, "State Party Systems and Policy Outputs," in Oliver Garceau (ed.), *Political Research and Political Theory* (Cambridge: Harvard University Press, 1968), pp. 190–220.

II. METHODS OF INDEX CONSTRUCTION

The conceptualization of state policy making into three sectors—social, political-governmental, and policy—has proven sufficiently fruitful to warrant its continued use. We have followed the guidance of previous studies in separating the objects of our study in this manner. Within each sector, however, there is no *a priori* reason to assume that the component variables are unidimensional or of equal weight. For that reason, we wish to employ techniques which test for dimensionality and which assess the relative contribution of specific variables to the patterns of relationship.

In any situation where one seeks to pin numbers on a concept, a wise strategem is to employ multiple measures in the hope that they will, in a sense, triangulate on the conceptual target more accurately than any single item.[3] Multiple measurement, however, still leaves one guessing what is the common element of the many variables used. Averaging scores or ranks and constructing composite indices of unweighted items runs the risk of making several unwarranted and unnecessary assumptions.[4] It allows for no indication of the relative impact of the items included. Multiple regression analysis runs a similar risk. It provides no means for handling the multiple measures of dependent variables. A strong case can be made for a lengthy list of indicators, but one also needs a mechanism for sorting out their commonality and diversity. If used with some sensitivity, factor analysis serves this need.[5]

Some writers have been critical of some uses to which factor analysis has been put, particularly with the increased availability of pre-packaged programs and sophisticated computing facilities. Two major criticisms are relevant in the present context. First, it is charged that factor analysis has been used in lieu of theory.[6] If one simply plugs into the machine all of the data at his command, some kind of factors will emerge. Factor analysis is not a substitute for theory. There must be some prior rationale which justifies the particular variables fed into the computer. Secondly, the technique has been criticized because the factors which emerge often seem to make little sense in terms of the theory with which one is working.[7] Theory is juggled to accommodate the machinery.

This is not the appropriate context in which to explore the intricacies of factor analysis. But we do recognize the necessity to account for some of the major criticisms and to point out how we have tried to minimize the difficulties in our inquiry. We have attempted to avoid these pitfalls in two ways. First, the variables we include have been employed because of their widespread currency in analyses by other scholars and the appealing theoretical rationale which

[3] For a lucid critique of single indicator analysis and a discussion of the methodological justification for multiple indicators see Eugene J. Webb, Donald T. Campbell, Richard D. Schwartz, and Lee Sechrest, *Unobtrusive Measures: Nonreactive Research in the Social Sciences* (Chicago: Rand McNally, 1966), pp. 3ff.

[4] For an example of such an index, see Hofferbert, "The Relation . . . ," *op. cit.*

[5] The technique of factor analysis starts from the basic assumption that interrelations among separate variables signal the existance of underlying traits—or "factors"—that they share in common. A factor analysis manipulates a collection of variables in order to discern the various patterns of relationships among them. The groups of variables that relate closely to one another, but only loosely (or not at all) to variables in other groups are extracted as the principal factors. The individual variables that show the strongest relationships with other members of their factor have, in the language of factor analysis, the highest *loadings*. A variable's *loading* is, in effect, the coefficient of correlation between that variable and the underlying factor. By knowing the variables with the highest loadings on each factor, it is possible to infer something about the underlying traits that the factor represents. The variables with the highest loadings come closest to representing the underlying trait, although it is unlikely that any single variable represents that trait perfectly. The factoring technique used with the political and service variables employs orthogonal rotation. The device examines the correlation matrix of the individual variables and extracts factors whose members show maximum correlations among themselves and minimum correlations with the members of other factors. The program used is "Image," by Walter Stoly, Mass Communications Center, University of Wisconsin, Madison, version of July 2, 1968. For an explanation of techniques used in the socioeconomic factor, see Hofferbert, "Socioeconomic Dimensions . . . ," *op. cit.* Although Hofferbert employed oblique rotation in the extraction of his factors, the comparability to our political and service factors is not appreciably affected. The intercorrelation of the two socio-economic factors for 1960 is .023.

[6] J. Scott Armstrong, "Derivation of Theory of Means of Factor Analysis or Tom Swift and His Electric Factor Analysis Machine," *American Statistician* (December, 1967), 17–21.

[7] See, for example, John E. Mueller, "Some Comments on Russett's 'Discovering Voting Groups in the United Nations,'" this REVIEW, 61 (March, 1967), 146–148.

these analysts have offered.[8] Second, as will be apparent, the factors we obtain *do* make sense and we have employed a number of checks to insure that the apparent sense they make is not an artifact of the technique.[9] In order to use the results of factor analysis to test relationships among various dimensions of politics, economics, and public policy we derived "factor scores" for each state from the combination of its value on the individual variables and the loading of each variable on each factor. The derivation of state scores on the socioeconomic factors is discussed elsewhere.[10] With factor scores as our independent and dependent variables, we use simple, partial and multiple correlation techniques to assess relationships between dimensions of state economics, politics, and public policy.

Political Factors. In choosing the items for construction of the political indices an effort was made to include a comprehensive list of characteristics that have long been of concern to political scientists. We chose measures of participation and party competition, the character of the legislative, judicial and executive branches of state government, and the individual and mutual aspects of state, local and intergovernmental fiscal structures.

The measures of turnout and competition assess prominent aspects of the electoral process, involving contests for both governor and United States Representative. The measures of party competition include both the spread of the vote received in recent elections and the distribution of legislative and gubernatorial control between parties and over time.

The variables indicating government structures, personnel and revenues tap several aspects of state and local government that may affect perception of and responsiveness to citizen interests. The variables pertaining to the legislature include three separate indices of apportionment equity, plus the number of legislators, the length of the legislature's session, the number of committees, the salary of members and total expenditures for legislative services, and the number of bills introduced and passed at a recent session. Other scholars have used these variables to assess the representativeness of state legislatures, their "professionalism" and their activism.[11] Several of the variables pertaining to the executive branch assess the magnitude and professionalism of the civil service, and the formal nature of leadership in the executive branch. They show the relative number of employees and their conditions of work. Two variables relative to executive leadership are the score of each state's governor on an index of formal authority,[12] and the number of adminis-

[8] But we have been sensitive to the possibility of biased results. Certain initial steps have been taken to reduce this bias. Factor analysis cannot be relied upon to define the dimensions of a complex phenomenon (e.g., the state political system), particularly if the collection of variables subject to factor analysis is pre-loaded with many measures of the same phenomenon, each of which is highly correlated with others. Thus our first task was to insure that we did not "stack" our deck of variables with highly intercorrelated measures ($r > .70$) of the same phenomenon. This problem was particularly acute in the case of measures of party competition. Similar screening of the policy variables we examined did not reveal comparable redundancies.

[9] To maintain *prima facie* relevance, we use factor analysis in "stages" first to select from among a large collection those variables that load highly on principal factors and then to define the loadings of these variables on factors that contain only those highly-loaded components. In this way, the factor technique produces relatively "pure" factors, devoid of large numbers of variables that contribute only weakly to the principal factors. In both the political and policy analyses, a large number of variables load low on two principal factors, and several variables load about equally on each principal factor. In order to simplify these factors for the purposes of clarity and further analysis, we eliminated all variables loading below .5 on both principal factors and those loading at least .5 on one but above .4 on the other factor in each sector. (The variables surviving this elimination are noted as such on Tables 1 and 3.) We then made separate factor analyses of the variables that remained after this culling procedure. (The results in Tables 2 and 4.) The values derived from this final set of analyses constitute the bases for constructing indices of the political and policy sectors of our model.

[10] Hofferbert, "Socioeconomic Dimensions of the American States: 1890–1960," *Midwest Journal of Political Science,* 12 (August, 1968), 401–418. The factor scoring program used for the political and policy factors if "FACTSCR2" by Keith R. Billingsley, Department of Political Science, University of Wisconsin, Madison, version of July 20, 1968. The scoring technique is described in Henry F. Kaiser, "Formulas for Component Scores," *Psychometrika,* 27 (March, 1962), 83–87.

[11] Dye, *op. cit.,* and John G. Grumm, "Structure and Policy in the Legislature," a paper presented at the Southwestern Social Science Association annual meeting, Dallas, March, 1967.

[12] Joseph A. Schlesinger, "The Politics of the Executive," in Herbert Jacob and Kenneth N.

trative officials who are directly elected. The age and length of the state constitution and the number of its amendments have been used by one scholar to assess the activism of state interest groups and their orientation toward constitution-building.[13] Also examined are terms, official qualifications, compensation and pension opportunities of the state judiciaries; these should denote some central aspects of judicial professionalism.

We measure state and local tax systems by the proportion of revenues raised from the major taxes. Each tax has a distinctive impact upon a particular segment of the population;[14] thus, the composition of state and local revenue systems may reflect the political strength of different economic groups, and affect the relationship between each group of taxpayers and state and local governments.

Several measures of intergovernmental relationships assess the mutual dependence of state and local governments and the role of federal agencies in financing public services. These variables permit an answer to the question: "Which government pays the bills?" Federal, state and local taxes vary in their progressivity-regressivity. A state that relies heavily on locally financed services may generate a different type of response among taxpayers than one that relies on state or federal revenues. It is also likely that each level of government differs in its responsiveness to demands. Thus a state that emphasizes services financed by federal or state agencies may present a different complexion of need-gratifications than one which is localist in orientation. Finally, the supervision of "avoided" programs is likely to differ from that of programs financed locally. A heavy reliance on federal and state revenues, therefore, may have administrative implications that influence other dimensions of state politics.

No two of our political variables measures the same phenomena in exactly the same manner. It is possible, however, that our penchant for a comprehensive list of indicators will overload the factor analysis in favor of phenomena (e.g., party competition) that have been the subject of several earlier measurements. In order to guard against this kind of overloading, we computed a matrix of simple correlation coefficients between all of our political variables. In those cases where variables measuring similar phenomena showed high coefficients of correlation ($r > 7$), we eliminated one of the highly intercorrelated pair from the factor analysis. The 41 variables that survived this first screening process are indicated in Table 1 and were entered into a preliminary factor analysis. This analysis was used to identify those variables which loaded high on two or more factors or loaded high in no factor. (See notes 8 and 9) A second factor analysis was performed without these ambiguities and irrelevancies, and produced the two principal factors reported in Table 2.

The first principal factor is labeled "Professionalism-Local Reliance." It draws its name from the *positively-loaded* measures of judicial and legislative compensation, expenditures on legislative services, and legislative activity; and from the *negatively-loaded* measures of reliance upon *state* government expenditures and *federal* aids. States scoring high on this factor show high salaries for judges and legislators, well-financed legislative staffs, and primary reliance on locally raised and spent revenues. The inverse juxtaposition in factor #1 of professionalism in government and the use of state and federal aids makes sense in terms of functions that intergovernmental payments are reputed to serve. They are often defended as devices that are used most by governments which redistribute resources from *have* to *have not* jurisdictions. And in fact, the recipients have apparent needs for assistance. Factor #1 suggests that states making heavy use of intergovernmental assistance have judicial and legislative institutions that are less well-developed than average.

The second factor—"Competition-Turnout" —has as its highest-loaded variables the measures of turnout in a gubernatorial election, an index of suffrage liberality, and (negatively) one-party dominance in the state legislature and in recent elections for governor.[15] The Competition-Turnout dimension provides some post hoc justification for the many studies of state politics that have focused almost entirely on electoral processes and inter-party struggles. Books and articles by V. O. Key, Duane Lockard, and John Fenton, among others, view party competition and electoral behavior as the primary stuff of politics.[16] Our factor analyses

Vines (eds.), *Politics in the American States* (Boston: Little, Brown, 1965), pp. 207–238.

[13] Lewis A. Froman, Jr., "Some Effects of Interest Group Strength in State Politics," this REVIEW, 60 (December, 1966), 952–962.

[14] See George A. Bishop, "The Tax Burden by Income Class, 1958," *National Tax Journal*, 14 (March, 1961), 41–58.

[15] In interpreting the results of the factor analysis, a loading of .700 will be considered to be high.

[16] V. O. Key, Southern Politics (New York: Alfred A. Knopf, 1949); Key, *American State Politics: An Introduction* (New York: Alfred A. Knopf, 1956); Duane Lockard, *New England State*

TABLE 1. POLITICAL VARIABLES SUBJECT TO ANALYSIS†

(Participation and Party)

**1. Percent voting age population voting for Governor
2. Percent voting age population voting for U.S. Representative
**3. Number of items on Milbrath's list of suffrage regulations on which a state scores as a "facilitator"
**4. Percent of gubernatorial vote won by major party
5. Percent of congressional vote won by major party
**6. Percent of seats in lower house of state legislature held by major party
7. Percent of seats in upper house of state legislature held by major party
*8. Number of years during 1952–62 when major party controlled Governor's office
**9. Number of years during 1954–62 when major party controlled lower house of legislature
*10. Number of years during 1954–62 when major party controlled upper house of legislature

(Institutions and Personnel)

*11. Dauer-Kelsay index of legislative apportionment
*12. David-Eisenberg index of legislative apportionment
*13. Shubert-Press index of legislative apportionment
*14. Number of state legislators
**15. Legislators' compensation
**16. Total expenditures on legislative services per legislator (minus compensation)
*17. Number of legislative committees
**18. Number of bills introduced into the legislature
19. Number of bills passed by the legislature
*20. Proportion of bills introduced that were passed
*21. Number of days in the legislative session
*22. Term of judges on state court of last resort
*23. Minimum age requirement for judges on appeals courts
*24. Minimum age requirement for judges on trial courts
**25. Compensation of judges on court of last resort
*26. Minimum term that will make state judges eligible for pension
27. Number of state employees per 10,000 population
28. Average salary of state employees
*29. Percent of state employees covered by state personnel system
*30. Schlesinger index of gubernatorial formal power
*31. Number of separately elected executive officials in the state government
*32. Number of state plus local government employees per 10,000 population
*33. Average salary of state plus local government employees
*34. Percent of state and local government employees covered by government supported health and hospital insurance
*35. Percent of state and local government employees covered by government supported life insurance
36. Number of words in state constitution
*37. Number of amendments in state constitution
*38. Date of state constitution's ratification
*39. Population per local governmental unit

(Fiscal Structure)

*40. Percent of state and local government revenues collected from real property taxes
*41. Percent of state and local government revenues collected from individual income taxes
*42. Percent of state and local government revenues collected from general sales taxes
43. Percent of state and local government revenues collected from excise taxes
*44. Percent of state and local government revenues collected from current service charges
45. Percent of state government revenue received as federal aid
**46. Percent of state and local government revenue received as federal aid
47. Percent of state and local government revenue raised by state government
**48. Percent of state and local government revenue spent by state agencies
49. Percent of state and local government revenue raised by local governments
50. Percent of local revenue received from the state government
*51. Percent of state aid to localities allocated on the basis of fixed criteria of population or the amount of certain taxes collected locally
52. Percent of state aid to localities allocated on the basis of need
*53. State aid to localities per capita

† Unless indicated otherwise, the data pertain to 1962.

* Variables surviving test for high intercorrelations among substantively similar variables.

** Variables surviving test for high loading on a single factor. (All ** variables also meet the test of those being noted with * only.)

TABLE 2. LOADINGS OF POLITICAL VARIABLES ON TWO PRINCIPAL FACTORS

Variables*	Professionalism—Local Reliance	Competition—Turnout
Compensation of judges (25)	.897	.045
Compensation of legislators (15)	.865	.142
Legislative Service expenditures (16)	.821	.025
Number of bills introduced (18)	.812	−.130
State and local revenue from federal government (46)	−.763	−.293
State and local revenue spent by state agencies (48)	−.732	−.330
Lower house seats of major party (6)	−.280	−.861
Gubernatorial election turnout (1)	.033	.827
Gubernatorial vote for major party (4)	−.077	−.775
Liberal suffrage laws (3)	−.006	.717
Lower house tenure of major party (9)	−.061	−.623
Percent of Total Variance	36.7	28.8

* The parenthesized numbers correspond to the variable numbers in Table 1.

suggest that an electoral-party dimension has indeed some importance as a distinct component of state politics.

Public Policy Factors. Our list of policy measures is limited in conception to the expenditures for and outputs of major public services. It is not designed to measure "everything government does." Some measures of government activity seem better conceived as measures of state politics than as measures of policy.[17] In the factor analysis of state public policies, the variables were chosen to represent both current expenditures for major services and approximations of results from service operations.[18] The non-expenditure variables measure "outputs" expressed as service actually rendered. Table 3 lists the variables entered into the factor analyses of policy outputs.

The measures of educational output show the capacity of secondary schools to entice students to remain until graduation and the success of each state's residents on a nation-wide examination. Presumably, the first measure assesses the schools' ability to serve the needs of the students, whether they are inclined toward college preparation or immediate placement in business or the trades. The incidence of examination passes should reflect the quality of information and the intellectual skills that state residents possess by the time they finish secondary school.

The highway output measures record the mileage of various types of state roads in relation to population. Population seems to be the best measure of traffic needs that is readily available.[19] Therefore, the measures show the incidence of various roads in relation to the demands of traffic. Admittedly, road location is not considered. Some states may build many

Politics (Princeton: Princeton University Press, 1959); John H. Fenton, *Politics in the Border States* (New Orleans: Hauser Press, 1957); and Fenton, *Midwest Politics* (New York: Holt, Rinehart and Winston, 1966).

[17] We have described above our justifications for including measures pertaining to government personnel, structure, and revenue among our political variables. In brief, they represent features of revenue inputs and governmental structures that may affect response to citizens' needs and demands. Our policy measures are more narrow in representing the benefits which governments actually provide to their citizens within the prominent categories of service.

[18] Although some research has relied almost exclusively on expenditures as indicators of public policy, a recent study shows the risk in assuming an expenditure-service relationship. The current expenditures of state and local governments may not be uniformly reliable indicators of the service that is actually provided to the population. But on the other hand spending may represent the effort that state and local governments are making in order to improve or maintain their services. Therefore it deserves a place on our list of policy variables. See Sharkansky, "Government Expenditures and Public Services in the American States," this REVIEW, 61 (December, 1967), 1066–1077.

[19] See Philip H. Burch, Jr., *Highway Revenue and Expenditure Policy in the United States* (New Brunswick: Rutgers University Press, 1962), p. 23.

miles of roads but place them unwisely with respect to centers of population, commercial needs, etc. The completion rate of the interstate system provides some indication of state highway department administrative skills and capacity to respond quickly in the face of a major opportunity. The inverted death rate provides a measure of road safety. Where a state scores high on the latter measure, it should indicate high standards of road design and maintenance and/or an adequate system of highway patrol.

Output measures in the public welfare field reflect both the generosity of payments and the coverage of each major public assistance program. These specific programs have been in operation generally for several years. They represent much of the work carried out by state departments of public welfare. However, the indicators do not assess such things as quality or quantity of counselling services, cost of living differences, or activities under the newer programs sponsored by the federal Office of Economic Opportunity.

Measures of health policies assess the likelihood of white and non-white children surviving their first year. Admittedly, these variables cover only a small portion of health-relevant services within each state. And they show the influence of many social and economic processes. Nevertheless, we assume that high scores on these scales reflect the presence of health and hospital facilities that are adequate in the face of cultural and medical needs.

Natural resources policies are reflected in measures of the tendency of state residents to use the programs and facilities offered by state departments of parks and wildlife. High scores on these variables should signal attractive programs. The basic assumption is that an attractive program stimulates usage by state residents.[20]

[20] In collecting the data for the measures of outputs and services, the assumption was made that recorded information is a reasonably accurate reflection of fact. The authors recognize considerable controversy about the reliability of the data. Yet the data chosen appear to be the best available, and they enjoy wide use among social scientists. For comments about each item the reader is referred to the sources. The sources for the political variables include: *Census of Governments, 1962: The Book of the States 1964–1965; Statistical Abstract of the United States, 1964;* the index of gubernatorial power comes from Schlesinger's article cited in note #12; the index of suffrage regulations comes from Lester Milbrath, "Political Participation in the States," in Jacob and Vines, *op. cit.*, pp. 25–60; the indices of legis-

TABLE 3. POLICY VARIABLES SUBJECT TO ANALYSIS†

(Education)

*1. Percent of ninth grade students (1959)graduating three years later
*2. Percent of candidates passing selective service mental examination
3. State and local government expenditures for education per capita

(Highways)

4. Total road mileage per capita
*5. Rural road mileage per rural resident
6. Municipal road mileage per urban resident
7. Percent of designated Interstate mileage completed
8. Population per highway fatality
*9. State and local government expenditures for highways per capita

(Welfare)

*10. Average payment, Aid to Families of Dependent Children
*11. Average payment, Old Age Assistance
*12. Average payment, Aid to the Blind
*13. Average payment, Aid to the Permanently and Totally Disabled
14. Incidence of AFDC recipients among population with incomes of less than $2,000
*15. Incidence of OAA recipients among population with incomes of less than $2,000 and over 65 years of age
16. Incidence of AB recipients among population with incomes of less than $2,000
17. Incidence of APTD recipients among population with incomes of less than $2,000
18. State and local government expenditures for public welfare per capita

(Health)

19. Proportion of white infants surviving their first year of life
20. Proportion of non-white infants surviving their first year of life
21. State and local government expenditures for health, hospitals, and sanitation per capita

(Natural Resources)

22. Visits per 10,000 population to state parks
*23. Fishing licenses sold per 10,000 population
*24. Hunting licenses sold per 10,000 population
*25. State and local government expenditures for natural resources per capita

(General)

26. Total state and local government general expenditures per capita

† The data all pertain to 1962.

* Variables surviving test for high loading on a single factor.

Examination of the simple correlations among the policy measures revealed no redundancies comparable to those found in the list of political variables. A preliminary factor analysis of the policy variables led us to eliminate ambiguities and irrelevancies which loaded high on two factors or on no factors. (See notes 8 and 9). Then a second factor analysis was performed with the remaining variables. The variables retained and their loadings are shown in Table 4. The first factor—"Welfare-Education"—emphasizes generous welfare payments, the tendency of high school pupils to remain until graduation, and the success of state residents on the national examination.

The second principal policy factor is "Highway-Natural Resources." Its major components are measures of rural highway mileage and highway expenditures, plus measures of fish and wildlife services and expenditures for natural resources. A curious variable that is loaded highly on this factor is Old Age Assistance recipients. At first glance, it seems out of its proper place in the Welfare-Education factor. Aid to the aged, however, is the product of a policy process that is distinct from that of other welfare and education programs. A number of rural states that have paid little attention to the average level of welfare benefits have shown considerable concern for their aged and have made the program available to a large number of beneficiaries. Some of this concern may be related to the political strategy of state governmental decision makers. Aside from welfare mothers (who are tainted with a stigma of illegitimacy), the aged are the largest groups of potential welfare recipients who are eligible to vote.[21] The lines of reasoning suggested by this instance will be more carefully explored in Section III below.

Socioeconomic Factors. The factor analysis of socioeconomic characteristics is discussed in detail elsewhere.[22] It includes measures of income, urbanization, industrialization, ethnicity, education and other likely components of "economic development." The variables included are listed in Table 5 in the order in which they load on two principal factors. The factors are here labelled "Industrialization" and "Affluence" ac-

lative apportionment comes from Paul T. David and Ralph Eisenberg, *Devaluation of the Urban and Suburban Vote* (Charlottesville: Bureau of Public Administration, University of Virginia, 1961), p. 5, 15, and Glendon Shubert and Charles Press, "Measuring Malapportionment," this REVIEW, 58 (1964), 969. The sources for policy variables include: National Education Association, *Rankings of the States, 1963*; U.S. Bureau of Public Road, *Annual Report 1963;* Social Security Administration, Social Security Bulletin: *Annual Statistical Supplement 1963; The Book of the States, 1964-1965; Statistical Abstract of the United States, 1964.*

[21] For a discussion of the politics of old age assistance in Alabama, see Sharkansky, *Spending in the American States,* pp. 138-141.

[22] Hofferbert, "Socioeconomic Dimensions . . . ," *op. cit.*

TABLE 4. LOADINGS OF POLICY VARIABLES ON TWO PRINCIPAL FACTORS

Variables*	Welfare—Education	Highways—Natural Resources
AFDC payments (10)	.911	.049
OAA payments (11)	.864	.170
High school graduates (1)	.848	.113
AB payments (12)	.834	.042
Examination success (2)	.779	.414
APTD payments (13)	.719	−.274
Rural road mileage (5)	.109	.869
Hunting licenses (24)	−.058	.860
Highway expenditures (9)	.118	.857
Fishing licenses (23)	.040	.800
Natural resource expenditures (25)	.310	.719
OAA recipients (15)	.041	.709
Percent of Total Variance	35.4	34.9

* The parenthesized numbers correspond to the variable numbers in Table 3.

TABLE 5. LOADINGS OF SOCIOECONOMIC VARIABLES ON TWO PRINCIPAL FACTORS: 1960*

Industrialization		Affluence	
value added by manufacture/capita	+.907	median school years completed	+.909
percentage employed in manufacturing	.877	estimated valued of real property/capita	.792
value/acre of farm land and buildings	.831	personal income/capita	.730
population/square mile	.775	motor vehicle registration/1,000 population	.703
percent foreign	.703	telephones/1,000 population	.675
total population	.672	percent increase in population	.551
percent urban	.657	percent urban	.522
telephones/1,000 population	.650	acreage per farm	.488
average no. of employees per manufacturing establishment	.638	divorce rate	.430
personal income/capita	.573	percent failures business+commercial establishments	.287
percent failures business+commercial establishments	.421	percent housing owner occupied	.240
estimated value real property/capita	.132	percent foreign	.230
percent Negro	.066	total population	.045
percent illiterate	.039	value per acre of farm land and buildings	.024
		value added by manufacture/capita	.015
		population/square mile	.008
percent increase in population	.006	percentage employed in manufacturing	.132
median school years completed	.026	average no. of employees per manufacturing establishment	.351
percent farms operated by tenants	.266	percent farms operated by tenants	.468
percent housing owner occupied	.316	percent illiterate	.737
divorce rate	.325	percent Negro	−.752
acreage per farm	.503		
motor vehicle registration/1,000 population	−.568		
percent of total variance	32.4		25.3

* Source: Hofferbert, "Socioeconomic Dimensions of the American States," *Midwest Journal of Political Science*, August, 1968.

cording to the variables that load highest on each one.[23] In this article, these factors are used to assess the socioeconomic conditions that support various aspects of state politics and public service and to test the interaction between the major dimensions of state economics, politics, and policy.

III. ECONOMIC-POLITICAL-POLICY RELATIONSHIPS: A COMPARISON OF FACTOR VERSUS INDIVIDUAL VARIABLE RELATIONSHIPS

One controversy that is clarified by the use of factor scores concerns relationships among political and economic characteristics of the states that are alleged to influence the nature of public policies. Several publications employ individual variables to answer the question raised in our introduction, i.e., how do political and economic elements relate with each other and with the nature of public policies? Although most authors use comparable measurements and attempt to make their research additive with respect to each others', the interpretation of their findings is made difficult by the discreteness of the variables examined. The correlations reported by Dye, for example, often show differing policy relationships with "industrialization" and "urbanization" than with "income" and "education."[24] The findings claimed when the "political" measures assess a certain type of voter turnout or party competition differ from the findings derived from "political" measures that assess the equity of apportionment or the centralization of state-local financial relationships. Likewise, findings vary with the use of expenditures or other

[23] *Ibid.* The label given to one of the socioeconomic factors is new with this article. In the original article "Affluence" was labelled "Cultural Enrichment," the present term seems more appropriate as a description of the contents of this factor.

[24] See, for example, Dye, *op. cit.*, p. 125 and p. 162.

TABLE 6. FACTOR SCORES FOR THE STATES ON TWO POLITICAL AND TWO POLICY FACTORS

State	Professionalism—local-reliance	Competition—turnout	Welfare—education	Highway—natural resources
Alabama	− .399	−2.504	−1.753	− .405
Arizona	− .005	− .141	− .266	.175
Arkansas	− .827	−1.360	−1.263	.056
California	2.140	.363	1.849	− .345
Colorado	− .255	.679	.711	.473
Connecticut	.200	.431	.846	−.845
Delaware	.116	.192	− .390	− .779
Florida	.260	− .560	− .875	− .589
Georgia	.087	−2.516	−1.452	− .535
Idaho	−1.052	1.266	− .045	1.583
Illinois	1.154	1.034	1.258	− .877
Indiana	.036	1.152	.155	− .399
Iowa	− .163	.326	.845	− .059
Kansas	− .159	.359	.852	− .082
Kentucky	− .661	− .351	−1.041	− .420
Louisiana	.031	−1.152	− .729	− .361
Maine	− .619	.045	− .465	.344
Maryland	.500	− .377	− .155	− .787
Massachusetts	1.651	.071	1.446	−1.363
Michigan	1.354	.627	.761	− .578
Minnesota	.417	1.339	1.316	.594
Mississippi	− .210	−1.770	−2.432	− .191
Missouri	.126	.305	− .378	− .298
Montana	−1.095	1.235	− .063	1.847
Nebraska	− .297	1.494	.705	.337
Nevada	− .221	− .170	− .535	2.866
New Hampshire	− .752	.191	1.035	.183
New Jersey	1.010	.843	1.035	−1.115
New Mexico	− .734	− .615	.184	.269
New York	4.230	− .643	1.152	−1.079
North Carolina	− .161	− .198	−1.079	− .857
North Dakota	− .869	.780	.901	.614
Ohio	.619	.226	.284	− .756
Oklahoma	− .799	− .293	.785	− .262
Oregon	− .207	.913	.735	.656
Pennsylvania	1.648	.192	.085	− .769
Rhode Island	− .328	.166	.640	−1.144
South Carolina	− .013	−2.838	−1.580	− .808
South Dakota	− .976	− .138	− .072	1.546
Tennessee	− .582	− .556	−1.738	− .169
Texas	.289	− .566	− .808	− .493
Utah	−1.032	1.629	.124	.688
Vermont	−1.546	− .483	− .615	1.313
Virginia	− .243	− .964	−1.062	− .645
Washington	− .160	.694	.789	.063
West Virginia	− .739	.281	−1.360	− .422
Wisconsin	.558	.949	1.445	.095
Wyoming	−1.421	.413	.220	3.728

variables that purport to measure public policy.[25]

At this point it is possible to face the issue of political or economic influences on public policies with some important new tools. The six factors measure prominent dimensions of state economics, politics, and public policies. They should enable us to see which types of economic and political phenomena are most relevant for which types of public policy. The statistics employed in this section are commonly used in the analysis of policy determinants: coefficients of simple, partial, and multiple correlation. The innovation here is in the variables. Instead of individual measures, the variables are represented by the factor scores reported in Table 6.

There is a strong association between Competition-Turnout and the Affluence dimension of state economics. (See Table 7.) Evidently, citizens are more likely to participate in politics and parties are most likely to compete with one another where the residents are well-educated and relatively wealthy. Of course, this finding is not new. However, it is a very important relationship in the political and economic systems of American states that is highlighted by the use of factor analysis.[26]

Another strong relationship is that between the Professionalism-Local Reliance dimension of

TABLE 7. COEFFICIENTS OF SIMPLE CORRELATION BETWEEN SOCIOECONOMIC AND POLITICAL FACTORS

Political Factors	Socioeconomic Factors	
	Industrialization	Affluence
Professionalism—Local Reliance	.73	.14
Competition-Turnout	.11	.66

[25] See Sharkansky, *Spending in the American States*, Chapters 4 and 7.

[26] The strength of relationships between the political and economic factors is stronger than that customarily obtained with individual variables. The correlations that we have described are .73 and .66. Only two out of the forty-eight simple correlations between economic and political variables that Dye reports in his study reach the level of .66 and none reach .73 (see his Chapter 3). The underlying dimensions that are tapped by factor analysis appear to be more salient representations of the economic-political nexus than are apparent in studies using single variables.

state politics and the Industrialization dimension of state economics. It is apparently the value of industrial output and the incidence of industrial employment—instead of education, wealth, or ethnicity—that has the most to do with the development of legislative and judicial professionalization and the tendency of local authorities to rely on their own economic resources. It may be that a large industrial tax base and an urbanized population permits governments to rely on locally-raised revenues. An industrial economy may also encourage states to develop professional, active policy making institutions in response to comparable models in the private sector.

The Welfare-Education dimension of state policy shows positive relationships with each of the political and economic factors. (See Table 8.) The coefficients of partial correlation reveal that this policy factor is associated most closely with high scores on Competition-Turnout and Affluence, with neither being significantly more important than the other. The elements of interparty competition, high voter participation, high levels of educational attainment, and wealth seem to provide the impetus—independently from other governmental and economic conditions—for public officials to provide generous welfare payments and successful educational services.[27]

[27] The political and economic factors are more successful in accounting for interstate variance in the policy factors than are most individual measures of these phenomena. Table 8 shows simple, partial, and multiple correlation coefficients between policy factors as dependent variables and the political and economic factors as independent variables. It shows that both political factors together account for 61 and 36 percent of the variance in the policy factors. In contrast, the multiple correlation coefficients that Dye reports show that his collection of individual political variables account for 36 percent of the variance in only fifteeen out of fifty-four dependent variables, and they account for 61 percent of the variance in only two out of the fifty-four instances. (See Dye, *op. cit.*, pp. 286–287.) Both of our socioeconomic factors account for 59 and 68 percent of the variance in the policy factors. In contrast, Dye's collection of individual social and economic variables account for 59 percent of the variance in only fourteen of fifty-four dependent variables, and they account for 68 percent of the variance in only eight of fifty-four dependent variables. A similar finding appears when we compare with his data the success of our economic and political factors together in explaining variance in our policy factors. We explain 69 and 70 percent of the variance in the policy factors by means of all political and socioeconomic variables: variables Dye explains 69 percent of the variance for only thirteen of fifty-four dependent variables,

TABLE 8. COEFFICIENTS OF SIMPLE, PARTIAL, AND MULTIPLE CORRELATION, AND MULTIPLE DETERMINATION BETWEEN SOCIOECONOMIC, POLITICAL, AND POLICY FACTORS

	Simple Correlation		Partial Correlation*	
	Welfare—Education	Highways—Natural Resources	Welfare—Education	Highways—Natural Resources
Professionalism—Local Reliance	.39	−.54	.26	−.24
Competition—Turnout	.68	.25	.47	−.02
Industrialization	.37	−.69	.17	−.55
Affluence	.69	.43	.43	.53
	Multiple Correlation		Multiple Determination	
Socioeconomic Factors	.77	.82	.59	.68
Political Factors	.78	.60	.61	.36
Socioeconomic + Political Factors	.83	.84	.69	.70

* Controlling for the other factors.

This finding of importance for the Competition-Turnout factor with respect to welfare and education services provides some latter-day support for hypotheses derived from Key and others that electoral and party characteristics of state politics have something to do with the nature of services that are provided.[28] Our findings do not, however, fit the neat, linear, single determinant structure of causality suggested by some of Key's followers. Rather, the data suggest that it is not any one element of state politics, but an underlying factor that is only partly measured by individual variables which exerts an independent influence on public services. The prominence of this finding is increased by the importance of the policy factor involved. Welfare and education services consume a substantial proportion of state and local government resources: 49 percent of general expenditures and 51 percent of government employment in 1965–66. Policy making in welfare and education are characterized by sharp controversies; this may explain their relation with electoral and partisan processes in a manner quite different from other areas of public policy.

The Highway-Natural Resources dimension of public policy shows its primary dependence

[28] See their works cited in note #16, *supra*.

on economic factors. It is inversely and strongly related to Industrialization and directly related to Affluence. Thus it appears that *low* levels of industrial output and employment, population density and urbanization together with *high* levels of personal wealth and education incline a state toward heavy investments in roads (especially in rural areas) and active fish and wildlife programs. Wide-open spaces present an abundance of recreational opportunities, the need for highway facilities, and the rural interests that have concentrated their political efforts in behalf of these services. Highway-Natural Resource policy also is inversely dependent on the Professionalism-Local Reliance dimension of state politics, but this is secondary to its dependence on socioeconomic characteristics.

Why the lack of dependence on political factors for highway and natural resource policies? Despite conflicting interests, the particular programmatic activities involved in these policies do not fall neatly along the symbolic continua which are most commonly the objects of partisan controversy. There seem to be few issues, for example, which array taxpayers and the economic *haves* against recipients of services and the *have nots* (in contrast to the case in education and welfare). This may be a function of highways, parks, wildlife, and conservation programs being "self-financed" by means of earmarked taxes, licenses and user fees. The recipients pay for much of the benefits received, and do not generate a great deal of conflict across the lines of different social classes or ethnic groups.

IV. SUMMARY AND CONCLUSIONS

This article reports two separate factor analyses of state politics and public policies. And together with two socioeconomic factors, it employs the political and policy factors to clarify relationships among three sectors of a widely used model of the state policy process. The major findings are:

1) The factor analysis of political variables reveals two underlying dimensions of state politics. These are Professionalism-Local Reliance and Competition-Turnout. The distinctiveness of the Competition-Turnout factor testifies to the importance of this dimension in distinguishing the politics of each state from the others. And it credits the perception of Key, Lockard, Fenton and other political scientists who took aspects of electoral and party behavior as the focus of their own work.
2) The factor analysis of policy variables reveals two principal dimensions of Welfare-Education and Highway-Natural Resources.
3) The examination of political and economic factors shows strong associations between Competition-Turnout and Affluence, and between Professionalism-Local Reliance and Industrialization.
4) The relationships among political, economic, and policy factors show that the Welfare-Education dimension of state policy is significantly dependent upon the Competition-Turnout dimension of state politics and the Affluence dimension of the state economy. Where a state is wealthy and shows high turnout and intense interparty competition, it is likely to score high on the level of welfare and educational services. The Highway-Natural Resources dimension of policy appears most dependent upon the Industrialization dimension of the states' economies. The relationship is inverse, indicating that well-developed highways and fish-game programs accompany low levels of industrialization and population concentration.
5) The relationships among economic, political, and policy factors are generally stronger than the relationships between individual variables that have previously been used. Thus the underlying dimensions tapped by factor analysis strengthen as well as simplify our explanations of policy making in the American states.

The single most important finding of this article may be its emphasis upon multidimensionality in state economics, politics and public policy. There is no single answer to the question: "Is it politics or economics that has the greatest impact on public policy?" The answer (contrary to the thrust of much recent research) varies with the dimensions of each phenomena that are at issue.[29] Welfare-Education policies relate most closely with the Competition-Turnout dimension of state politics *and* with the Affluence dimension of the economy. Highway-Natural Resource policies show their closest (inverse) relationships with the Industrialization dimension of the state economy.[30]

[29] Charles F. Cnudde and Donald J. McCrone "Party Competition and Welfare Policies in the American States, this REVIEW, 63 (September 1969).

[30] One limitation of the data employed throughout this article is their lack of historical perspective. A separate study of the two socioeconomic factors and their relationship with individual policy

While these findings add to the inquiry into political and economic determinants of public policies, they offer little encouragement to those who would seek to expand the level and scope of public services by manipulating one political or structural characteristic of state government (e.g., voter turnout, party competition, or apportionment). It is apparent only that certain aspects of politics having to do with voter turnout and interparty competition are related to certain public policies. But this does not terminate the inquiry. Factor analyses depend on the nature of the variables included in them. Although this study has included a wide range of political, economic, and policy variables in its factor analyses, it is likely that further analysis —with more measures of political, economic, and policy traits—will permit the extraction of additional dimensions from the sectors of state policy systems.

The present factors leave unexplained much of the variance in state politics and policy.[31] This unplumbed variance might be reduced by adding more variables to the factor programs. However, it may be inherent in the nature of the phenomena that much variance will remain unexplained by factor analysis. This unexplained variance may reflect the importance of particularistic, unpatterned happenings that fashion and lend excitement to political institutions. The effort to explain why politics and policies differ from one state to the next may be helped considerably by examining the dimensions lying beneath readily measured variables. But no amount of archival search and factor analysis will account for the contributions made to the institutions of policies of individual states by dynamic personalities or the force of strong traditions. The study of elite and organizational behavior, plus exploration of the values that prevail in the cultural environments of the individual states may be essential for a thorough understanding of inter-state differences in politics and public policy.

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and electoral variables has shown that the strength of relationship is, in some cases, quite fluid over time. (See Hofferbert, "Socioeconomic Dimensions . . . ," *op. cit.*) This fluidity, however, may be a result of simple changes in the value of isolated dependent variables and not an accurate picture of the more analytically interesting dimensions of policy and political life which we have been considering here. Hofferbert found that the relative contribution of individual variables maintained a high degree of consistency in their loadings on the principal factors from one decade to the next, although individual variable-by-variable correlations shifted considerably. If it were possible to analyze the full component of political and service variables over extended time, the same type of configuration could probably be expected. Consequently, the strength of relationship of *factors* could be fairly constant over time even though the individual variable correlations changed considerably. Because of limited availability of the political and policy data for previous years, however, this must remain a matter for speculation.

For a useful discussion of the methodological problems of theory construction employing measures standardized for a specific population and time, such as the correlation coefficients used here, versus unstandardized regression coefficients see Hubert Blalock "Causal Inferences, Closed Populations, and Measures of Association," this REVIEW, 61 (March, 1967), 130–136. Blalock argues that in building theoretical statements of more general application, the regression is the most use since it is not as sensitive as the correlation coefficient to specific population values at a given time. However, in an investigation such as ours, where possibility of longitudinal analysis is limited due to the availability of relevant data, it seemed advisable to retain the greater precision of standardized measures. Furthermore, in the case of the states, the relative magnitudes of most of the variables is not particularly fluid as might be the case with some other types of indicators, such as the congressional roll call votes discovered by Blalock.

[31] Although the figures in Table 2 report that the political factors account for 66 percent of the variance, this pertains to the variance in the eleven variables that were subject to this factor analysis. These factors do not account for this much of the variance in the total collection of fifty-three variables that are presented in Table 1.

THE DIFFUSION OF INNOVATIONS AMONG THE AMERICAN STATES*

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We are now in the midst of a notable revival of interest in the politics of the American states. During the last decade many studies have been conducted of the social, political and economic determinants of state policy outcomes.[1] Several of these writers have argued that the relative wealth of a state, its degree of industrialization, and other measures of social and economic development are more important in explaining its level of expenditures than such political factors as the form of legislative apportionment, the amount of party competition, or the degree of voter participation.[2] It has been claimed that such factors as the level of personal income or the size of the urban population are responsible *both* for the degree of participation and party competition in a state, *and* the nature of the system's policy outputs. By making this argument these writers have called into question the concepts of representation and theories of party and group conflict which, in one form or another, are the foundations for much of American political science.[3]

There is a growing awareness, however, that levels of expenditure alone are not an adequate measure of public policy outcomes. Sharkansky has shown, for example, that levels of expenditure and levels of actual service are seldom correlated; presumably, some states are able to reach given service levels with much less expenditure than others.[4] Besides establishing the appropriate level of expenditure for a program, policy makers must also decide about the program's relative scope, provisions for appeal from administrative orders, eligibility requirements, the composition of regulatory boards and commissions, and many other matters which have little to do with money. Before we can evaluate the relative importance of structural and political factors as determinants of policy, therefore, we need to investigate decisions outside the budgetary process. In order to advance that object this study will focus on one of the most fundamental policy decisions of all: whether to initiate a program in the first place.

States have traditionally been judged according to the relative speed with which they have

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[1] Beginning with Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics* (May, 1963), 265–289, there have been numerous articles and books on the subject. The most recent summary is: John H. Fenton and Donald W. Chamberlayne, "The Literature Dealing with the Relationships Between Political Processes, Socio-economic Conditions and Public Policies in the American States: A Bibliographical Essay," *Polity* (Spring, 1969), 388–394.

[2] For examples see: Herbert Jacob, "The Consequences of Malapportionment: A Note of Caution," *Social Forces* (1964), 260–266; the chapters by Robert Salisbury, Robert Friedman, Thomas Dye, and Dawson and Robinson in: Herbert Jacob and Kenneth Vines (eds.), *Politics in the American States: A Comparative Analysis* (Boston, 1965); Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW (March, 1966), 73–82; and Thomas Dye, *Politics, Economics and the Public: Policy Outcomes in the American States* (Chicago, 1966)

[3] For an evaluation of the significance of this literature and its implications for political science see: Robert Salisbury, "The Analysis of Public Policy: A Search for Theories and Roles," in Austin Ranney (ed.), *Political Science and Public Policy* (Chicago, 1968), pp. 151–178.

[4] Ira Sharkansky, "Government Expenditures and Public Services in the American States," this REVIEW (1967), 1066–1077. Sharkansky also identifies important political variables in his: "Economic and Political Correlates of State Government Expenditures: General Tendencies and Deviant Cases," *Midwest Journal of Political Science* (1967), 173–192.

accepted new ideas. Wisconsin, because of its leadership during the Progressive period and its early adoption of the direct primary, the legislative reference bureau, and workmen's compensation, gained a reputation as a pioneering state which it has never lost. Reputations of this kind are usually based only on random impressions and they may be inaccurate or misleading, but if it is true that some states change more readily than others a study of the way states adopt new ideas might lead to some important insights into the whole process of political change and development.

This essay is primarily an exercise in theory building. My aim is to develop propositions which might be used as guides to the study of the diffusion of innovations and which might also apply to budgeting and other forms of decision making.[5] Limitations in the data I have collected do not allow empirical testing of all the explanations I propose; the currently untestable propositions are presented in the hope that they may help in preparing the ground for future research. The study begins with an effort to devise a measure of the relative speed with which states adopt new programs. Once a measure of this phenomenon is created efforts are made to discover its principal demographic and political correlates. The article concludes with an effort to devise an explanation for the adoption of innovations based on insights gathered from studies of decision making, reference group theory, and the diffusion of innovations. The major questions being investigated are: (1) why do some states act as pioneers by adopting new programs more readily than others, and once innovations have been adopted by a few pioneers, (2) how do these new forms of service or regulation spread among the American states?

I. DEFINITIONS AND DISTINCTIONS

Several terms have already been used here which have ambiguous meanings and it is important to make clear just how they are to be defined. The most important, and potentially misleading, is the term "innovation." An innovation will be defined simply as a program or policy which is new to the states adopting it, no matter how old the program may be or how many other states may have adopted it. Even though bureaucratic innovations or new departures by regulatory commissions or courts may be mentioned in the course of the discussion, the data used to measure the relative speed of adoption of innovations consists exclusively of legislative actions, simply because the data was readily available only in that form.

We are studying the relative speed and the spatial patterns of *adoption* of new programs, not their invention or creation. Invention, or bringing into being workable, relevant solutions to pressing problems, is an important activity and has been the subject of fascinating research.[6] We will concentrate on the way in which organizations select from proposed solutions the one which seems most suited to their needs, and how the organizations come to hear about these new ideas in the first place.[7] We are not trying to specify the circumstances under which new ideas or programs will be conceived or developed; we are studying instead the conditions under which state decision makers are most likely to adopt a new program.

The object of this analysis is the process of diffusion of ideas for new services or programs. Sometimes new legislation is virtually copied from other states. The California fair trade law, adopted in 1931, "was followed either verbatim or with minor variations by twenty states; in fact, ten states copied two serious typographical errors in the original California law."[8] No as-

[5] There is a well established body of research on the diffusion of innovations from which I have drawn many insights. For general reviews of this literature see: Everett M. Rogers, *Diffusion of Innovations* (New York, 1962), Elihu Katz, Martin L. Levin, and Herbert Hamilton, "Traditions of Research in the Diffusion of Innovations," *American Sociological Review* (1963), 237–252. For early attempts to study the American states from this perspective see: Ada J. Davis, "The Evolution of the Institution of Mothers' Pensions in the United States," *American Journal of Sociology* (1930), 573–582; Edgar C. McVoy, "Patterns of Diffusion in the United States," *American Sociological Review* (1940), 219–227; and E. H. Sutherland, "The Diffusion of Sexual Psychopath Laws," *American Journal of Sociology* (1950–51), 144–156. Also see: Torsten Hagerstrand, *Innovation Diffusion as a Spatial Process* (Chicago, 1967); and Robert Mason and Albert N. Halter, "The Application of a System of Simultaneous Equations to an Innovation Diffusion Model," *Social Forces* (1968), 182–193.

[6] For examples see: Gary A. Steiner (ed.), *The Creative Organization* (Chicago, 1965); and Tom Burns and G. M. Stalker, *The Management of Innovation* (London, 1961).

[7] There is much confusion over this distinction in the literature on diffusion. For an excellent discussion of the problem see: Lawrence B. Mohr, "Determinants of Innovation in Organizations," this REVIEW (1969), 111–126.

[8] Once the mistake was discovered, the Arkansas statute, which reproduced a model prepared by the National Association of Retail Druggists, was

sumption is being made, however, that the programs enacted in each state are always exactly alike or that new legislation is written in exactly the same way by every legislature. It is unlikely that the highway department established in Wisconsin in 1907 had the same organizational format as the one adopted by Wyoming in 1917, or that the council on the performing arts created in New York in 1960 bears an exact resemblance to the one created by Kentucky in 1966. In each case, however, a commitment was made to offer a new service, establish a new principle of regulation, or create an agency which had never existed before. Our concern is the origin and spread of the idea to provide public subsidies for the arts, not the detailed characteristics of institutions created in each state to implement the policy.

No ideological bias was employed in selecting issues for study. The patterns of diffusion for each issue have been treated equally, and no effort was made to develop any method of determining the relative importance or desirability of the programs.[9] Programs are sometimes enacted only to provide symbolic rewards to groups within the population and once created are left with inadequate funds or otherwise disabled.[10] Oklahoma's legislature, for example, emulated other states by creating a state civil rights commission, but once the commission was established, only $2,500 was appropriated for its operation.[11] For the purposes of this study, however, all adoptions are equal. My goal is to provide an explanation of the relative speed of adoption and the patterns of diffusion of innovations; I am not interested in the effectiveness of Oklahoma's civil rights commission, but in where the legislature got the idea to create such a commission and why it acted when it did.

II. THE INNOVATION SCORE

My first aim is to explain why some states adopt innovations more readily than others. I assume that the pioneering states gain their reputations because of the speed with which they accept new programs. The study must begin, therefore, with an attempt to devise an innovation score that will represent the relative speed with which states adopt innovations.

The innovation score is based on the analysis of eighty-eight different programs (see the Appendix for a list) which were enacted by at least twenty state legislatures prior to 1965, and for which there was reliable information on the dates of adoption. In order to make the collection of programs as comprehensive and representative as possible, I adopted a list of basic issue areas similar to the one employed by the Council of State Governments in its bi-annual reports included in the *Book of the States*. I tried to study six to eight different pieces of legislation in each of these areas: welfare, health, education, conservation, planning, administrative organization, highways, civil rights, corrections and police, labor, taxes, and professional regulation. In the course of my analysis I studied issues ranging from the establishment of highway departments and the enactment of civil rights bills to the creation of state councils on the performing arts and the passage of sexual psychopath laws. Most of the programs were adopted during the twentith century, but sixteen of them diffused primarily during the latter half of the nineteenth century.

Once the eighty-eight lists of dates of adoption were collected they were used to create an innovation score for each state. The first step was to count the total number of years which elapsed between the first and last recorded legislative enactment of a program. Each state then received a number for each list which corresponded to the percentage of time which elapsed between the first adoption and its own acceptance of the program. For example, if the total time elapsing between the first and last adoption of a program was twenty years, and Massachusetts enacted the program ten years after the first adoption, then Massachusetts received a score of .500 on that particular issue. The first state to adopt the program received a score of .000 and the last state received a 1.000. In cases in which all the states have not yet adopted a program, the states without the program were placed last and given a score of 1.000.[12] The in-

copied either verbatim or with minor changes by seventeen states. Ewald T. Grether, *Price Control Under Fair Trade Legislation* (New York, 1937), pp. 19–20.

[9] In later work I will report the results of comparisons of the diffusion patterns of issues from different subject matter areas. Preliminary efforts at such comparisons, however, have not revealed significant variations. There does not seem to be much difference in the diffusion patterns of issues of different types.

[10] For a discussion of this phenomenon see: Murray Edelman, *The Symbolic Uses of Politics* (Urbana, 1964), chapters 2 and 9.

[11] Duane Lockard, *Toward Equal Opportunity* (New York, 1968), p. 23.

[12] The beginning point for the existence of each state was the date upon which it was officially organized as a territory. Using this system, Oklahoma is the last state to come into being, having been organized in 1890. If a program began its diffusion before a state came into existence, tha

novation score for each state is simply 1.000 minus the average of the sum of the state's scores on all issues. The larger the innovation score, therefore, the faster the state has been, on the average, in responding to new ideas or policies. The issues may be divided into groups according to subject matter areas or time periods, and separate scores can be created for these smaller groupings of issues by following the same procedure. The results of this scoring procedure, using all eighty-eight issues, are presented in Table 1.

A note of caution should be sounded before the results of this exercise are analyzed. We are endeavoring to measure a highly complex process in which an enormous number of idiosyncratic influences are at work; an official with an unusually keen interest in a particular program, a chance reading of an article or book by a governor's aide, or any number of other circumstances peculiar to any one issue might lead to the rapid adoption of a piece of legislation by a state which is usually reluctant to accept new programs. Mississippi, which has the lowest average score and ranks last among the states in relative speed of adoption, was nonetheless the first state to adopt a general sales tax.

If this reservation is kept in mind, the data in Table I provide a crude outline of the standard or typical pattern of diffusion of new programs or policies among the American states. The states at the top of the list tend to adopt new programs much more rapidly than those at the bottom of the list. Having provided a preliminary measurement of this phenomenon, we must now try to explain it. Why should New York, California and Michigan adopt innovations more rapidly than Mississippi, Wyoming and South Dakota?

III. THE CORRELATES OF INNOVATION

Demographic Factors: After studying the acceptance of technological innovations by both individuals and organizations, several writers have concluded that the decision maker's relative wealth, or the degree to which "free floating" resources are available, are important determinants of the willingness to adopt new techniques or policies.[14] If "slack" resources are available, either in the form of money or a highly skilled, professional staff, the decision maker can afford the luxury of experiment and can more easily risk the possibility of failure.[15] Other studies, especially in the areas of agriculture and medicine, have also shown organizational size to be a strong correlate of innovation.[16] Given these results from prior studies in other fields we might expect to find

issue was not included in figuring the innovation score for the state.

[13] Alaska and Hawaii were omitted from the analysis because data for their years of adoption were often missing.

[14] Everett M. Rogers, *Diffusion of Innovations* (New York, 1962), pp. 40, 285–292. Also see: S. N. Eisenstadt, *The Political Systems of Empires* (New York, 1963), p. 27, 33–112.

[15] For a discussion of "slack" resources and innovation see: Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J., 1963), pp. 278–279.

[16] Rogers, *op. cit.*, Mohr, *op. cit.*; and also: Edwin Mansfield, "The Speed of Response of Firms to New Techniques," *Quarterly Journal of Economics* (1963), 293–304; Jerald Hage and Michael Aiken, "Program Change and Organizational Prop-

TABLE 1. COMPOSITE INNOVATION SCORES FOR THE AMERICAN STATES[13]

New York	.656	New Hampshire	.482	Idaho	.394
Massachusetts	.629	Indiana	.464	Tennessee	.389
California	.604	Louisiana	.459	West Virginia	.386
New Jersey	.585	Maine	.455	Arizona	.384
Michigan	.578	Virginia	.451	Georgia	.381
Connecticut	.568	Utah	.447	Montana	.378
Pennsylvania	.560	North Dakota	.444	Missouri	.377
Oregon	.544	North Carolina	.430	Delaware	.376
Colorado	.538	Kansas	.426	New Mexico	.375
Wisconsin	.532	Nebraska	.425	Oklahoma	.368
Ohio	.528	Kentucky	.419	South Dakota	.363
Minnesota	.525	Vermont	.414	Texas	.362
Illinois	.521	Iowa	.413	South Carolina	.347
Washington	.510	Alabama	.406	Wyoming	.346
Rhode Island	.503	Florida	.397	Nevada	.323
Maryland	.482	Arkansas	.394	Mississippi	.298

that the larger, wealthier states, those with the most developed industrial economies and the largest cities, would have the highest innovation scores. It would seem likely that the great cosmopolitan centers in the country, the places where most of the society's creative resources are concentrated, would be the most adaptive and sympathetic to change, and thus the first to adopt new programs.

In order to test these assumptions several measures of social and economic development were correlated with the innovation score. As we can see in Table 2, there is evidence that the larger, wealthier, more industrialized states adopt new programs somewhat more rapidly than their smaller, less well-developed neighbors. Fairly strong relationships exist between the innovation score and the value added by manufacturing, the average per acre value of farms, the size of the urban population, and the average per capita income. These relationships remain virtually unchanged in all time periods. In fact, the only relationship which changes substantially over time is that between innovation and the percentage of illiterates in the population which declines steadily across the three time periods. This declining relationship and the low correlation between innovation and the median school year completed is caused primarily by the states in the Rocky Mountain region which have the highest rankings on median school years completed and yet are among the slowest to adopt new programs.[17] The median of educational attainment in the states with the highest innovation scores is pulled down by the presence of a large, poorly educated, lower class, living primarily in the inner cities. The highly industrialized states with large urban concentrations are characterized by great inequality of social status and attainment. It would seem, however, that the elements necessary to foster innovation are present in these states even though they do not have highest average level of educational achievement.

Political Factors: Although students of policy

TABLE 2. CORRELATIONS BETWEEN INNOVATION SCORES AND FIVE SOCIAL AND ECONOMIC VARIABLES, BY TIME PERIODS

Social-Economic Variables	Innovation Scores*			Composite Score
	1870–1899	1900–1929	1930–1966	
Per Cent Population Urban:	.62**	.69	.62	.63
Total Population:	.52	.40	.50	.59
Average Income, Per Capita:	—***	.62	.50	.55
Value Added Per Capita by Manufacturing	.46	.55	.57	.66
Average Value, Per Acre, of Farms:	.70	.52	.52	.54
Per Cent Population Illiterate:	−.58	−.44	−.12	−.23
Median School Years Completed:	—***	—***	.24	.26

* In order to insure that the innovation score and the social and economic variables came from comparable periods, separate innovation scores were calculated for three time periods: 1870–1899, 1900–1929, and 1930-1966. In constructing this table each innovation was placed in the time period during which the first ten states adopted it. Thus, if a program was adopted by only four states during the 1890's, and completed its diffusion during the 1900's, the program is placed in the second time period: 1900–1929, even though its first adoptions took place during the nineteenth century. Social and economic data are taken from the years 1900, 1930, and 1960. The composite score is correlated with social and economic data from 1960.

** The table entries are Pearson product-moment correlations.

*** Measures of these phenomena corresponding with these time periods do not exist.

pret, but will be treated more fully in later work. For an example of this problem discussed in another context see: Raymond Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," this REVIEW (1966), 306–326. For a more extensive discussion of the methodological implications see the discussion of "interaction effects" in Hugh Donald Forbes and Edward R. Tufte, "A Note of Caution in Causal Modelling," this REVIEW (1968), pp. 1261–1262; and the communication from Dennis D. Riley and Jack L. Walker, this REVIEW (September, 1969), pp. 880–899.

erties: A Comparative Analysis," *American Journal of Sociology* (1967), 516–517; and Richard J. Hall, S. Eugene Haas, and Norman J. Johnson, "Organizational Size, Complexity and Formalization," *American Sociological Review* (1967), 903–912.

[17] Regional affects of this kind appear frequently in analyses of data from the American states. In many studies, especially those which involve measures of political participation or party competition, strong relationships appear which are actually only a result of the distinctive nature of the southern states. In order to insure that the correlations in this analysis were not merely a result of the social and political peculiarities of the South, the eleven states of the confederacy were removed from all distributions. Since the Southern states do not cluster at one extreme of the innovation scale, no great changes occurred in correlation coefficients based upon data from the thirty-nine states outside the South. Within the eleven Southern states, however, almost all the relationships were substantially reduced in size. Because only eleven states are involved, this fact is difficult to inter-

making have begun to doubt the importance of the political system as an independent determinant of the behavior of decision makers, it seems likely that both the degree of party competition and a state's system of legislative apportionment would affect its readiness to accept change. It would seem that parties which often faced closely contested elections would try to out-do each other by embracing the newest, most progressive programs and this would naturally encourage the rapid adoption of innovations. Lowi argues that new departures in policy are more likely at the beginning of a new administration, especially when a former minority party gains control of the government.[18] If this tendency exists it would also seem likely that state political systems which allow frequent turnover and offer the most opportunities to capture high office would more often develop the circumstances in which new programs might be adopted.[19]

Another prerequisite for the rapid adoption of new programs might be a system of legislative apportionment which fully represented the state's urban areas and which did not grant veto power to groups opposed to change. Such a system might be expected to allow consideration and debate of new policies and programs in all areas. Some recent findings, such as Barber's study of legislators in Connecticut,[20] lead us to speculate that representatives from newly developing urban and suburban areas would be more cosmopolitan, better informed, and more tolerant of change. If nothing else, urban legislators would probably be more willing to deal with problems of sanitation, planning, transportation, and housing peculiar to large metropolitan areas.

TABLE 3. CORRELATIONS BETWEEN INNOVATION SCORES AND MEASURES OF POLITICAL VARIABLES, BY TIME PERIODS

Political Variables*	Innovation Scores			Composite Score
	1870–1899	1900–1929	1930–1966	
Party Competition for Governorship:	.36	.02	.14	.24
David-Eisenberg Index of Malapportionment:	**	.07	.55	.65

* The Index of party competition used in this table is the per cent of the total vote going to the gubernatorial candidate coming in second, times 2. This yields a scale from 0 to 100. It was created by Richard Hofferbert. The apportionment Index appears in Paul T. David and Ralph Eisenberg, *Devaluation of the Urban and Suburban Vote* (Charlottesville: Bureau of Public Administration, University of Virginia, 1961).

** Measures of this phenomenon corresponding with this time period do not exist.

No matter what the composition of the legislator's constituency, however, it would seem that the presence of competent staff, superior clerical facilities, and supporting services would allow him to give serious consideration to a larger number of new proposals. Several studies of the diffusion of technological innovations have demonstrated that the best informed individuals are most likely to pioneer in the use of new techniques or tools,[21] and so the states which provide the most extensive staff and research facilities in their legislatures ought to pioneer in the adoption of new programs.[22]

In Table 3 efforts to test some of these hypotheses in different time periods are displayed. Measures of political variables are usually based on evidence only from contemporary periods because data are seldom available on state and local elections or the operation of legislatures in earlier decades. Measures are available, however, for the degree of party competition and the ex-

[18] Theodore Lowi, "Toward Functionalism in Political Science: The Case of Innovation in Party Systems," this REVIEW (1963), 570–583. Evidence which seems to confirm Lowi's theory may be found in: Charles W. Wiggens, "Party Politics in the Iowa Legislature," *Midwest Journal of Political Science* (1967), 60–69; and Frank M. Bryan, "The Metamorphosis of a Rural Legislature," *Polity* (1968), 191–212.

[19] Joseph A. Schlesinger has developed an index of the "general opportunity level" in each state. The index measures the relative number of chances which exist in each state to achieve major political office. See: *Ambition and Politics: Political Careers in the United States* (Chicago, 1966), pp. 37–56.

[20] James D. Barber, *The Lawmakers: Recruitment and Adaptation to Legislative Life* (New Haven, 1965). For testimony from legislators about the importance of reapportionment see: Frank M. Bryan, "Who is Legislating," *National Civic Review* (December, 1967), 627–633; Allan Dines, "A Reapportioned State," *National Civic Review* (February, 1966), 70–74, 99.

[21] Rogers, *op. cit.* Also see: Mansfield, *op. cit.*; James S. Coleman, Elihu Katz, and Herbert Menzel, *Medical Innovation: A Diffusion Study* (Indianapolis, 1966); and John W. Loy, Jr., "Social Psychological Characteristics of Innovators," *American Sociological Review* (1969), 73–82.

[22] For a somewhat different view see: Norman Meller, "Legislative Staff Services: Toxin, Specific, or Placebo for the Legislature's Ills," *The Western Political Quarterly* (June, 1967), 381–389.

tent of legislative malapportionment.[23] As we can see in Table 3 party competitiveness does not seem to be consistently related to the innovation score, at least as it is measured here.[24] Legislative apportionment is not correlated with the innovation score in the 1900–1929 period, but is related in the 1930–1966 period. Since legislatures steadily became less representative of urban populations after 1930, it may be that we have here some empirical evidence of the impact of malapportionment on policy making in the states.

Recent studies of state expenditures have shown that the explanatory effects of political variables could be eliminated if statistical controls for social and economic variables were applied. Therefore, in Table 4 I have presented both the zero-order correlations of the composite innovation score with measures of party competition, turnover in office, legislative apportionment, and legislative professionalism,[25] and

[23] There is one other index in existence which deals with political phenomenon: Rodney Mott's Index of Judicial Prestige. The Mott index measures the degree to which state supreme courts were used as models by the legal profession. It is based on a study of citations in federal Supreme Court decisions and all state supreme court decisions, the number of cases reprinted in standard textbooks, and the opinion of a panel of prominent legal scholars; it covers the period 1900 to 1930. The Mott index and the innovation score from the same time period are correlated at .62. This finding might be interpreted to mean that emulative behavior in the judicial arena is not much different from that in the legislative arena. For details of the Judicial Prestige Index see: Rodney L. Mott, "Judicial Influence," this REVIEW (1936), 295–315.

[24] Data for this table was derived from Richard Hofferbert's collection, "American State Socioeconomic, Electoral, and Policy Data: 1890–1960" which he has graciously allowed me to use.

[25] The sources are: Richard Hofferbert, "Classification of American State Party Systems," *Journal of Politics* (1964), 550–567; Dennis Riley and Jack L. Walker, "Problems of Measurement and Inference in the Study of the American States" (Paper delivered at the Institute of Public Policy Studies, University of Michigan, 1968); David and Eisenberg, *op. cit.;* Glendon Shubert and Charles Press, "Measuring Malapportionment," this REVIEW (1964), 302–327, and corrections, 968–970; Schlesinger, *op. cit.;* and John Grumm, "Structure and Policy in the Legislature," (Paper presented at the Southwestern Social Science Association Meetings, 1967).

TABLE 4. RELATIONSHIPS BETWEEN THE COMPOSITE INNOVATION SCORE AND MEASURES OF LEGISLATIVE APPORTIONMENT AND PARTY COMPETITION

		Partials				
	Zero-Order	Value Added Manufacturing	Per Cent Urban	Total Population	Per Capita Income	Four Factors Combined
Apportionment						
David-Eisenberg Index	.65	.47	.64	.67	.60	.58
Schubert-Press Index	.26	.12	.34	.31	.26	.21
Party Competition						
Hofferbert Index	.54	.35	.34	.50	.26	.12
Riley-Walker Index —Gov.	.40	.33	.22	.47	.09	.17
Riley-Walker Index —Legis.	.31	.24	.17	.34	.04	.07
Turnover in Office						
Schlesinger Index of Opportunity	.53	.40	.39	.32	.34	.24
Legislative Services						
Grumm's Index of Legislative Professionalism	.63	.38	.33	.41	.51	.11

also partial correlations with four social and economic variables controlled. The control variables are value added by manufacturing, per cent urban population, total population size, and per capita personal income, all of which earlier proved to be independently related to the innovation score. In Table 4 the effect of each control variable is displayed separately along with the combined impact of all four. The results tend to corroborate earlier analyses which minimize the independent effects of these political variables on policy outcomes. The Schlesinger index of opportunity, which measures the difference among the states in the average number of times major offices have changed hands, and the Hofferbert index of inter-party competition seem to have some independent impact on innovation, although it is greatly weakened when all four control variables are combined. This finding lends some credence to Lowi's argument that turnover in office fosters change.

Certainly, the most important result depicted in this table is the consistent strength of the correlation between innovation and the David and Eisenberg index of urban representation.[26] Earlier studies, using expenditures as a measure of policy outcomes, have consistently found that apportionment has little importance as an explanatory variable.[27] Our findings indicate that apportionment does make a difference where innovation is concerned. Although the other political factors do not have great independent impact on innovation, the clear implication arising from Tables 3 and 4 is that those states which grant their urban areas full representation in the legislature seem to adopt new ideas more rapidly, on the average, than states which discriminate against their cities.

Given the results of this correlational analysis, we might conclude that New York, California and Michigan adopt new programs more rapidly than Mississippi, Wyoming, and South Dakota primarily because they are bigger, richer, more urban, more industrial, have more fluidity and turnover in their political systems, and have legislatures which more adequately represent their cities. Although these findings are important, they leave many important questions unanswered. The political system does not react automatically in response to the growth of manufacturing industries or to the increase in the percentage of the population living in cities. Developments of this kind obviously cause problems which public officials might try to solve, but the mere presence of such a stimulant does not cause public officials to act, nor does it determine the form the solution will take, or which state might act first to meet the problem. Our analysis has provided us with evidence that change and experimentation are more readily accepted in the industrialized, urban, cosmopolitan centers of the country, but we have not improved our understanding of the institutions and decision-making processes which cause strong statistical relationships between industrial output and innovation. Also, we have not explained the way innovations spread from the pioneering states to those with lower innovation scores. In order to develop explanations of these processes we must go beyond the search for demographic correlates of innovation and develop generalizations which refer to the behavior of the men who actually make the choices in which we are interested.

IV. POLITICAL SCIENCE AND INNOVATION

In one form or another, interest group theories, based on self-regulating systems of countervailing power, are at the heart of much of the recent research into American politics.[28] Studies of the legislative process in the United States, for example, have been strongly influenced by theories which emphasize the importance of the

[26] Although much simpler than the Schubert and Press measure, the David and Eisenberg index seems to have more relevance to political outcomes. Thomas Dye had the same experience. See Dye, *op. cit.*, pp. 19–20, 63–69, 112–114, 146–148, 174–177, 236–237, 270–281.

[27] Herbert Jacob, "The Consequences of Malapportionment: A Note of Caution," *Social Forces* (1964), 260–266; Thomas R. Dye, "Malapportionment and Public Policy in the States," *Journal of Politics* (1965), 586–601; Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW (1966), 73–82; David Brady and Douglas Edmonds, "One Man, One Vote—So What?" *Trans-action* (March, 1967), 41–46. A recent article calls some of the conclusions of this research into question: Alan G. Pulsipher and James L. Weatherby, Jr., "Malapportionment, Party Competition, and the Functional Distribution of Governmental Expenditures," this REVIEW (1968), 1207–1219.

[28] Examples of this general approach to policy making are: David B. Truman, *The Governmental Process* (New York, 1960); Edward Banfield, *Political Influence* (New York, 1961); and Richard E. Neustadt, *Presidential Power* (New York, 1960). For an excellent critique of theories which employ concepts of power as a major explanatory variable see: James G. March, "The Power of Power," in David Easton (ed.), *Varieties of Political Theory* (Englewood Cliffs, 1966), pp. 39–70.

group basis of politics. Beginning with the efforts of A. Lawrence Lowell[29] political scientists have worked to discover the basic factions within the legislature and have striven to develop operational definitions of power or influence.[30] Extensive efforts have been made to isolate and measure the various influences which come to bear on the individual legislator and motivate him to join one or another legislative bloc: what is a legislator's most important source of cues; is it a lobbyist with whom he has close connections, his party leaders, members of his constituency, the governor, or members of his own family? What impact on his attitudes does the legislative institution itself have; do its informal rules and traditions affect the legislator's decisions, and if so, in what way?[31] Great emphasis has been placed on the analysis of roll-call votes and several sophisticated research techniques have been developed to pursue this work, ranging from Beyle's cluster bloc analysis and Guttman scaling to the more complex, computerized routines presently in use.[32] But all this machinery is useful only in studying those roll-calls which cause divisions in the house; all unanimous votes, nearly eighty per cent of the total in most legislatures, are ignored. Riker has devised a technique in which he uses the percentage of the total membership which is present for the vote and the closeness of the division to determine the relative significance of roll-call votes in legislatures. The more legislators present and the closer the vote, the more significant the issue involved.[33] The full attention of the researcher is thus focused on the relatively small number of decisions which cause significant disagreements, because it is assumed that these are the most important votes; at least, they are the only ones which will provide clues to "the conflicting forces and pressures at work in the legislative system,"[34] and the discovery of those forces and pressures, according to the group theory of politics, is the principal object of political science.

One of the main purposes in this study is to develop an approach to governmental policy making which will serve as a guide in the analysis of *all* legislative decisions, the unanimous as well as the contested ones, and which will lead as well to a better understanding of decisions made by bureaucrats, political executives and other governmental officials. Rather than focus upon the patterns of conflict among factions within the legislature or the administrative agencies, I will search for the criteria employed by legislators and administrators in deciding whether a proposal is worthy of consideration in the first place. This search rests on the belief that whoever the decision maker may be, whether administrator, lobbyist, party leader, governor or legislator, and however controversial a particular issue may become, a set of general criteria exists in every state which establishes broad guidelines for policy making. Regardless of the interests supporting an innovation, no matter whether the decision system is primarily monolithic or pluralistic, if a proposal for change does not fall within those guidelines its chances for acceptance are slim. Many of the propositions I will develop cannot be verified until they are tested with evidence from individual decision makers;[35] they are presented here only as a first, tentative step toward a more comprehensive theory of governmental policy making.

V. EMULATION AND DECISION MAKING IN THE STATES

We are searching for answers to three major questions: 1) why do certain states consistently adopt new programs more rapidly than other states, 2) are there more or less stable patterns of diffusion of innovations among the American states, and 3) if so, what are they? Our answers to these questions will be founded, in part, on the theories of organizational decision making developed in recent years by writers like Simon, March, Cyert and Lindblom.[36] At the

[29] A. Lawrence Lowell, "The Influence of Party Upon Legislation," *Annual Report of the American Historical Association* (1901), pp. 321-543.

[30] The best example is: Robert Dahl, "The Concept of Power," *Behavioral Science* (1957), pp. 201-215.

[31] For the best general review of the results of research on the legislative process, see: Malcolm E. Jewell and Samuel C. Patterson, *The Legislative Process in the United States* (New York, 1966).

[32] For a discussion of these techniques see: Lee F. Anderson, Meridith W. Watts, Jr. and Allen R. Wilcox, *Legislative Roll-Call Analysis* (Evanston, 1966). Also see Jewell and Patterson, *op. cit.*, pp. 528-550.

[33] William H. Riker, "A Method for Determining the Significance of Roll Calls in Voting Bodies," in John C. Wahlke and Heinz Eulau (eds.), *Legislative Behavior* (Glencoe, 1959), pp. 337-383.

[34] Jewell and Patterson, *op. cit.*, p. 416.

[35] Thanks to a grant from the Carnegie Corporation I have been able to launch a pilot study involving interviews in several states.

[36] I refer to: Herbert Simon, *Administrative Behavior*, Second Edition (New York, 1957); Richard M. Cyert and James C. March, *A Behavioral*

heart of these theories is the concept of the decision maker struggling to choose among complex alternatives and constantly receiving much more information concerning his environment than he is able to digest and evaluate. An ordinary decision maker, required to make frequent choices and faced with an inconclusive flood of reports, programs, suggestions and memos, must simplify his task in some way. According to Simon, he does not—cannot—search in every case for the best possible solution to the problems he faces; he has neither the time nor the energy. Instead, he makes decisions by searching until he finds an alternative which he believes is good enough to preserve whatever values are important to him. The limits of rationality imposed by human capacities prevent him from maximizing his benefits in every situation; rather, he "satisfices," or chooses a course of action which seems satisfactory under the circumstances.

The individual in a complex organization, therefore, does not deal directly with all the sources of information potentially available to him, nor does he evaluate every conceivable policy option. In place of the debilitating confusion of reality he creates his own abstract, highly simplified world containing only a few major variables. In order to achieve this manageable simplicity he adopts a set of decision rules or standard criteria for judgment which remain fairly stable over time and which guide him in choosing among sources of information and advice. A decision maker decides both where to look for cues and information and how to choose among alternatives according to his decision rules; these rules also embody the current goals and aspirations of his organization, or the values which the organization is designed to advance and protect. Hence, if we wish to predict the decision maker's behavior, we should try to discover these rules of thumb, or "heuristics" as they are sometimes called, which shape his judgment. His choices could then be explained in terms of the alternatives he considers, his knowledge of each alternative, the sources of his knowledge, and the standard decision rules he applies in cases of this kind.[37]

Taking cues from these theories of human choice and organizational decision making our explanation of the adoption of innovations by the states is based on the assertion that state officials make most of their decisions by analogy. The rule of thumb they employ might be formally stated as follows: *look for an analogy between the situation you are dealing with and some other situation, perhaps in some other state, where the problem has been successfully resolved.*[38]

We are looking to what has been called the "inter-organizational context."[39] or the *horizontal* relationships among the states within the federal system, for the principal influences which regulate the speed of adoption and the patterns of diffusion of innovations. Most of the existing work on intergovernmental relations and federalism concentrates on the question of centralization within the American system of government. In line with the general interest of most political scientists in the factors which affect the access of organized groups and the lines of authority within a political system, many writers are concerned with the virtues of centralization or decentralization and try to determine how much of either exists in the system. They have studied primarily the *vertical* relationships among national, state and local governments, and have usually identified the party system and its demands as the institutional influence most responsible for maintaining the present, decentralized, federal relationships.[40] I want to focus attention on the mutual perceptions and relationships among state governments and to show how

Theory of the Firm (Englewood Cliffs, N.J. 1963); and Charles E. Lindblom, *The Intelligence of Democracy* (New York, 1965).

[37] For a comprehensive review of the literature on decision making see: Donald W. Taylor, "Decision Making and Problem Solving," and Julia Feldman and Herschel E. Kanter, "Organizational Decision Making," in James G. March (ed.) *Handbook of Organizations* (Chicago, 1965), pp. 48–86, 614–649. Also see: W. Richard Scott, "Theory of Organizations," in Robert E. L. Faris (ed.), *Handbook of Modern Sociology* (Chicago, 1964), pp. 485–529.

[38] Decision rules of this kind are mentioned in both Taylor, *op. cit.*, pp. 73–74; and Cyert and March, *op. cit.*, especially pp. 34–43.

[39] William M. Evan, "The Organization-Set: Toward a Theory of Inter-Organizational Relations," in James D. Thompson (ed.) *Approaches to Organizational Design* (Pittsburgh, 1966), pp. 173–191.

[40] Some recent examples are: William Anderson, *The Nation and the States, Rivals or Partners?* (Minneapolis, 1955); M. J. C. Vile, *The Structure of American Federalism* (London, 1961); William Riker, *Federalism: Origin, Operation, Significance* (Boston, 1964); Daniel J. Elazar, *American Federalism: A View From the States* (New York, 1966); Morton Grodzins, *The American System* (Chicago, 1966). For a general critique see: A. H. Birch, "Approaches to the Study of Federalism," *Political Studies* (1966), 15–33.

these relationships affect the behavior of state decision makers.[41]

One of the most common arguments used in state legislatures against raising taxes or passing measures designed to regulate business is the fear that such measures might retard industrial development or force marginal plants to leave the state. Lawmakers often are called upon to deal with the problems which arise when one or two states establish extremely permissive standards for the granting of licenses, such as the corporation laws in New Jersey and Delaware, or the divorce laws in Nevada. However, interstate competition does not always drive standards down; it has a positive side as well. State decision makers are constantly looking to each other for guides to action in many areas of policy, such as the organization and management of higher education, or the provision of hospitals and public health facilities. In fact, I am arguing that this process of competition and emulation, or cuetaking, is an important phenomenon which determines in large part the pace and direction of social and political change in the American states.[42]

Uncertainty and the fear of unanticipated consequences have always been formidable barriers to reform. Proponents of new programs have always had to combat the arguments of those who predict dire consequences if some innovation is adopted. Even though American history is full of cases where the opponents of change have later had to admit that the dangers they feared never materialized, inertia and the unwillingness to take risks have prevented a more rapid rate of change.

Inertia can more easily be overcome, however, if the proponent of change can point to the successful implementation of his program in some other similar setting. If a legislator introduces a bill which would require the licensing of probation officers, for example, and can point to its successful operation in a neighboring state, his chances of gaining acceptance are markedly increased. As Harsanyi has asserted:

> . . . it is not an overstatement to say that a very considerable part of the social values of most societies is based on sheer ignorance. . . . One of the reasons why other persons' example is so important in encouraging changes in people's values and behavior lies in the fact that it tends to dispel some groundless fears about the dismal consequences that such changes might entail. Another reason is of course that people can more easily face the possible hostility of the supporters of the old values if they are not alone in making the change.[43]

In fact, once a program has been adopted by a large number of states it may become recognized as a legitimate state responsibility, something which all states ought to have. When this happens it becomes extremely difficult for state decision makers to resist even the weakest kinds of demands to institute the program for fear of arousing public suspicions about their good intentions; once a program has gained the stamp of legitimacy, it has a momentum of its own. As Lockard found in studying the passage of Fair Employment Practices laws the actions of other states are sometimes key factors in prompting

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[41] This is not the first study to discover the important role of emulation and competition in the development of public policy. Richard Hofferbert in: "Ecological Development and Policy Change in the American States," *Midwest Journal of Political Science* (1966), p. 485; and Ira Sharkansky in: "Regionalism, Economic Status and the Public Policies of American States," *Southwestern Social Science Quarterly* (1968) both mention the influence of other states in the calculations of state decision makers. Several earlier students of local government complained that sparsely populated, arid Western states had blindly copied from the heavily populated Eastern states forms of local government which were inappropriately suited for the conditions prevailing in the Great Plains. See: A. Bristol Goodman, "Westward Movement of Local Government," *The Journal of Land and Public Utility Economics* (1944), pp. 20–34; Herman Walker, Jr. and Peter L. Hansen, "Local Government and Rainfall," this REVIEW (1946), 1113–1123. Robert L. Crain has recently used emulation as a principal explanatory variable in his study of the spread of water fluoridation programs among American cities: "Fluoridation: The Diffusion of an Innovation Among Cities," *Social Forces* (1966), 467–476; as did Thomas M. Scott in his: "The Diffusion of Urban Governmental Forms as a Case of Social Learning," *The Journal of Politics* (1968), 1091–1108.

[42] This set of hypotheses is consistent with more general theories concerning the manner in which human beings formulate judgments and establish expectations in all areas of life. See: Leon Festinger, "A Theory of Social Comparison Processes," *Human Relations* (1954), 117–140; and Robert Merton, *Social Theory and Social Structure* (Rev. Ed.; Glencoe, 1957), pp. 225–420.

[43] John C. Harsanyi, "Rational Choice Models v. Functionalistic and Conformistic Models of Political Behavior," (Paper delivered at American Political Science Association Meetings, 1967), p. 17.

reluctant politicians to accept controversial programs.

Pressure mounted in New Jersey during 1944 and 1945 for some stronger policy, and when New York passed its FEP law certain key politicians in New Jersey decided to act. Governor Walter E. Edge concluded, apparently reluctantly, that he had to commit himself to such a law. "As the session drew to a close," Edge wrote in his autobiography, "minority racial and religious groups pressed for adoption of an antidiscrimination program. While it was a subject which I would have preferred to give greater study, politically it could not be postponed because New York had passed a similar measure and delay would be construed as a mere political expedient."[44]

For similar reasons there have been numerous efforts to enact a program of homesteading in Hawaii as a way of disposing of its arable public lands even though the circumstances there are quite different from other states where homesteading was successfully introduced.[45] And in Connecticut one of the most powerful arguments in favor of introducing the direct primary system during the 1950's was simply that all the other states had adopted one.[46]

The Connecticut case neatly illustrates some of the generalizations we are developing. Lockard points out that the leaders of both political parties privately opposed the introduction of a primary system but felt that an endorsement of the idea had to be put into their platforms to avoid having their opponents charge them with "bossism." Demands for the primary came for the most part from small groups in the state's suburban areas which were interested in the issue as "a consequence of the influx of migrants from states with primaries."[47] Speaking as a professional political scientist as well as a legislator, Lockard was well suited to counter the extreme fears expressed by the party leaders who predicted that party organizations would be completely destroyed if primaries were introduced. Lockard reasoned by analogy to the experience in other states both in countering the opponents of change and in shaping his own moderate position:

I expressed my considerable doubts about the effect of party primaries on party organization. From observations of politics in some of the most thoroughgoing party primary states, [however,] it seemed that the party organizations had been shattered with many undesirable consequences. In my campaign I expressed support only for a limited form of a primary and not one calculated to wreck the party system.[48]

Events like these illustrate the way in which the agenda of controversy in a state is determined, at least in part, by developments in other states, and they also show how experiences and examples from outside the system help to overcome the natural reluctance of any institutional structure to risk the consequences of change. The constituent units of any federal system are under considerable pressure to conform with national and regional standards or accepted administrative procedures. These norms result primarily from the processes of emulation and competition we have described and also from the efforts of nationally organized interest groups. They are affected also by the growth and development of professional organizations and other forms of communication among state administrators, and the natural circulation of active, politically involved citizens among the states, such as the Connecticut suburbanites who began agitating for a primary system in their adopted political home.

VI. REGIONAL REFERENCE GROUPS AND STANDARDS OF EVALUATION

Nationally accepted standards or norms provide a convenient measure which can be used by interested citizens or political leaders to judge the adequacy of services offered in their own states. But these norms have an ambiguous influence on the performance of state governments. On the one hand, the existence of national standards probably encourages *higher* performance among the *poorer* members of the federation than we could expect if functions and service levels were established independently within each unit of government, solely as a result of internal demands. An example of this tendency was discovered by May in his study of Canadian federalism:

Newfoundland chose for a long time to remain outside the Canadian federation, thus not subjecting itself to the forces of national reorientation, and when, after joining the Dominion, a royal commission reported on its financial position, the commission observed that Newfoundland's public services were very backward in re-

[44] Duane Lockard, *Toward Equal Opportunity* (New York, 1968), pp. 20–21.

[45] Allan Spitz, "The Transplantation of American Democratic Institutions," *Political Science Quarterly* (1967), 386–398.

[46] Duane Lockard, *Connecticut's Challenge Primary: A Study in Legislative Politics* (Eagleton Case #7, New York, 1959).

[47] *Ibid.*, p. 2.

[48] *Ibid.*, p. 22.

lation to those of the other provinces, including even the maritimes. . . .[49]

In the United States, Mississippi, Vermont, and North Dakota are good examples of relatively poor states which are making unusually large efforts to bring their public services into closer approximation of national standards. But, on the other hand, national standards and norms can have a *conservative* impact, especially in the richer, industrial states which are able to provide services somewhat above the national averages with relatively little effort.[50] Hansen complains of this tendency when he points out that:

> Some northern states fall considerably below their northern neighboring states in public service standards. . . . Their fiscal problems arise not because they are poor but because their tax levels are low by northern standards. This is notably true for example of a tier of large industrial states—Illinois, Indiana, Ohio and Pennsylvania. . . . These states are not excessively hard pressed by tax burdens relative to the country as a whole.[51]

This statement by Hansen is drawn from an essay in which he expresses disapproval of what he considers the inadequate public services of large industrial states which have relatively low tax burdens. But the statement we have cited contains several ambiguities. For example, Hansen charges that "some northern states fall considerably below their northern neighboring states in public service standards," but then he specifically points as examples to Illinois, Indiana, Ohio, and Pennsylvania, states which border on each other. It is not clear whether we are being asked to compare these states to their neighbors, to other northern states with higher tax burdens, or to "the country as a whole." Within Illinois, however, the states' decision makers are probably comparing their own performance with their counterparts in Indiana, Ohio, Pennsylvania or New Jersey. Officials in Illinois may know of the procedures and performance levels in New York or California, but they are unlikely to think of events in these states as legitimate guides to action.[52]

When examining the public policy of any state, therefore, it is important to discover in which "league" it has chosen to play. For example, Salisbury, in a statement much like Hansen's, reasons by analogy in arguing that Missouri does not provide as much aid for its schools as its potential resources might warrant. He points out that in 1959 the "state ranked 18th in per capita income but 38th in per capita expenditure for local schools."[53] This relatively low level of support seems to result from the correspondingly low aspirations of the officials of the Missouri State Teachers Association who, according to Salisbury, "have chosen to get what they can with a minimum of agitation or conflict rather than attempt broader public campaigns in behalf of larger objectives."[54] The officials of MSTA "are fully conscious of the gap between the Missouri school aid level and that of, say, neighboring Illinois," but they are quick to point out "that by comparison with other neighboring states—Arkansas, Oklahoma, or Nebraska, for example—Missouri's record is much more impressive."[55] It would seem from this example that Missouri's leaders, at least those concerned with public education, are emulating and competing primarily with the states to their south and west, rather than with the Great Lakes states to their north and east, or the Rocky Mountain states, the Deep South or the Far West. The choice of relatively poor states like Arkansas and Oklahoma as the principal, legitimate reference groups establishes an upper limit of aspirations which is considerably below that which might exist if Missouri's accepted basis for comparison were the public services of Illinois, Wisconsin or Michigan.

VI. REGIONAL GROUPINGS AMONG THE STATES

We have come far enough in our analysis to see that our original presentation of the innovation scores in Table 1 as a linear distribution masked some pertinent information. A more useful representation of the data, which would con-

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[49] Ronald J. May, *Financial Inequality Between States in a Federal System* (unpublished doctoral dissertation submitted to Nuffield College, Oxford University, 1966), p. 168.

[50] For a somewhat similar argument concerning government spending see: Anthony Downs, "Why the Government Budget is too Small in a Democracy," *World Politics* (July, 1960), 541–563.

[51] Alvin H. Hansen, *The Postwar American Economy: Performance and Problems* (New York, 1964), pp. 30–31.

[52] For evidence of this perspective, see Thomas J. Anton, *The Politics of State Expenditure in Illinois* (Urbana, 1966), p. 263.

[53] Nicholas A. Masters, Robert Salisbury, and Thomas H. Eliot, *State Politics and the Public Schools* (New York, 1964), p. 12.

[54] *Ibid.*, p. 25.

[55] *Ibid.*, p. 21. For a similar discussion of the importance of aspirations in determining the speed with which innovations are adopted see: Rufus P. Browning, "Innovative and Noninnovative Decision Processes in Government Budgeting," in Robert T. Golembiewski (ed.), *Public Budgeting and Finance* (Itasca, Illinois, 1968), pp. 128–145.

form more closely to the actual patterns of diffusion, would have to be in the form of a tree. At the top of the tree would be a set of pioneering states which would be linked together in a national system of emulation and competition. The rest of the states would be sorted out along branches of the tree according to the pioneer, or set of pioneers, from which they take their principal cues. States like New York, Massachusetts, California, and Michigan should be seen as regional pace setters, each of which has a group of followers, usually within their own region of the country, that tend to adopt programs only after the pioneers have led the way. For example, Colorado, which ranks ninth in Table 1, might be seen as the regional leader of the Rocky Mountain states. The rest of the states in that region are found much further down the list: Utah is twenty-second, Idaho is thirty-third, Arizona is thirty-sixth, Montana is thirty-eighth, New Mexico is forty-first, Wyoming is forty-sixth, and Nevada is forty-seventh. All of these states, with the possible exception of Utah which may share in the leadership of the region, might be seen as Colorado's followers who usually pick up new ideas only after the regional pioneer has put them into practice.

If we are right about the general patterns of competition and emulation, we should discover in our data some evidence of the existence of regional clusters among the states. In an effort to find such groupings, a varimax factor analysis was performed, using a matrix of pair-wise comparisons of all state innovation scores on all eighty-eight issues. If states in the same region are adopting programs in a similar order or pattern over time, a factor analysis should uncover several underlying dimensions in the matrix along which all states would be ordered according to their responses to the programs upon which the innovation score is based. The results of the factor analysis are presented in Table 5.

As we can see, the regional groupings we expected to find do exist, although the patterns are not as neat and clear as we might have hoped. To produce each factor I recorded all loadings which were over .400. The five factors which result bring the states into generally recognizable, contiguous groupings. The states with the largest loadings in each region are not necessarily those with the highest innovation scores. Instead, they are states like Connecticut, Florida, or New Mexico whose innovation scores are closer to the average for their regions. The presence of Nebraska, Iowa and South Dakota on Factor 1, which otherwise identifies Southern states, may indicate that more than one regional cluster is being identified on that factor.

There are several ambiguities in the data. For example, New York, Pennsylvania, West Virginia, Arkansas, and Illinois are loading on more than one factor. The easiest explanation of this may be that the states actually have connections with more than one region. This is especially true of New York, the state with the highest innovation score, which displays fairly strong connections in this analysis with the New England. Mid-Atlantic, and Great Lakes states. I believe that this finding reflects the fact that New York actually serves as a model for states in all three areas. Certainly New York is formally involved in interstate compacts with all three regions, and, if nothing else, enjoys a perfect geographical position from which to carry on relations over such a large area. If the findings concerning New York seem explainable, those concerning California do not. I cannot explain why California loads on Factor V, especially since many of its neighbors load on Factor III. These ambiguous findings concerning New York and California might be merely a reflection of ambiguity in the data. Factor analysis will identify regional groupings in the data only if the regions respond to new programs as a unit, adopting some new ideas with haste and lagging behind on others. Since New York and California consistently lead the country in the adoption of new programs, they may not be members of the cohesive regional group or "league" of states, a fact which may prevent their neat categorization through factor analysis.

There is no accounting at all in this analysis for the behavior of three states: Arizona, Colorado, and Kansas. Both Colorado and Arizona load at the .300 level on Factor III, the one which includes most of the rest of the Rocky Mountain states. Colorado and Nevada both load strongly (.577 and .485 respectively) on a separate factor which was not reported since no other state scored higher than .300 on the factor and its contribution score was only 1.7. The same is true for Kansas which was the only state loading strongly (at .658) on a factor whose contribution score was only 1.9.

VII. SPECIALIZED COMMUNICATIONS AMONG THE STATES

Our analysis has provided evidence that a continuum exists along which states are distributed from those which are usually quick to accept innovations to those which are typically reluctant to do so; we also know something about the correlates of innovation and have evidence of regional groupings among the states; but it is not always easy to identify a regional pioneer or to know exactly which states make up each

TABLE 5. VARIMAX FACTOR ANALYSIS OF INNOVATION SCORES FOR FORTY-EIGHT STATES

Factor Loading	State
FACTOR I (South)	
.756	Florida
.711	Tennessee
.663	Alabama
.661	Virginia
.656	Georgia
.630	Mississippi
.621	Delaware
.600	North Carolina
.590	South Carolina
.576	Arkansas
.543	Texas
.517	Nebraska
.464	West Virginia
.460	Louisiana
.459	Iowa
.454	South Dakota
.433	Nevada
7.8	Total Factor Contribution
FACTOR II (New England)	
.795	Connecticut
.766	Massachusetts
.758	New Hampshire
.659	Rhode Island
.536	New York
.512	Vermont
.434	Maine
.404	Pennsylvania
4.1	Total Factor Contribution
FACTOR III (Mountains and Northwest)	
.791	New Mexico
.719	Idaho
.702	Montana
.694	Utah
.638	Washington
.620	North Dakota
.610	Wyoming
.569	Oklahoma
.516	Louisiana
.503	South Dakota
.432	Oregon
.419	Maryland
.410	Arkansas
.407	West Virginia
6.7	Total Factor Contribution

TABLE 5—(*Continued*)

Factor Loading	State
FACTOR IV (Mid-Atlantic and Great Lakes)	
.795	New Jersey
.637	Wisconsin
.605	New York
.577	Minnesota
.536	Illinois
.516	Pennsylvania
.451	Indiana
4.0	Total Factor Contribution
FACTOR V (Border, Great Lakes and California)	
.698	California
.610	Missouri
.584	Kentucky
.577	Michigan
.548	Ohio
.515	Nebraska
.458	Illinois
4.1	Total Factor Contribution

"league" or sub-system of cue-taking and information exchange. Some states seem to have connections with more than one region and may regularly receive cues from states in both groupings. As the American political system has developed, an increasing number of specialized communication systems have been created which cut across traditional regional lines and bring officials from many different regions into contact with each other and with federal and local officials, journalists, academic experts, and administrative consultants.

Several organizations now exist, such as the Council of State Governments, the Federal Commission on Intergovernmental Relations, and the recently established Citizen's Conference on State Legislatures, whose primary function is to improve communications among the states. Most important of these specialized communications networks are the professional associations of state officials, such as the National Association of State Budget Officers, or the National Association of State Conservation Officers. Associations of this kind were first created late in the nineteenth century and more seem to be forming each year. There were only five formed prior to 1900, but by 1930 there were approximately thirty-one, and by 1966 there were at least eighty-six in existence.[56]

[56] Unpublished memo from the Council of State Governments, Chicago, Illinois.

These groups serve two general purposes: first, they are sources of information and policy cues. By organizing conferences or publishing newsletters they bring together officials from all over the country and facilitate the exchange of ideas and knowledge among them, thus increasing the officials' awareness of the latest developments in their field. Secondly, these associations serve as "occupational contact networks" which expedite the interstate movement or transfer of personnel. Through the efforts of these groups officials become aware of desirable job openings in other states and are able to create professional reputations that extend beyond the borders of their own states.[57]

By rapidly spreading knowledge of new programs among state officials and by facilitating the movement of individuals to jobs in other states, professional associations encourage the development of national standards for the proper administration and control of the services of state government. Just as in other sectors of American life such as the business, the military and the academic world, as individuals increase their mobility, their role perceptions are likely to change; they are likely to adopt a more cosmopolitan perspective and to cultivate their reputations within a national professional community rather than merely within their own state or agency.[58]

Since general awareness of new developments is achieved much more quickly now than ever before, we would expect that the time which elapses from the first adoption of an innovation by a pioneering state to its complete diffusion throughout all the states would be greatly reduced. Certainly, several recent innovations, such as educational television or state councils on the performing arts, have diffused rapidly. In Table 6 we have measured the average speed of diffusion in years for three periods of time: 1870–1899, 1900–1929, and 1930–1966. The results shown in the first column of this table make it very plain that the speed of diffusion has been constantly increasing as time has passed. This measurement, however, is somewhat misleading. The second column of the table indicates the average number of years it took the first twenty states to adopt the programs in each time period. The same trend toward increased speed of diffusion is evident here, but the differences among the three time periods are much smaller.[59] This evidence suggests that the pioneering states, those with high innovation scores, adopted new programs about as quickly in the early part of this century, prior to the development of many specialized communication links, as they did in the 1960's. The total elapsed time of diffusion, however, has decreased primarily because the laggard states, those with low innovation scores, are now reacting more quickly to pick up new programs adopted by the pioneers. This development results partly from the efforts of the federal government to stimulate state action through grants-in-aid, and partly from the increasing professional development in state government. Both these tendencies seem to have had a larger impact on the behavior of the more parochial states than the more cosmopolitan, pioneering states.

TABLE 6. AVERAGE ELAPSED TIME OF DIFFUSION IN YEARS FOR INNOVATIONS IN THREE TIME PERIODS

Time Periods	For All Adoptions	First Twenty Adoptions
1870–1899:	52.3	22.9
1900–1929:	39.6	20.0
1930–1966:	25.6	18.4

VIII. THE PERSISTENCE OF REGIONALISM

Improved communications and greatly increased contacts of all kinds among state officials seem to be accelerating the process of diffusion, but this does not necessarily mean that the regional clusters or "leagues" of states

[57] For a discussion of the role of professional organizations in determining career lines see: Fred E. Katz, "Occupational Contact Networks," *Social Forces* (1958), 52–58. Also see: Jack Ladinsky, "Occupational Determinants of Geographic Mobility Among Professional Workers," *American Sociological Review* (1967), 253–264.

[58] Merton, *op. cit.* Also see: Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles," *Administrative Science Quarterly* (1957), 281–306; and Harold L. Wilensky, *Intellectuals in Labor Unions* (Glencoe, 1956).

[59] A small portion of the difference between the two columns in Table 6 is an artifact of measurement. Since not all the programs in this analysis have been adopted by all forty-eight states, laggard states sometimes remain. As time passes and programs receive widespread acceptance these laggard states slowly fall into line and adopt the programs. Since the programs in the first two time periods have been around longer, they have more likely completed their spread among the states and thus, given our scoring procedure, are also more likely to have a longer period of diffusion.

to which we have referred have been destroyed.[60] In order to investigate this question the innovation scores in the time periods from 1870 to 1929 were combined, and two matrices of innovation scores of almost equal size were created, one for 1870–1929 and the other for 1930–1966.[61] Within each of these matrices each state's set of innovation scores (issue by issue) was correlated with the set of innovation scores for each other state. A varimax factor analysis was performed on each matrix, just as was done earlier to produce Table 5.

The results of this analysis are presented in Table 7. The factors derived from 1870–1929 are presented in the left column of the table and those from 1930–1966 are presented in the right column. The factors from each time period are arranged with the highest loadings first and the rest following in descending order. As we can see, the factors from the two time periods are not completely comparable. Some states change their relative rankings on the two factors, and some appear on a factor during only one of the time periods. The state of Georgia, for example, is found at the bottom of Factor 1 during 1870–1929 and moves all the way to the top of the same factor during 1930–1966. Some regional groupings, such as New England, seem to be disintegrating, while others, such as the Middle Atlantic states, seem to be more clearly defined in the later period. The factors for the later period include more states, on the average, and have slightly higher contribution scores, but they are not quite as distinct as those in the early period and include more inappropriate loadings. These data do not contain evidence of any large scale erosion of regionalism in the United States, but a drift away from clearly defined clusters of states is apparent.

During the last thirty years many new professional associations have been formed and more inter-state and federal agencies have begun facilitating communications and encouraging national uniformity. The diffusion process is operating much faster today than ever before, especially among those states which have traditionally lagged behind in adopting new ideas. The older, established modes of communication and evaluation, based on traditional ties of region and common culture, are persisting, but there are indications in these data that the system is slowly changing. Decision makers in the states seem to be adopting a broader, national focus based on new lines of communication which extend beyond regional boundaries.

IX. CONCLUSIONS

This essay began as an effort to explain why some states adopt innovations more rapidly than others, but in order to explain this aspect of American federalism, we have had to make a more extensive investigation of the complex system of social choice by which we are governed. The approach to policy making which has emerged from our investigation is founded on the perceptions and attitudes of individual state decision makers. Of course, as I have already mentioned, the theory cannot be fully elaborated or put to a test until data can be gathered directly from legislators, bureaucrats, governors, and other officials in several states, on a comparative basis. Enough evidence has been presented already, however, to make apparent the major theoretical and practical implications of this approach.

The theory presented here directs our attention to the rules for decision employed by policy makers, rather than their formal group affiliations or their relative power or authority, and thus enables us to offer useful explanations of all policy decisions, not merely those which generate controversy. Emphasis is placed on those factors which lead to the establishment of parameters or guidelines for decision, not on the groups or interests supporting one policy over another. In Figure 1 the outlines of the diffusion process are depicted as it operates in a single state. There are undoubtedly many other influences on the level of agitation for change than the ones presented here, and many other secondary effects stemming from the enactment of new programs; this simple diagram is only meant to summarize the fundamental process operating in most cases of diffusion. Relationships are characterized by plus and minus signs but no effort has been made to estimate their relative importance in the system.

The process we have been describing is extremely complex; many influences shape deci-

[60] The best recent analysis of long-term changes in the American political system is: Donald Stokes, "Parties and the Nationalization of Electoral Forces," in William N. Chambers and William D. Burnham (eds.), *The American Party Systems: Stages of Political Development* (New York, 1967), pp. 182–202. Also see: Norval D. Glenn and J. L. Simmons, "Are Regional Cultural Differences Diminishing?" *Public Opinion Quarterly* (1967), 196–205; and Ira Sharkansky, "Economic Development, Regionalism and State Political Systems," *Midwest Journal of Political Science* (1968), 41–61.

[61] When the data are combined in this manner the 1870–1929 matrix contains 42 issues and the 1930–1966 matrix contains 46 issues.

TABLE 7. VARIMAX FACTOR ANALYSIS OF INNOVATION SCORES FOR FORTY-EIGHT STATES IN TWO TIME PERIODS

FACTOR I (South) 1870–1929		FACTOR I (South) 1930–1966	
Factor Loading	State	Factor Loading	State
.762	Tennessee	.793	Georgia
.748	Mississippi	.759	Virginia
.745	Florida	.649	Delaware
.705	North Carolina	.629	Tennessee
.662	West Virginia	.623	Florida
.646	Kentucky	.593	Texas
.521	Louisiana	.570	North Carolina
*.499	Arizona	*.541	Utah
.465	Delaware	.524	Alabama
.426	Virginia	.494	Maryland
.425	South Carolina	*.493	Nebraska
*.424	Iowa	.493	South Carolina
.404	Georgia	*.451	Arizona
		*.432	Montana
5.7	Total Factor Contribution	*.426	Kansas
		*.415	Iowa
		*.415	Maine
		.413	Louisiana
		*.410	New Hampshire
		7.1	Total Factor Contribution
FACTORS II AND III (New England—Mid-Atlantic—Great Lakes)			
.851	Connecticut	.800	Connecticut
.814	New Hampshire	.702	Massachusetts
.707	Vermont	.629	New Hampshire
.705	Massachusetts	*.564	Colorado
.670	Rhode Island	*.498	Oregon
.576	Maine	.467	Rhode Island
.509	Delaware		
.487	New York	1.7	Total Factor Contribution
.467	Pennsylvania		
.467	Virginia		
.405	Maryland		
*.405	Alabama		
5.3	Total Factor Contribution		
.808	Kansas	.778	New York
.694	Indiana	.686	Pennsylvania
.643	Wisconsin	.684	New Jersey
.622	Illinois	.666	Wisconsin
.601	Minnesota	.537	Illinois
*.448	Texas	.491	Michigan
		.486	Indiana
4.5	Total Factor Contribution	.474	Minnesota
		.448	Maryland
		4.8	Total Factor Contribution
FACTOR IV (Plains and Mountains)			
.769	North Dakota	.710	North Dakota
.762	New Mexico	.683	New Mexico
.722	Montana	.682	Kansas
.709	Utah	.641	Wyoming

* States which are loading on inappropriate factors are marked with an asterisk.

TABLE 7—(*Continued*)

FACTOR I (South) 1870–1929		FACTOR I (South) 1930–1966	
Factor Loading	State	Factor Loading	State
.665	Idaho	.633	Oklahoma
.639	Washington	.598	Washington
.567	South Dakota	.572	Oregon
*.494	Maine	.557	Utah
		*.494	Alabama
4.7	Total Factor Contribution	.462	Idaho
		*.457	Vermont
		*.439	West Virginia
.751	Arizona	*.416	Wisconsin
.588	Nevada	.410	Montana
.578	Wyoming	*.406	Mississippi
*.469	Arkansas		
		6.5	Total Factor Contribution
2.5	Total Factor Contribution		
.730	Oregon		
.611	California		
.645	Colorado		
*.433	Maryland		
2.9	Total Factor Contribution		
FACTOR V (Mid-America)			
.885	Missouri	.726	Missouri
.767	Nebraska	.614	Mississippi
*.639	Michigan	*.600	South Carolina
.419	Ohio	*.589	Idaho
*.400	California	.573	Arkansas
		.530	Tennessee
3.4	Total Factor Contribution	.432	Illinois
		.426	West Virginia
		*.409	South Dakota
		*.409	Montana
		4.5	Total Factor Contribution

sions to adopt innovations and no two ideas diffuse in exactly the same way. In all cases, however, the likelihood of a state adopting a new program is higher if other states have already adopted the idea. The likelihood becomes higher still if the innovation has been adopted by a state viewed by key decision makers as a point of legitimate comparison. Decison makers are likely to adopt new programs, therefore, when they become convinced that their state is relatively deprived, or that some need exists to which other states in their "league" have already responded.

Before states may respond to new programs adopted in other states their political leaders must be aware of these developments, so interstate communications are an important factor in the process of diffusion. We have mentioned that many specialized systems of communication among the states have grown up during the last thirty years, mainly through the creation of

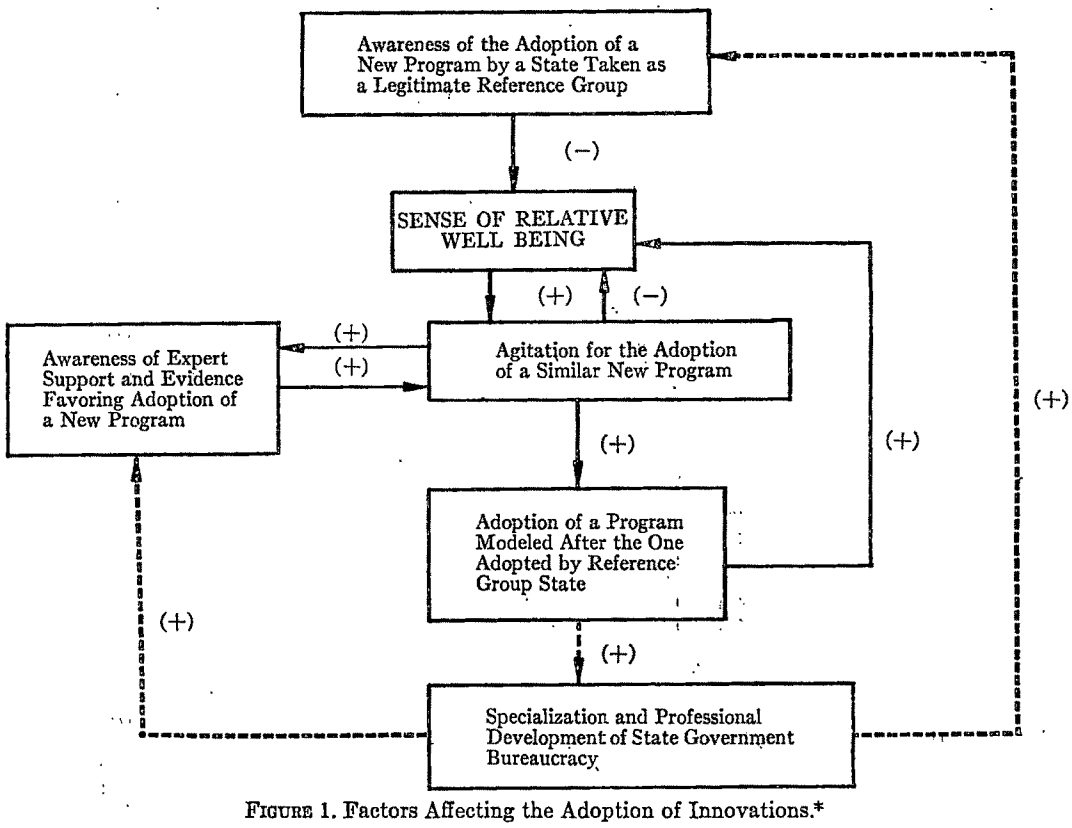


FIGURE 1. Factors Affecting the Adoption of Innovations.*

* Secondary effects depicted by broken lines.

professional associations among state administrators. These new information networks are spreading into all the states, but even today the isolation of some state capitols from the major cosmopolitan centers of the country is a major obstacle to the adoption of new ideas.[62]

Emerging from this study is the picture of a national system of emulation and competition. The states are grouped into regions based on both geographical contiguity and their place in the specialized set of communication channels through which flow new ideas, information and policy cues. Through this nationwide system of communications a set of norms or national standards for proper administration are established. This system links together the centers of research and generation of new ideas, national associations of professional administrators, interest groups, and voluntary associations of all kinds into an increasingly complex network which connects the pioneering states with the more parochial ones. Because of the limitations of the data presently available to us we can only outline each regional grouping of states, and we cannot yet construct an elaborate theory of the interactions among professional associations, federal officials, private interest groups, and political leaders in setting the agenda of politics within a state. Normative questions arise, which cannot be considered here, concerning the responsiveness of this system and the degree to which it is subject to the control of democratic, representative institutions.[63] Much more investigation will

[62] See Alan L. Clem's description of the isolation of Pierre, the capitol of South Dakota, in his: *Prairie State Politics: Popular Democracy in South Dakota* (Washington, 1967), p. 137; and Norton E. Long's emphasis on the importance of information sources in his: "After the Voting is Over," *Midwest Journal of Political Science* (1962), 183–200. For a general review of communications theory and its application to politics see: Richard R. Fagen, *Politics and Communication* (Boston, 1966), especially pp. 34–69, 88–106. Also see: Karl W. Deutsch, *The Nerves of Government*, Second Edition, (New York, 1966), especially pp. 145–256.

[63] Questions of this kind have been raised already in: Daniel P. Moynihan, "The Professionalization of Reform," *The Public Interest* (1965), 6–16; Theodore J. Lowi, "The Public Philosophy: Interest Group Liberalism," this REVIEW (1967), 5–24; and Philip Green, "Science, Government, and the Case of RAND: A Singular Pluralism," *World Politics* (1968), 301–326.

be necessary before we can gain a full understanding of this system and its function as a device for controlling the pace and direction of policy development in the American states. Once we know more, it might be possible to prescribe with confidence some changes in the decision-making system, or the creation of some new governmental institutions, which might accelerate or redirect the process of innovation.

APPENDIX

NOTE: Following are the eighty-eight programs upon which the innovation score is based.

1. Accountants Licensing
2. Advertising Commissions
3. Agricultural Experiment Stations
4. Aid for Roads and Highways
5. Aid to the Blind (Social Security)
6. Aid to Dependent Children (Social Security)
7. Aid to Permanently and Totally Disabled (Social Security)
8. Air Pollution Control
9. Alcoholic Beverage Control
10. Alcoholic Treatment Agencies
11. Anti-Age Discrimination
12. Anti-Injunction Laws
13. Architects Licensing
14. Australian Ballot
15. Automobile Registration
16. Automobile Safety Compact
17. Beauticians Licensing
18. Board of Health
19. Budgeting Standards
20. Child Labor Standards
21. Chiropractors Licensing
22. Cigaret Tax
23. Committee on the Aged
24. Compulsory School Attendance
25. Conservation of Oil and Gas
26. Controlled Access Highways
27. Council on the Arts
28. Court Administrators
29. Debt Limitations
30. Dentists Licensing
31. Direct Primary
32. Education Agencies
33. Education Television
34. Engineers Licensing
35. Equal Pay for Females
36. Fair Housing—Private
37. Fair Housing—Public Housing
38. Fair Housing—Urban Renewal Areas
39. Fair Trade Laws
40. Fish Agency
41. Forest Agency
42. Gasoline Tax
43. Geological Survey
44. Highway Agency
45. Home Rule—Cities
46. Human Relations Commissions
47. Initiative and Referendum
48. Integrated Bar
49. Junior College—Enabling Legislation
50. Juveniles Supervision Compact
51. Labor Agencies
52. Legislative Pre-Planning Agencies
53. Legislative Research Agencies
54. Library Extension System
55. Mental Health Standards Committee
56. Merit System
57. Migratory Labor Committee
58. Minimum Wage Law
59. Normal Schools—Enabling Act
60. Nurses Licensing
61. Old Age Assistance (Social Security)
62. Parking Agencies—Enabling Act for Cities
63. Park System
64. Parolees and Probationers Supervision Company
65. Pharmacists Licensing
66. Planning Board—State Level
67. Development Agency
68. Police or Highway Patrol
69. Probation Law
70. Public Housing—Enabling Legislation
71. Real Estate Brokers—Licensing
72. Reciprocal Support Law
73. Retainers Agreement
74. Retirement System for State Employees
75. Right to Work Law
76. School for the Deaf
77. Seasonal Agricultural Labor Standards
78. Slaughter House Inspection
79. Soil Conservation Districts—Enabling Legislation
80. Superintendent of Public Instruction
81. Tax Commission
82. Teacher Certification—Elementary
83. Teacher Certification—Secondary
84. Urban Renewal—Enabling Legislation
85. Utility Regulation Commission
86. Welfare Agency
87. Workmens' Compensation
88. Zoning in Cities—Enabling Legislation

COMMUNICATIONS

To the Editor:

In the March, 1969 issue of the Review (pp. 157–162) Thomas Dye reported on an analysis of relationships between political processes, socio-economic characteristics, and policy outcomes in the American states. The analysis featured the Gini Index, a measure of the degree of inequality in the distribution of income within states, a characteristic never before investigated by political scientists. The format and methodology of the study resembled many others published on this subject during the last six years in that no effort was made to isolate regions within the country to find if relationships changed in different cultural or political settings.[1] We consider this a major shortcoming of Dye's analysis.

Since the Southern states all have relatively large inequalities in income distribution, they cluster at the top of the scale created by the Gini Index and are also grouped at the top or bottom of many of the the other measures of political participation and economic development employed in Dye's study. The differences are so great between the South and the rest of the country and the Southern states so homogeneous on many of the measures used by Dye that he is often dealing with two distinct populations. When regions cluster in this way the correlations reported from bi-variate relationships often reflect only the social and political distinctiveness of geographic regions within the country rather than the functional relationships between variables within state political systems. Because of the distinctive nature of the South, correlational analyses based on all the American states may either obscure important relationships existing at the regional level or may lead one to see relationships where none actually exists.

For example, Dye reports a strong negative correlation of −.64 between per capita personal income and his measure of income inequality. When the eleven states of the confederacy are removed from this distribution, however, the correlation for the remaining thirty-nine states is reduced to −.39, about half its original strength. Within the eleven Southern states the relationship increases to −.85, almost twice its original strength. In Figure 1 we display a scatter-plot of the relationship between the Gini Index and per capita personal income which demonstrates graphically why the removal of the South has such an impressive impact.[2] The Southern states are concentrated in the upper left portion of the graph and the remaining states are much more widely and irregularly dispersed. For the thirty-nine states outside the South, knowledge of a state's score on the Gini Index allows one to explain only 15 per cent of the variance in state scores on per capita personal income.

The removal of the South may affect correlations of data on the states in many different ways. In Table 1 we display correlations of Dye's Gini Index with twelve variables; scores for each relationship are shown with the South both included and removed from the distributions.[3] The result of the South's removal usually

[1] For a review of research on this subject see: John H. Fenton and Donald W. Chamberlayne, "The Literature Dealing With the Relationships Between Political Processes, Socio-economic Conditions and Public Policies in the American States: A Bibliographical Essay," *Polity* (Spring, 1969), pp. 388–94.

[2] The Gini Index is taken from Dye's article, *op. cit.;* the data on per capita personal income by state are for 1960 and are taken from: U. S. Bureau of the Census, *Statistical Abstract of the United States* (Washington, 1967).

[3] All demographic data are for 1960; all expenditure data are for 1959. Sources are: U. S. Bureau of the Census, *Ibid.* The industrialism factor score is from: Richard Hofferbert, "Socioeconomic Dimension of the American States: 1890–1960," *Midwest Journal of Political Science* (August, 1968), 401–418. Party competition scores for the Senate and House cover 1961–62 and are taken from: John Grumm, Calvin Clark and Edward Flentje, *State Data* (compiled for the Citizen's Conference on State Legislatures, 1967). The scores represent the minority party percentage of the two-party membership times two. Party competition scores for the Governorship cover 1962 and are taken from: Richard I. Hofferbert, *American State Socioeconomic, Electoral, and Policy Data: 1890–1960.* (By permission of the compiler). The scores represent the percent of the total vote going to the candidate coming in second, times two. Turnout figures have been computed on the basis of total population above voting age. Turnout scores are calculated for senatorial and gubernatorial races in non-presidential years from 1946 to 1962, and are reported in: Lester Milbrath, "Political Participation in the States" in Herbert Jacob and Kenneth N. Vines (eds.), *Politics in the American States* (Boston, 1965), pp. 25–60. The apportionment index appears in: Paul T. David and Ralph Eisenberg, *Devaluation of the Urban and Suburban Vote* (Charlottesville: Bureau of

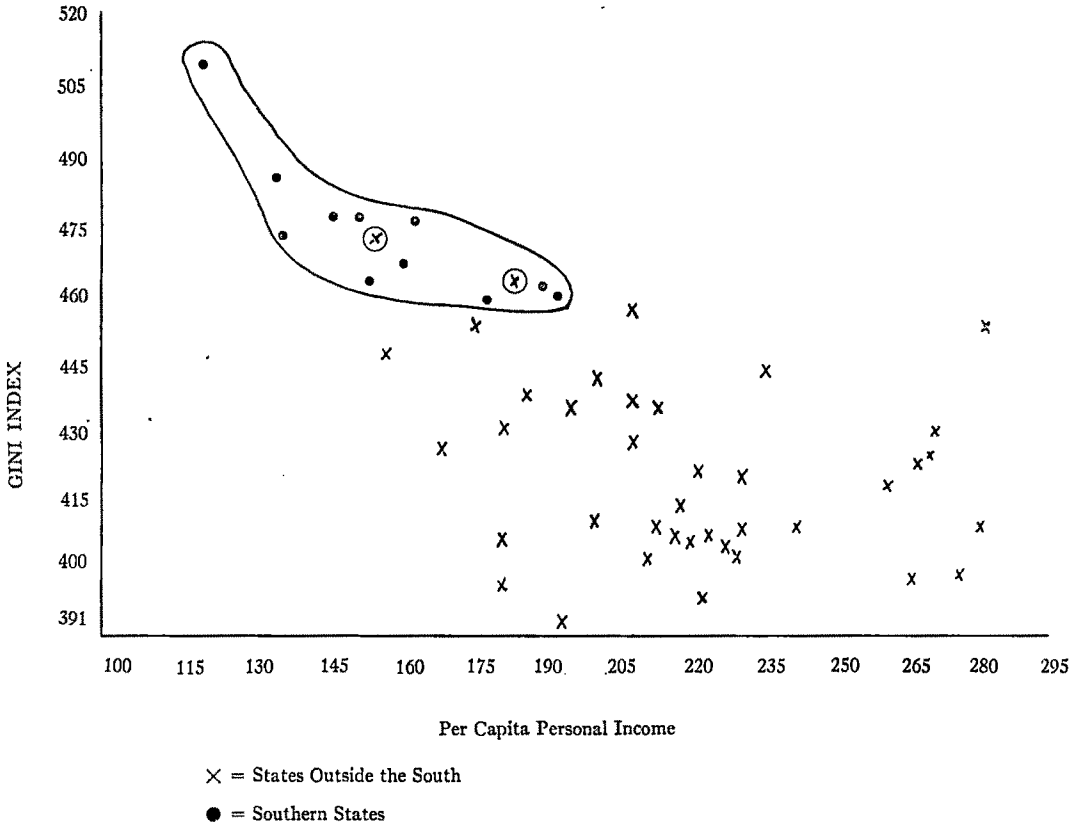


FIGURE 1. Per Capita Personal Income and the Gini Index.

is a sharp reduction in the correlation for the remaining thirty-seven states. The only exception is the case of per capita expenditures on welfare, where correlations increase when both regions are taken separately. The correlations within the Northern states for urbanism, industrialism, apportionment, and the formal powers of the governor are lowered, but do not change radically. Relationships are reduced to insignificance, however, for per cent Negroes, measures of party competition, electoral turnout, total per capita expenditures, or per capita expenditures for highways. Within the South the relations between the Gini Index and the political system measures are either eliminated, rendered insignificant, or greatly reduced in size. By testing for the regional effect, however, we have uncovered a strong positive relationship within the South between the Gini Index and Hofferbert's industrialism factor score which was concealed in Dye's analysis. Dye also reports that total per capita expenditures and per capita expenditures for highways are negatively correlated with income inequality, but we find, even though the coefficients are still small, that within the South the two variables actually are *positively* related to one another.

We have concentrated on the eleven states of the confederacy because Dye's Gini Index tends to emphasize their distinctiveness and there are good reasons to assume that they form a natural region. They are not only contiguous, but also share sociological and economic characteristics which set them apart from the rest of the country. Of course, the boundaries of the region are not necessarily confined to the states we have chosen; the distinctive political culture of the South may be found within portions of states outside the region, just as some parts of Texas or Florida could no longer be considered part of the South in sociological terms.[4] Nor is

Public Administration, University of Virginia, 1961). The index of the formal powers of the governor can be found in: Joseph A. Schlesinger, "The Politics of the Executive," in Herbert Jacob and Kenneth N. Vines (eds.), *op. cit.* pp. 207–237.

[4] For recent analyses of regions within individual states see: V. O. Key, Jr. and Frank Munger, "Social Determinism and Electoral Decision: The Case of Indiana," in Eugene Burdick and Arthur

TABLE 1. INCOME INEQUALITY CORRELATED WITH SELECTED ECONOMIC VARIABLES*

Economic Development Measures	All States	Northern States	Southern** States
Percentage Population Negro	.74	.16	.68
Industrialism Factor Score	.33	.28	.74
Percentage Population Urban	−.48	−.37	−.58
Political System Measures			
Competition for Senates	−.70	−.06	−.17
Competition for Houses	−.76	−.03	−.26
Competition for Governorships	−.54	.16	.13
Percentage Voter Turnout	−.82	−.18	−.39
David and Eisenberg Apportionment	−.45	−.40	−.03
Formal Powers of Governor	−.43	−.33	−.22
Policy Output Measures			
Per Capita Expenditures—Welfare	.26	.36	.32
Per Capita Expenditures—Highways	−.29	−.10	.24
Total Per Capita Expenditures	−.20	.01	.28

* Alaska and Hawaii were omitted because of missing data for several variables.

** Southern states are: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

the South the only distinctive region in the country. If median school years completed were one of our predictor variables we would find that the Rocky Mountain states form a distinctive area, and if we were concerned with the size of legislative bodies or per capita expenditures for higher education we would have to deal with the peculiar characteristics of New England.[5]

Whichever region one examines, the results we report in Table 1 should be a convincing argument for the importance of testing relationships for the possible effects of regional differences. This would seem to be a necessary prerequisite to the creation of general theories of political participation or policy making. In an earlier work, however, Dye has made the opposite case, arguing that hypotheses must be tested using all fifty states:

> Since the object of comparative study is to develop general explanations which will apply to political systems universally, the universe of American state politics must include all the states. If we excluded any group of states from our analysis, or if we tested explanatory hypotheses with reference to regional groupings of states, we would be engaged in something other than genuine comparative analysis . . . if regional conditions, historical circumstances, or cultural patterns prevent the development of explanatory statements which apply to all fifty states, then genuine comparative study is impossible and general explanatory models have little value. . . . Certainly the inclusion of the 11 Southern states in our research operations will affect the results: but so does the inclusion of any other 11 states.[6]

The removal of any eleven states chosen at random would have no systematic impact upon correlation coefficients unless the states selected happened to form a natural or sociological region within the country.[7] Studies which proceed as if there were no regional peculiarities in data on the states must also make the prior assumption that an increase in party competition, a change in the system of legislative apportionment, or an increase in industrialism will have the same kind of impact on states as dissimilar as Nevada, New York, Mississippi, and Vermont. This assumption may be justifiable for some variables, but it plainly is not justifiable for many of those employed in recent studies. As Hayward Alker has written:

> A good deal of the literature of social science suggests that relationships between variables will be different for data from different geographic or cultural contexts. The best approach to these pos-

J. Brodbeck (eds.), *American Voting Behavior* (Glencoe, 1959), pp. 281–299; Raymond Wolfinger and Fred I. Greenstein, "Comparing Political Regions: The Case of California," this REVIEW (March, 1969), 74–85; and Daniel J. Elazar, *American Federalism: A View From the States* (New York, 1966).

[5] The importance of regions in the American political system has been recognized in several recent articles: Norval D. Glenn and J. L. Simmons, "Are Regional Cultural Differences Diminishing?" *Public Opinion Quarterly* (1967), 176–193; Ira Sharkansky, "Economic Development, Regionalism and State Political Systems," *Midwest Journal of Political Science* (1968), 41–61; and Jack L. Walker, "The Adoption of Innovations by the American States," this REVIEW (September, 1969).

[6] Thomas R. Dye, *Politics, Economics and the Public* (Chicago, 1966), p. 43.

[7] To check on this point a sample of eleven states was chosen from an alphabetical listing using a table of random numbers. Correlations of the Gini Index and several variables were then calculated using these new distributions. There were only small unsystematic changes in correlation coefficients based on distributions of forty-eight states and the eleven state sample, and no appreciable differences at all between the forty-eight state distributions and the thirty-seven state distributions.

sibilities of 'configurative regionalism' is a heightened sensitivity on the part of those who do universalistic analyses to possible regional or cultural differences. Regional specialists, on the other hand, should make continual distinctions between local events and worldwide patterns; they should state their findings in the most universal terms possible. There is a need to use and develop more adequate formal models of the various ways in which particular regional contexts influence configurations.[8]

We can never fully realize the potential of the American states for comparative political research if we attempt to build explanations based on correlations which are the result of statistical artifacts. Dye suggests that the distribution of resources within the state is more effective in shaping the character of political systems than in establishing the levels of public spending and service. The results of Table 1 provide little support for such a generalization; when the regions are taken separately there are as many significant correlations with measures of policy output as with measures of the political systems, and in both cases there are few strong relationships.

One of the virtues of the American states as objects of comparative analysis is that they are different. Some states are highly developed urban and industrial systems, others have vast unsettled stretches of land with total populations less than one fourth the size of New York City; some bear the impact of Southern history and traditions, others the influence of the Western frontier. One of our principal theoretical tasks is the creation of explanations for the way these differences, including regional differences, affect the political processes and governmental systems of the states. A truly universal theory would identify the common characteristics of states and also provide an explanation of their differences.

DENNIS D. RILEY
JACK L. WALKER
The University of Michigan

TO THE EDITOR:

A great deal of the research on state politics in recent years has employed a comparative approach in which propositions are developed and tested against data derived from *all* fifty states. This represents a departure from earlier studies of "New England politics," "Southern politics," "Midwest politics," "politics in Wisconsin," etc. The Riley-Walker note suggests that the field of state politics is ripe for a debate analogous to the "comparative approach" versus "area studies" debate in the field of comparative government. I hope this debate will develop along constructive rather than argumentative lines.

It is true that the removal of the eleven Southern states from an analysis of the impact of inequality on politics and public policy reduces the size of the coefficients. Since the Southern states are the most unequal in the nation, their removal reduces the range of variation in our inequality measure. Removing the Southern states, therefore, reduces our ability to assess the full impact of inequality on political systems. Fortunately for the sake of analysis (albeit unfortunately for many inhabitants), the Southern states *do* provide us the opportunity to observe the political correlates of inequality. Their inclusion in our analysis permits us to view relationships between inequality and politics which would not be observable in subsets of states where the range of inequality is narrow.

Riley and Walker are quite wrong in saying correlations based on all fifty states are "statistical artifacts." They are no more statistical artifacts than correlations based on 39 states, or 25 states, or any other number of states. Riley and Walker may choose to see whether a proposition developed in a comparative study of all fifty states applies within regional subsets, but such an exercise cannot detract from the validity of the general proposition as it applies to all fifty states. Their Table 1 does not contradict our findings that among the fifty states, where variation in inequality is substantial, inequality is independently related to a number of important political variables.

In short, I am interested in developing propositions based on all fifty states, propositions which describe the full range of system behavior in the American states. Riley and Walker develop some propositions which apply to regional subsets of states. Both are equally valid exercises depending upon the purposes of the researcher.

THOMAS R. DYE
Florida State University

TO THE EDITOR:

It is embarrassing to have to defend the merits of one's own work. I do so chiefly in the hope

[8] Hayward R. Alker, Jr., "Regionalism Versus Universalism in Comparing Nations," in Bruce Russett, *et al., World Handbook of Political and Social Indicators* (New Haven, 1964), p. 323. Also see Alker's discussion of the "Universal Fallacy" and the "contextual fallacy" in his: *Mathematics and Politics* (New York, 1965), pp. 101–106; and the discussion of "interaction effects" in Hugh Donald Forbes and Edward R. Tufte, "A Note of Caution in Causal Modelling," this REVIEW (December, 1968), 1261–1262.

that the objectionable tone like that exhibited throughout Mr. Ashcraft's attack on my *The Liberal Politics of John Locke* (this REVIEW, March, 1969, pp. 191–192) might in future induce the Editor to check whether such a tone does not indicate the libelous character of a review.

1. The reviewer states the absence of "a defensible (historical) definition of liberalism." Even if my treatment on pp. 17–26 of "the complexity" of and "the misapprehensions about liberal theory" (*Lib. Pol.*, pp. 26, 21), were not a contribution towards a re-definition of liberalism, my view of the full picture of liberalism emerges quite clearly since throughout the book I relate each of the components of Locke's politics to accepted notions about the liberal tradition. Ashcraft's slanted choice of quotations is sufficient evidence against his denial of the fact.

2. In his article, "Locke's State of Nature: Fact or Fiction?" (this REVIEW, Sept., 1968, p. 915), Ashcraft offers views on empirical and traditional theory which I, too, express—and apply —in my book. Yet, he completely misconstrues my view on the affinities between "operative democratic theory and Locke" (*Lib. Pol.*, p. 301). He disregards my explanation that, if empirical theory shows "that the democratic process is not as democratic as ideology would have it," Locke was no democrat in the first place (p. 302). Although I refer to Dahl, Kaplan and Laswell (pp. 285, 293) in connection with what remains important in democratic procedures, Ashcraft maintains that from my references to these scholars (and Easton) it follows that they are "the true sons of liberalism." What the reviewer seems to have in mind is liberalism that *inter alia* would not exclude "rule by a Supreme Court or a Poohbah."

3. In all his statements, including those on executive prerogatives, he overlooks that especially throughout part III I make it clear that Locke "is the harbinger of a liberal constitutionalism which is stricter and more unqualified than the constitutionalism with which he was satisfied" (p. 372). Thus, in his derision of what I associate with "'liberal' regimes," Ashcraft fails to take notice of the historical perspective in which I express on p. 293 the view on the adjustability of the liberal principle of the sharing of power to "elitist, authoritarian and extra-constitutional notions." My central thesis that, in denying general suffrage, Locke leaves the people only the right of extra-constitutional dissent, is historically hardly surprising, and the reviewer's fragmented quotations are intentionally misleading. The account of my interpretation of Locke's theory of popular consent—and of other topics—given in the *Times Literary Supplement* of February 13, 1969, shows that my way of interpretation is no hindrance to the accurate evaluation of its contents.

4. As an unprejudiced reading of pp. 32–33 will confirm, I do not refer to the controversies of contemporaries to justify disregard of historical evidence and contemporaneous writings. Detailed comparisons concerning property can be found in Chapter VI. Elsewhere, too, I have evaluated Locke's views in relation to other thinkers (especially Filmer) and to contemporaneous practices. I have also shown that Locke himself viewed issues in historical categories and perspectives, especially one-man rule.

5. The reviewer equally misleads the reader in producing one sentence bearing on a threadbare aspect of economic liberalism, while keeping silent on my attempts to reassess the relationship in the *Treatises* between property and politics and to highlight its principles which permit government intervention, and even a welfare policy, although in practice Locke was not prepared to recommend it. (Chapter VI, 4.)

6. To offer the so far most detailed analysis of Locke's "mature political thought as expressed in the *Treatises*" (*Lib. Pol.*, p. 40), would not seem to be an improper limitation if it distorted or added nothing to our understanding of one of the classics of political thought, or if the analysis were entirely unrelated to the state of political science in our time. Ashcraft's objections are the less convincing since in his "Locke's State of Nature," he does not make a single point which is relevant to Locke's political thought and which the *Treatises* could not bear out. He does not improve his case by clamouring for the reasons of Locke's "change of heart" about revolution, as if Cranston had not broken new ground and Laslett added more emphasis to what follows in this respect from Locke's association with Shaftesbury. Ashcraft does not mention my references to the association in my treatment of Locke's revolutionism (*Lib. Pol.*, pp. 36, 312–13, 364), I do also not ignore or reject the evidence of earlier and other works of Locke when it differs from my reading of the *Treatises* but in these cases I try to explain the difference or I suggest a change of mind (*Lib. Pol.* pp. 152 and 276).

7. Bits of quotations torn out of context are so arranged by the reviewer as to suggest that to distinguish between various senses in which Locke used the notion of the state of nature attests by itself confusion. Ashcraft himself speaks in his article of "the three possible states of nature" (p. 901). This highlights what is essentially wrong with Ashcraft's attitude.

Not all points in his article are different from mine. Yet he refrains from registering any agreement, nor does he dedicate any of the roughly

1200 words of his review to *proving* one single instance where I misread Locke, as distinct from simply ruling, for instance, against the distinction between rebellion and revolution, noted also by other scholars (cf. *Lib. Pol.* note 44, p. 316), or against that between "absolute" and "arbitrary" power. His failure to express agreement or prove a misreading accords with his failure to approximate in his article a decent proportion between the mention of other scholars and the reproduction of their views. (Examples supplied to the Editor.) Ashcraft does not give the slightest indication of what his conclusion owes and adds to existing interpretations. It is no novelty to evaluate Locke's state of nature as "an historical and a moral description of human existence" (p. 898) and to identify the second description as "a critique of existing society by indicating the legal and moral boundaries of political relationships" (p. 914). Yolton and Polin (who mention also Lamprecht) speak of "a normative device" and of "un état normatif," Strauss and Laslett say the same in other words. (References in *Lib. Pol.*, notes 36–37, p. 99 and note 2, p. 83).

Ashcraft's inability or unwillingness to give other scholars' works their due explains the nature of his conclusion that, applied to my book, my own criticism of an unhistorical attitude should prove "suicidal." This wishful thinking reflects itself in the method not to disprove but to cut to measure quotations bearing on a fraction of the topics the book deals with, or rather, like "conquerors' swords . . . cut up . . . by the roots, and mangle . . . to pieces," to adapt Locke's words (II, 211) to the case in hand.

M. SELIGER
The Hebrew University of Jerusalem

TO THE EDITOR:

Every author is, in my opinion, entitled to present an intelligent, clear, and persuasive reply to a critical review of his work. I see no reason why Professor Seliger's failure to avail himself of that privilege necessitates my engaging in polemical argument with him. In addition to the fair treatment of a work owed to the author, a reviewer has an obligation to the reader to advise him on the merits of the book. Given the number of books published annually, the limitations of an individual's reading time, and three careful readings of Professor Seliger's book, I advised the reader that he could better employ his time than by groping his way through the fog-bound thinking of the author.

Aside from the specific textual criticisms mentioned in my review, I pointed out that Seliger had chosen to disregard (a) more than thirty volumes of Locke's correspondence, journals, and manuscripts, (b) the majority of Locke's published writings, (c) numerous works by seventeenth-century writers important to Locke's political thought, and (d) the historical context of events which shaped Lockean ideas. The result is that Seliger set out to write a book on Locke with a seriously inadequate knowledge of his subject and virtually no knowledge of the seventeenth century in which Locke lived. Instead, Seliger throws a few scattered pieces of Locke's thought into a cauldron, and I suggested that from such intellectual alchemy few golden truths were likely to emerge. If the reader is persuaded by Seliger's reply that these defects are irrelevant, he is free to disregard my advice and read the book.

However, in response to Professor Seliger's rather strange notion of scholarship, viz., that original sources may be disregarded with impunity, but secondary sources should be meticulously cited (point 7), I will glady recommend an excellent secondary source on Locke to the reader; namely, Mr. John Dunn's *The Political Thought of John Locke* (Cambridge Press, 1969). As to Seliger's compaints about the brevity of my quotations from his work, I would willingly have provided further illustrations of his confusion, but my space was limited. Professor Seliger had 372 pages to state his case; I was allowed 1000 words. That is a little like trying to cure a diseased elephant with a sugar cube.

RICHARD ASHCRAFT
University of California, Los Angeles

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REVIEW ARTICLE

ON MODELS AND THE FRENCH POLITICAL SYSTEM:

Commentary on Duncan MacRae Jr.'s *Parliament, Parties, and Society in France 1946–1958*[1]

HOWARD ROSENTHAL
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Without exaggeration, it can be stated that the study of French politics has witnessed a certain *immobilisme* in the past half century. Duncan MacRae Jr.'s *Parliament, Parties, and Society in France 1946–1958* marks an important turning point in the application of quantitative methods, specifically in the use of scaling and regression models. In analyzing this commendable work, I attempt to indicate what I believe are nevertheless more appropriate methodological strategies for the study of the French political system. Questions of method, in turn, are paramount to an evaluation of MacRae's substantive interpretations. Insofar as MacRae uses techniques that are being increasingly applied in political science and sociology, this essay may prove of interest to those with more general methodological interests.

The measure of *immobilisme* is that we must return to Siegfried's *Tableau Politique de la France de l'Ouest* (1913!) to find a major innovation along scientific lines.[2] To be sure, the postwar period has not missed "wise" men (Aron, Duverger, Fauvet, Goguel, Hoffmann, Leites, Leuthy, Williams, etc.) on both sides of the Atlantic. But their insights rarely represent theories or models that express skills and techniques required for empirical replication.[3] Some of us will see a certain opening to the study of social systems and authority relations. However, the breach has been small. Wylie's perceptive observations have not led to rigorous analysis. While the speculative conclusions of Crozier's study of the bureaucratic system have interested political scientists, they have failed to adapt his brilliant empirical strategies to the study of the political system. Similarly, Boudon and Davidovitch's simulation of the minor judiciary has not been followed in political science.[4]

Of course, public opinion surveys are an innovation. While students of politics have an obligation to the first pollsters in France, particularly Stoetzel, it must be regretted that polls have been so heavily treated by simple enumeration (marginals and cross-tabulations), a device inherent in Siegfried's work even if applied to aggregate data.

To claim total *immobilisme* would be course be wrong. New methodology exists in works like Converse's analysis of the spatial distribution of party preferences.[5] Nevertheless, the record is far from brilliant.

Against this background, MacRae's attempt to provide a comprehensive analysis of the political system of the Fourth Republic represents a breakthrough.

MacRae's central concern is the most salient aspect of the Fourth Republic: cabinet instability. His thesis is that cabinet instability existed despite a consensual system among the *ministrables* of the inner political club. The *ministrables* tended to support cabinets on critical roll-call votes and managed to take defensive action in thwarting three challenging "surge" movements, Gaullism, Mendesism, and Poujadism. Nevertheless, this club could not form lasting coalitions capable of positive action in many domains, particularly the fatal Algerian situation. Instead, unstable government occurred.

This instability resulted, in MacRae's view, largely from pressures put upon deputies by *ideological* party militants. These pressures broke the leadership consensus by either dividing parties

[1] St. Martin's Press, New York, McMillan & Co., London, 1967, xiii + 375 pp. Further unreferenced page numbers refer to this work.

[2] Second edition, Armand Colin, Paris, 1965.

[3] The exceptions are disappointing. Thus, Goguel has basically continued the Siegfried tradition without modification. Leites, who brilliantly draws attention to the psychic world of deputies, fails to indicate how he samples language and fails to relate the psychic world to other influences such as constituency pressures.

[4] Raymond Boudon, *L'Analyse Mathematique des Faits Sociaux*, Paris, Plon, 1966.

[5] Authors mentioned above and not cited are referenced in MacRae's bibliography.

internally in National Assembly votes or, in the case of the Socialists, a party with relatively disciplined voting, forcing party leadership to desert the governing coalition. These ideological defections might have been acceptable were it not that generally one-third of the Assembly votes were "unavailable" to the central consensus.

The unavailable votes were produced by election outcomes. Although voters were, MacRae argues, even less ideological and politicized than those in the United States, the governing parties could not enlarge upon their clientele since France's social divisions were constrained by a vertical communications system. Consequently, each of the stable parties aimed its communications at a small range of voters. The Communists, the major bloc of unavailable votes, also had a specific clientele. Shifts in opinion were channeled into the "surge" movements cutting across social cleavages to add to the unavailable votes. While the social cleavages responsible for voting behavior were rather unrelated to the ideologies of militants, both factors contributed to cabinet instability.

MacRae's thesis, it should be noted, has no explicit concept of a "rational" deputy: one who is trying to meet various aspiration levels of political power such as reelection and cabinet status. In fact, *Parliament, Parties, and Society* rejects the hypothesis that instability resulted because the *ministrables* themselves engaged in gaming behavior designed to further their personal ambitions.

The strength of MacRae's work lies in his effort to link the Parliamentary system to the behavior of voters and militants. The latter category is the book's weakest link, for little empirical research is reported. MacRae infers the ideological character of militants only from the high levels of party membership and partisan political activity in France and from descriptive reports. Voters are studied through analysis of survey data and multiple regression analysis of election results. Finally, the book's major empirical work is a scale analysis of roll-call votes in the Assembly.[6]

The scale analysis is carefully done and takes its place beside MacRae's earlier work on the American Congress.[7]

In the present work, MacRae has computed separate scales for each of the major parliamentary groups for each legislature. For a given group, roll-calls where the group had less than 90% consensus were considered. MacRae's interpretations of most of the scales is that they are "issue-specific." Consequently, he sees roll-call voting as being organized along ideological patterns. In applying scale analysis to roll-call voting, MacRae has made a major innovation in the study of parliamentary bodies.

The usefulness of this innovation and the remainder of MacRae's analysis is aptly demonstrated by his treatment of the religious issue. Most observers simply relate greatly attenuated Catholic-laic antagonisms. Leites very much ridicules their importance.[8] A much more precise and correct analysis of the religious question is available from MacRae's work.

As for voters, MacRae shows that religiousity remained important, perhaps foremost, among the divisions that rigidified party clienteles and led to the unstable "political arithmetic" of the Assembly.[9] MacRae also argues that the religious question was more important locally than at the national level. Consequently, while deputies may have taken a consensual approach, they may have been pressured by militants, eg. public school teachers, into more rigid positions.[10] Through scale analysis, MacRae shows how Radicals were consistently split on this issue. Even more critical was the Socialists' total opposition to the bills providing funds for church

[6] The scales represent heuristic clusterings of roll-calls. All items in a scale have Yule's Q coefficients of .8 or better with one another. While better scaling procedures, particularly multidimensional scaling, might now be available, I present no critique on this topic given the development of data processing at the time MacRae carried out his research. Also, while MacRae largely selected roll-calls on the basis of *L'Année Politique* and while a better selection might have been made, I assume the improvement would have had only a marginal impact on the results.

[7] *Dimensions of Congressional Voting.* University of California Publications in Sociology and Social Institutions, Vol. 1, No. 3. Berkeley, University of California Press, 1958.

[8] Nathan Leites, *On the Game of Politics in France,* Stanford, Stanford University Press, 1959, pp. 29-32.

[9] This finding is held subject to the reservations about the multiple regression analysis made below. On the continued importance of religious practice for voting behavior, see Howard Rosenthal, "The Popularity of Charles de Gaulle: Findings from Archive-Based Research," *Public Opinion Quarterly,* Summer, 1967.

[10] Specific data on such local pressures is now available from a survey of Fourth Republic politicians conducted by the French Institute of Public Opinion (IFOP). See Guy Michelat and Howard Rosenthal, "Studies of Electoral Coalitions in the Fourth Republic," Graduate School of Industrial Administration, Carnegie-Mellon University, mimeo. 1968.

schools passed under the Pleven government in the Second Legislature. This defeat likely influenced those in the party who forced their consensual leader, Guy Mollet, to bring about Pleven's fall. Thus, both directly and indirectly, religious matters had real import for Fourth Republic politics.

The religious question clearly indicates the utility of MacRae's modes of analysis. He also adds to our knowledge of the degree of imbalance between shifts in governments and their policies and shifts in public opinion; the types of issues that divided the parties; and several other substantive points. As to his more general hypotheses about cabinet instability, if the elements of his view have been advanced in the past, less analytical writers are far from a consensus. Thus, Fauvet and Leites have taken opposing views regarding the strength of the influence of ideology. I would suggest that MacRae's interpretation is correct: ideology counted, mass social cleavages counted, and a consensus operated among the *ministrables*. On the other hand, MacRae's picture is far from complete; other factors, including personal ambitions of deputies, may well have influenced cabinet instability. This lack of completeness or balance—which is part of the inherent approximation of research and which does not suggest that one refrain from giving this book a careful reading—leads to a number of criticisms. Hopefully these criticisms constitute useful research strategy. While, for reasons of brevity, these criticisms are focused upon MacRae's scaling analysis of roll-call votes and regression analysis of party voting, the criticisms would apply to the analysis of social cleavages and other topics considered by MacRae.

Simply reinterpreting MacRae's roll-call analysis in a different framework raises some doubt as to his emphasis on ideological factors in the fall of cabinets. Two points must be considered: which deputies were scaled and which roll-calls were scaled.

As to the deputies, among the non-Communists, about 50 were excluded from scaling analysis for the First Legislature and over 100 in the Third.[11] In the Second Legislature, the dissident Gaullists, the ARS, were not scaled.

The ARS began to break with De Gaulle's RPF by supporting Pinay, the first Moderate premier since the Liberation. Perhaps the ARS dissidence was precipitated by support for Pinay's conservative policies, but the non-ideological issue of opportunistic participation in the spoils of Fourth Republic government had long been latent within the RPF. The ARS was rewarded with cabinet participation in the subsequent Mayer government while the loyal Gaullists waited until Laniel in order to join the club.

It is not denied that the ARS members were on the "Right," with respect both to the entire political spectrum and to the Gaullist movement. Their conservatism may explain why they deserted De Gaulle over Pinay rather than Pleven or Faure, but it does not explain why they failed to vote the investiture of another Moderate, Petsche, at the beginning of the Legislature. It can be advanced that the ARS simply wanted to ride the winning tide of Gaullism; when Gaullism lost its force, as good "gamesmen" they sought better fortunes.

Similarly, the unscaled deputies in the other Legislatures may well have been maneuvering with personal ambition foremost in mind. Some of them were gentlemen who shifted their party identification while others belonged to small parties. At the margin, their votes were important, and they must be studied before formulating any conclusions about the Assembly.

The question of omitted deputies, however, is probably less important than that of omitted roll-calls. In fact, a large proportion were not scaled. In only one case, that of First Legislature Moderates, where 28% of votes involving dissidence within the party did not enter scales, does MacRae provide precise information on this point.[12] Elsewhere, the proportion seems lower; thus, of divided MRP votes, about 78%, 51%, and 86% were not scaled in the First, Second, and Third Legislatures, respectively.[13]

What interpretation can be placed on these

[11] These figures are estimates made from various tables as MacRae does not give precise figures. Even if separate analyses were carried out by party, it would have been useful to have a scale analysis of all deputies to treat the problem of unscaled deputies. Such an analysis would also have presented a dimensional portrait of the parties. Consequently, one would have been able to handle more precisely the question of the left-right position of the Radical, MRP, Moderate, and Gaullist blocs. One could also relate such a "mapping" to the various government coalitions. Michael Leiserson has made an initial attempt in this direction. See his "Political Coalitions," unpublished doctoral dissertation, Yale University, 1966.

[12] P. 96. On p. 181, MacRae unfortunately leaves the impression that only unanimity was a reason for a vote not being scaled. Such is obviously not the case; many roll-calls were not scaled because they did not fit into a cluster.

[13] Based on Tables 3.6, p. 56; 4.5, p. 104; 5.5, pp. 146–147; 6.5, p. 174.

roll-calls that were not shown to be strongly related to general patterns of party division? Rarely, as in the case of the school aid question in the Third Legislature, they represent an important division that was infrequently put to a vote. More frequently, these roll-calls may have represented different dimensions for different deputies. For example, one deputy might interpret a roll-call as "European" while another sees it as "economic." Even in this case, such differential perceptions would hardly suggest widely shared ideological structures. A third possibility is specific questions of interest, sugar beets, etc. But such roll-calls would certainly show that deputies and militants are pursuing non-ideological interests of economic, electoral, and other types. Finally, the non-scaled roll-calls can indicate specific gaming maneuvers within the coalition system. (However, the "ideological" explanation might still be retained through multivariate techniques not used by MacRae. See the remarks on methodology, below.)

For these numerous non-scaled roll-calls, it is especially important to look at their distribution over roll-calls of obvious importance to government instability. Here I include roll-calls where a premier is invested or rejected, where his cabinet is approved or rejected, and where he falls or chooses to resign.[14] Since party divisions on these roll-calls are presented graphically by MacRae, we can only gauge approximately whether a sufficient proportion of the members (10%) voted in such a way that MacRae treated the vote as expressing dissidence.[15]

On roughly 111 occasions a scaled group—Socialists, Moderates, MRP, and Radicals plus UDSR in all three Legislatures, the Gaullist URAS in the Second—divided on these critical votes. Since only 39% of these divisions entered into scales, conceivably a majority of the divisions expressed ample "gaming." The pattern holds even for the relatively "ideological" groups, the MRP and the Gaullists. Only 8 of 19 MRP divisions and 3 of 11 for the Gaullists were scaled.[16]

These aggregate results must be focused in terms of the history of coalition formation and breakdown. For the First Legislature, MacRae rightly argues that the Socialists were responsible for the fall of cabinets. In turn, the Socialists were split internally on ideological lines. But one needs more information on the extent to which ideology was more than a symbolic overlay for power struggles within the party, Mollet being particularly concerned with his personal position. Furthermore, MacRae himself raises the possibility of dissent within the party coming from conditions of electoral competition within constituencies as well as from the personal beliefs of militants.[17]

In addition to the Socialists, one ought to look at the Radicals whose support was generally essential to any First Legislature coalition. A mark of Radical power in coalition formation was their ability to receive cabinet posts in greater proportion than the number of Radical votes supporting the cabinet.

At the beginning of the Fourth Republic, after uniting on a series of investiture votes, the Radicals split on Ramadier's fall and Blum's subsequent investiture attempt. Then unity was refound on the investiture of Schuman but readily lost when his cabinet was presented five days later. The first two of these divisions are not part of any scale. The division over Schuman belongs to a scale (I-2 Marie-Queuille) that is not particularly related to specific issues but includes all investiture and cabinet votes until the vote on Queuille's first cabinet. All scale items fall in the period November, 1947, July, 1949. Thus, it might be suggested that Ramadier's fall marked the beginning of coalition bargaining that yielded a relatively stable pattern with the Schuman cabinet.

Nevertheless, the pattern showed signs of collapse over Schuman's fall, which was not scaled. After Queuille's first investiture, the Radicals could not be scaled on any of the "critical" votes in the remainder of the Legislature. If united on investitures, dividing only over Mollet and Pleven, the party split, and *split differently* at the end of the final three governments to fall Queuille, Bidault, and Pleven.[18]

This overview of Radical voting suggests that the Radicals engaged in coalition in-fighting MacRae's scale analysis has the merit of isolating a period when alignments were relatively stable. It would be possible for the most success-

[14] This categorization omits critical roll-calls during the life-time of a cabinet. Such roll-calls, which could be spotted by more detailed research and which would present great interest in the general study of instability, are not necessary to the points made here.

[15] Although some roll-calls which appear on the graphs to contain 10% dissidence may not have been scaled because some of the dissidents did not stay within the group for the whole Legislature, such dissidents, as suggested above, bear close watching for "gaming" behavior.

[16] In the entire Fourth Republic, the Socialists divided on only two "critical" votes, the Thorez and De Gaulle investitures.

[17] P. 292.

[18] Queuille's final government simply terminated with the elections for the Second Legislature.

ful bargainers to have paid for their rewards by supporting the government while the least successful attempted to demonstrate their power by casting negative votes. From this viewpoint, which still permits gaming, finding an association between *ministrable* status and support for cabinets is not surprising.

As for the other parties, the Right was heavily divided throughout the Legislature, splitting on 24 of the 31 "critical" roll-calls. When these Moderates split on a roll-call that was scaled, they may have done so along ideological lines, for 7 of the 10 such roll-calls belonged to their general left-right scale (I-1). However, 14 roll-calls were not found in any scale. Similarly, when the MRP divided it did not do so in ways that led to scaling (5 of 6 cases). In summary, even if we go so far as to admit that First Legislature governments fell from the Socialists' ideological disputes, instability may also have resulted from gaming over the composition of governments.

In the Second Legislature, the MRP was more divided than in the First although these divisions may have reflected splits on issues rather than coalition maneuvers as 7 of the 11 splits were scaled. Yet the scaled roll-calls were scattered about four scales, the major contrast being between votes on Mendès-France and votes on other premiers. In turn, MacRae admits that Mendès was fought as much for his unwillingness to abide by the rules of the club as for ideological reasons.

The Moderates were less divided than in the First Legislature, but 7 of their 11 divisions could not be scaled. The two scaled votes on Mendès investiture seemed to be specific reactions to Mendès while the fall of Mendès and of Pleven were related to key foreign policy issues.

Continued suggestion of Radical gaming comes from 9 roll-calls that could not be scaled. If three scaled roll-calls bear some relation to issues, three others (Mendès' fall and Faure's second investiture and fall) are related to a specific coalition struggle stemming from the club's unwillingness to submit to the popular Mendès.

For the Second Legislature, however, MacRae rests his case on the Gaullists, who caused the fall of most governments. If the eventual members of the URAS were unified until Mayer's fall, thereafter they seemed to have the usual ambitions of the system. After Mayer's fall there was a prolonged crisis of one month when four candidates, Reynaud, Mendès, Bidault, and Marie, all lost investiture votes. Although the Gaullists divided on all of these votes, none was found in a scale. The three votes on Laniel were scaled, but on three distinct scales, one of which was simply titled "disaffection from Laniel"; thus, gaming can not be ruled out here. Finally, none of the succeeding divisions on Mendès, Pineau, and Faure could be scaled. Consequently, for critical votes, MacRae's results do not suggest that Gaullist splits were primarily ideological but rather that without De Gaulle there is no Gaullism.

In fact, MacRae's conclusions concerning Gaullism appear to follow more from preconception than from empirical research. When, for example, he attempts to relate roll-call voting to constituency factors, instead of a set of research results, we find the assertion:

> We should expect the constituency relations of the Gaullists, as inferred from their scale positions after they began dividing on roll-calls, to have been loose. In its early years, the Gaullist movement was ideological, and the most enthusiastic Gaullists took their stands as a matter of personal conviction than because they came from "Gaullist districts"—or conformed to them.[19]

In a recent essay, however, I have indicated that the Gaullist scales are rather strongly related to a measure of electoral interest in the constituencies. I have also shown that measures of electoral interest are closely related to Gaullist choices in electoral coalitions under the *apparentement* system.[20] These findings suggest

[19] P. 299.

[20] Howard Rosenthal, "The Electoral Politics of Gaullists in the Fourth French Republic: Ideology or Constituency Interest?", this *Review*, June, 1969, pp. 476–487.

Other limitations of MacRae's analysis of constituency relations are: (1) While he suggests that party militants limited bargaining at the local level (pp. 11, 289–302), study of *apparentement* alliances for the 1951 elections shows that the "highly organized" MRP formed the most coalitions. Another type of coalition, joint lists among two or more parties, in turn seems as related to the size of their vote as to their membership ratios. See Howard Rosenthal, "Voting and Coalition Models in Election Simulation," in William Coplin, ed., *Simulation and the Study of Politics*, Chicago, Markham Press, 1968; (2) Generally, the treatment of the role of electoral interest vs. that of ideology is too superficial. Thus, MacRae interprets (p. 176) an association between scales specific to issues and scales specific to cabinets as supporting evidence for the lack of a game and infers that high specificity was a characteristic of "ideological" parties. A "perfect" data point in this respect is represented by the Socialists' unique First Legislature scale which was entirely specific to one cabinet and to one issue, the electoral law. Yet MacRae only hurriedly investigates (pp. 290–

that non-ideological considerations are important to the analysis of Gaullism, even as early as 1951.

Similarly, MacRae finds that Gaullism as a "surge" movement had "politically unskilled" leaders.[21] Further on, although forced to note that the Gaullist parliamentary group indeed tended to come from skilled occupational groups, he argues, "they lacked the skills of negotiation and compromise."[22] This contention seems to revolve about De Gaulle's failure to come to power before 1958.[23] But are we now to believe that in the Fifth Republic the Gaullists are skilled and the unsuccessful men of the Fourth unskilled? By rejecting the Fourth Republic, the Gaullists had to appear as uncompromising. At least their local coalition choices were well in keeping with the skill of electoral rationality. Thus, it seems dubious that the Gaullists were a group of unskilled ideologues who caused the instability of the Second Legislature.[24]

The Third Legislature, in contrast, comes closest to supporting MacRae's emphasis on ideology. The Moderates, who were primarily responsible for the fall of governments in this brief legislature, were divided at the fall of Mollet, Bourgès-Maunoury, and Gaillard. The latter two votes as well as Pflimlin's investiture on the *treize mai* belong to two scales dealing with the Algerian issue, and Mollet's fall is more related to the "Algeria" scale than any other.[25]

Algeria was also important for the Radicals. Although the vote ending Mollet's reign fell on the Europe scale, both Bourgès and Gaillard fell on two highly associated scales that MacRae labelled "Mendesism" but could as well go under "Algeria."

Still, gaming was perhaps not absent, for the Moderate votes on three investitures and the Radical votes on four were not members of scales. While the MRP only divided on two investiture votes among the "critical" roll-calls, neither of these entered the single MRP scale for the Legislature. It is perhaps of consequence that the Moderates and MRP, excluded from the first two cabinets, participated in the Gaillard government. Furthermore, as MacRae himself contends that "a more fundamental reason for the Moderates' defection [from Mollet] was their feeling of growing strength in the country," ideological motivations were certainly not unrelated to tactical considerations.[26]

If the foregoing successfully suggests that ideology's role in cabinet instability may not be as secure as MacRae would like us to believe, doubts arise regarding not only the interpretation MacRae gave his results but also the methodology used to obtain the results. In fact, the foregoing reinterpretation must be regarded as provisional, given MacRae's methodological structure.

In effect, MacRae initiated scale analysis of all divided roll-calls and afterwards inspected the results in relation to the fall of cabinets. It would perhaps be preferable to treat the critical votes as a series of dependent variables. Then, in terms of roll-calls, one could see what other votes clustered with each critical vote. In MacRae's heuristic, a critical vote is unfortunately excluded from scaling if it fails the necessary association level for one or two votes in an existing cluster. The method suggested here would permit focusing directly on the correlates of each critical vote.

Another alternative and perhaps superior strategy would be to perform a scale analysis on all roll-calls except critical roll-calls and to then develop multivariate measures of association between the scales and the critical roll-calls. Within such a model, one could also accommodate measures relative to past critical votes, to the constituencies, to ministerial rewards, etc.[27]

291) the extent to which Socialists were simply voting their electoral interests on this issue. While awaiting such an examination, an alternative proposition can be advanced; namely, that since specificity seems to be related to the number of votes scaled, it is primarily related to the extent of party discipline rather than ideology. Since, when discipline breaks down, it may tend to do so during a brief period of time over a single question (which may be either ideological or tactical), cabinet and issue specificity may be associated. Hence, neither of these measures by themselves can be expected to reveal much about either ideology or coalition formation.

[21] P. 9.

[22] P. 53 and n.35, p. 63.

[23] Pp. 308, 313–315.

[24] Similarly, it is doubtful that the Mendesists were unskilled.

[25] P. 169.

[26] P. 161.

[27] With respect to ministerial rewards, MacRae has only considered differences in support between *ministrables* and non-*ministrables*. He thus has no measures on those who aimed at office but were unsuccessful. He also did not report on the relationship of voting to the allocation of posts over time.

In any case, his inference of a consensual system from ministrable support for cabinets can be questioned in that examination of Table 7.2 (p. 187) discloses that the association between *ministrable* status and cabinet support was strong only for the First Legislature. Furthermore, on p. 193

Such a strategy would give a more comprehensive view of behavior on critical votes and, in particular, it would remove much of the vagueness now present concerning the non-scaled votes.

The argument for a methodology centered about dependent variables also applies to MacRae's regression analysis of the geographic variation of the vote over the French departments.[28] Again, instead of creating an appropriate model for each dependent variable, he uniformly applies the same set of four independent variables to seventeen voting outcomes. For the stable parties, this method is moderately successful with "explained" variance (in terms of squared Pearson correlations) ranging from 27% to 52%. Much lower levels were obtained for the Gaullist gain in 1951 (26%) and the Poujadist vote in 1956 (19%).

Even if such a technique has comparative interest, its danger is that, if the model is incorrectly specified, one will generate improper interpretations from the resulting coefficients. An example is available by comparing a more elaborate study of Poujadism to that of MacRae.[29]

In the former, first a simple regression was run using "ruralism" (defined as per cent of the labor force in non-agricultural occupations) plus six other independent variables: % independent non-agricultural proprietors; % Communist vote in the 1951 elections; wine-growers as % of male labor force; home distillers as % of male labor force; automobile registration per capita; North-South based on the Vichy occupation line. As a simple regression, there is an adjusted squared multiple correlation of .43 as against .19 for MacRae.[30] While MacRae reports "ruralism" (which he defines somewhat differently) as the most important correlate of Poujadism, in the seven variable analysis, "ruralism" has no "significant" relationship to the Poujadist vote, significant coefficients being found only for home distillers, automobiles, and North-South.

To go one step further toward proper specification, another seven variable model was run incorporating a number of detailed hypotheses about the geographic variation of the Poujadist vote. While the Communist vote again entered the equation, the Communist vote weighted by Ruralism was introduced to capture the competition between Poujadism and Communism in rural areas. Thus, while the former term had a positive partial correlation with the Poujadist vote, the latter was negatively related. Similarly, the two alcohol variables were multiplied (weighted) by automobiles, expressing the hypothesis that discontent must be related through communications. Finally, automobiles, independent proprietors, and North-South were all weighted by ruralism to express the hypothesis that these effects were strongest in rural areas. This model, in which all coefficients are "significant" has an adjusted squared correlation of .53. Although many elements of this model are mentioned in passing by MacRae, his failure to give them explicit statistical analysis leads to a fragmentary intepretation of Poujadism. His interpretation of other voting phenomenon should also suffer from this tendency to compare associations rather than specify models.

MacRae's preference for general patterns of association may be a deliberate avoidance of complexity as the book has been carefully written to reach those without mathematical knowledge. Following a preface offering some uncomplex "conjectures" that can be "perceived" by the "nonstatistical observer," the book frequently resembles the "wise" man's argument for a particular thesis rather than an attempt to provide a rigorous statement of the measure of multiple influences on complex behavior.[31] Of

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MacRae incorrectly interprets data in support of his hypothesis:

> For one *ministrable,* Antoine Pinay, there is additional evidence of his support for cabinets of which he was a member. Pinay's friends constituted a well-known group . . . four of the seven are in the top row on Scale II-2. They were somewhat more favorable to the cabinet than other members of the party.

We can hardly believe this since 43 of the total 63 Moderates scaled were also in the top row (see Table 7.4, p. 192).

[28] Actually 82 departments or dioceses, except for the Poujadist vote where there were fewer units. MacRae originally published these results in the *American Journal of Sociology* in 1958. While it may now appear subject to reservations, MacRae's study was undoubtedly instrumental in stimulating the use of multiple regression in French political studies.

[29] See Howard Rosenthal, "Poujade's Voters: Suggestions for the Study of the Social Geography of the New French Politics," Carnegie-Mellon University, Graduate School of Industrial Administration, mimeo., 1968. This study further raises the point that beyond the particular case of "ruralism" discussed below, MacRae did not consider that Poujadism might represent a more complex reaction to modernization than the simple "defense" of *"situations acquises"* (p. 12).

[30] MacRae does not indicate whether his correlations are adjusted.

[31] P. vii. The lack of rigor seems especially true for the comparisons MacRae draws between France

course, although "nonstatistical observers" may think in black and white, one need not research and write for such an audience.

Beyond questions of audience, the methodological problems of *Parliament, Parties, and Society* reflect a lack of theoretical underpinnings. While there is some reference to sociological theory (albeit in such non-operational terms as "structure," "norm," "status," and "ideology"), there are no references to what might be termed the "neo-rational" school of political science. No mention, for example, is made of the Simon-March-Cyert work on organizations, and, particularly, incrementalism.[32] Yet their conceptual framework has heavily influenced the work of Boudon and Crozier in France and, to speak specifically of a legislative process, the work of Davis, Dempster, and Wildavsky on budgetary processes in the United States.[33] Similarly, there is no reflection of marginalist explanations of voting behavior beginning with Downs nor of the growing interest in coalition theory proceeding from Riker.[34]

To suggest one way in which "neo-rational" thinking is likely to be of help, consider the first proposition MacRae would question: Cabinets tended to be brought down by the operation of a parliamentary game in which prospective ministers were the center of opposition.[35]

MacRae's main attack on this "conjecture" comes from finding that *ministrables* tended to support cabinets more than non-*ministrables*. But setting up a "conjecture" and dealing with it so simply is a quite narrow tactic. Surely one would be better to assume that cabinet status was an important goal for at least some deputies —MacRae does not indicate the contrary. How, one would ask, did deputies implement this goal? Once that question is answered, one could investigate the consequences of such behavior for cabinet instability.

One would also investigate how the cabinet status goal interacts with other goals, particularly reelection. If, for example, the *ministrables* have more secure electoral bases that non-*ministrables*, they can afford to support the cabinet even when it might mean losing some voter support. This would generate differential levels of support for the government without excluding "gaming" at opportune junctures.

Goal interactions should be related to the characteristics of roll-calls. Among the possibilities: (1) investiture votes, which focus on little-known personalities, may be less salient in the constituencies than votes on intense issues that lead to the fall of governments. Consequently, one might expect "gaming" for the cabinet status goal to be more important on the former. (2) On matters where a positive vote and a negative vote would have equal effect on the reelection goal, a deputy would be relatively free to vote according to personal ideology. (3) Even if a deputy would be inclined, *ceteris paribus*, to vote in line with his constituency interest, his vote is likely to be influenced by his past support for the current government.

The type of approach that has just been suggested can be extended to the other "conjectures" considered by MacRae. In general, we need behavioral models of the mass "market" of voters, party organizations, and the Assembly itself. Without having achieved such objectives MacRae has prompted their pursual.

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and the United States. The qualitative differences between, say, lack of white-black communication in the U.S. and *ouvrier-patron* in France are so important that it is very risky to rely on the types of statistical tables now available for international comparisons and thereby establish such propositions as, "In personal relations and social structure, France is more divided than the United States" (p. 6).

[32] James G. March and Herbert Simon, *Organizations*, New York, John Wiley, 1957; Richard M. Cyert and James G. March, *Behavioral Theory of the Firm*, New York, Prentice-Hall, 1964.

[33] Otto A. Davis, M. H. Dempster, and Aaron Wildavasky, "A Theory of the Budgetary Process," this *Review*, 60, 1966, pp. 529–547.

[34] Anthony Downs, *An Economic Theory of Democracy*, New York: Harper & Row, 1957; William Riker, *The Theory of Political Coalitions*, New Haven, Yale University Press, 1962.

[35] P. vii.

BOOK REVIEWS

Public Policymaking Reexamined. By Yehezkel Dror. (San Francisco: Chandler Publishing Company, 1968. Pp. 370.) *Political Science and Public Policy.* Ed. by Austin Ranney. (Chicago: Markham Publishing Company, 1968. Pp. 287.

TWO VIEWS

"Public policy" is a historic concern of political scientists. Two books that reflect current concerns with policy are *Public Policymaking Reexamined* by Yehezkel Dror and *Political Science and Public Policy* edited by Austin Ranney.

It is not very often that we see a book like Yehezkel Dror's *Public Policy-making Reexamined.* Dror offers a great leap forward in the analysis of public policy. His scope is huge and his ambition is extensive. He tries to encompass the whole field in this volume, not merely for the sake of description but for the purpose of assessing the present quality of policymaking and proposing major reforms. The book warrants the label "definitive" insofar as it defines a full sweep of concerns that are held in common by political scientists interested in normative *or* behavioral approaches to public policy. Dror does not fill all the segments of this field, however, and some of the gaps raise serious questions about the larger enterprise.

The field that Dror's book defines is "policymaking." This includes the substance of policies that are considered and implemented by governments, the structures and behaviors—both official and private—that combine to make policy, plus other structures and behaviors that shape and modify policymaking procedures.

The breadth of Dror's book ranges from first principles through empirical findings, a major reform proposal, and detailed prescriptions. The most elementary of Dror's "first principles" is his distinction between homo sapiens and the primates on the basis of the ability to "make conscious and rational decisions, at least part of the time." His concern for empirical findings is more often asserted than demonstrated; he excuses himself from paying greater attention by alleging inadequacies in the literature.

At best, the empiric study of policymaking is just now emerging. A significant and increasing amount of work is being done on minor decisions and secondary policies, but most of it suffers from its lack of comprehensive, theoretical frameworks. Very little empiric work is being done on the macrosystem of public policymaking. At present, even suitable research methods are conspicuous by their absence.

At this and other points, Dror should plead his lack of familiarity with much of political science, especially that dealing with American legislatures, legislative-administrative relations, voting and attitudes, and the newer varieties of comparative state and urban policy-analysis. Such authors as David Easton, Richard F. Fenno, John P. Crecine, Warren Miller, Donald Stokes, Heinz Eulau and Thomas R. Dye have contributed far more to our understanding of policymaking than Dror acknowledges.

The central feature of Dror's book is his "optimal model" for policymaking. This is both a standard against which he compares existing modes of policymaking, and the goal of his reforms. Dror is especially critical of those policymaking procedures he calls "muddling through" and "incrementalism." These have insufficient concern for rational assessment and control of policymaking. "The most harmful effect of the incremental-change argument (which denies the possibility that significant improvements could be made in public policymaking by some innovative jumps) is that it paralyzes efforts, and thus tends to be a self-fulfilling prophecy." Dror describes his own model in several places, but qualifies himself often enough to complicate the tasks of identifying and assessing it. The basic features of Dror's model resemble the components of "rational decision-making" that have been abstracted before by Charles Lindblom and others. To his credit, it should be said that Dror accommodates many of the *caveats* pertaining to pure rationality. He recognizes the high costs of information, and admits the need for cutoff points in assessing alternative policies. He concedes the need for "extrarational" components in policymaking, and suggests a pragmatic mixing of political and economic feasibility in the criteria that are used to select the "best" policies from those available. His concern for "extrarationality" in policy-making, a continuing examination of policy-feedback, and the redesign of policymaking in the face of this examination are central features of his proposal. "Extrarationality" appears to be little more than "creativity." It is an ability to leap from empirical evidence about existing concepts and offer a new proposal that will make "progress" in an attack against intractable social, economic

or political difficulties. Dror uses "intuition" and "judgment" to characterize his concept of extrarationality, and shuns the connotations of "mysticism" and "charisma."

The major question about Dror's herculean task concerns its success: Do his proposals fit into the empirical literature about policymaking, and prescribe a set of goals that are consistent with existing knowledge? Because Dror does not give adequate coverage to the empirical literature a reader must evaluate the book partly with his own sense of what is feasible. Indeed, Dror sometimes acknowledges a casual departure from his own understanding of the empirical literature, and thereby shakes the foundation of his own proposals.

> My findings will be based on a survey of both "literary" and scientific literature and on my own subjective impressions . . . I will regard them as being in the range between "suggestive" and "highly probable" . . . I am not going to keep my factual findings and evaluation formally separate. I will present my findings in a concise, generalized form, without paying what would usually be the necessary amount of attention to exceptions, deviations, and subvariations.

As might be expected from this excerpt, a reader occasionally flails at Dror for making an overly-simple assertion, or reaching a specific recommendation with no basis of analysis.

Dror's analysis suffers from his lack of "the necessary amount of attention to exceptions, deviations, and subvariations." In drawing out a model of muddling through and incrementalism as key features in a policymaking system that he wishes to reform, he does not fully explore the opportunities for innovation within incremental procedures. A system that makes changes incrementally might not be stricken with rigor mortis. Just within the recent history of the United States, there have been dramatic changes in public policy toward minority groups, capital punishment, health insurance, and the use of federal resources to support educational programs in both religious and secular schools. While the spending of state and local governments may change according to the routines of incremental budgeting, the "incremental" growth has been continuous and substantial. Between 1963 and 1967, the combined spending of state and local governments (per capita) increased by 33 percent. Dror himself acknowledges that policy situations may lend themselves to strategies of minimal risk or innovation; and if the former should lead to incremental change. Thus, Dror overdraws the conservatism of incrementalism, and then concedes his reform will have room for incremental decision-making. A reader is left with questions that require more specificity than Dror provides. *What are the worst squeak points of incrementalism? What are their implications for the nature of policy? What conditions require the use of incremental or "optimal" policymaking?* These questions require an exhaustive concern with "exceptions, deviations, and subvariations," and not a disregard for details.

The collection edited by Austin Ranney does not lend itself to the thematic analysis applied to Dror's *Public Policy-making Reexamined*. Its original essays treat various aspects of the policy process, but do not reflect a continuing line of reasoning. Partly for this reason, it complements Dror's book. While Dror offers a comprehensive (and somewhat oversimplified) outline of policymaking, Ranney's collection offers many limited essays that reveal the rich variety of questions, theories, data, research techniques and findings in the policy field.

As its title suggests, the collection is meant to identify the contribution of political scientists (qua scientists) to the *understanding* of public policy, and to its *formulation, implementation* and *evaluation*. Its twelve essays suggest the range of talents that political scientists can bring to these tasks: as developers of theoretical frameworks that help identify crucial lines of influence among the features of the policy process; as describers of governmental structures and political behaviors that are likely to affect those who make policy, and inhibit certain kinds of policy proposals; as assessors of specific policies, to determine which interests they benefit, and whether they accomplish the goals which they claim; and as evaluators of reforms proposed for the policymaking process, to identify those features that are likely to arouse antagonism from policymakers., and those features that are likely to have consequences unforeseen by the reformers.

Aaron Wildavsky looks at several policymaking techniques that have been devised, primarily by economists: systems analysis, program budgeting, cost-benefit analysis, and planning-programming-budgeting. He makes some of the arguments that have been identified as "that political scientist's" rebuke of reformers who disregard central features of the political system. He writes that goals are evolved as the products of interactions among participants, and may be submerged within agreements about specific policies. To upset the customary ways of making decisions about goals and policies would threaten important—if informal—mechanisms. Policymakers must protect their reputations and their access to other officials with whom they share governmental authority, and refrain from

offending citizens in ways that might provoke civic discord and undermine the legitimacy of the political system. Although the economists' concern for efficiency in providing services, and their weighing of tangible benefits and costs offer information of great value to the policymaker, their usefulness seems bounded by the "political" considerations that policymakers use to protect themselves from one another.

Ralph Huitt adds more to our understanding of constraints on policymaking in his discussion of "political feasibility." Huitt sees policymakers' use of economic rationality limited by the complexity of their goals and by the decentralized nature of the political system. The simple decision rules associated with planning-programming-budgeting have had relative success in the Defense Department because the goals of that organization are comparatively clear and noncontroversial (deterrence of war, defense of the country, and victory in war). Policy planners in the military may compare alternative policies for their relative costs and benefits, while officials in other units are reluctant to announce the probable long-range accomplishments of their programs in order to avoid conflicts among policymakers who can support specific activities. The decentralized nature of the political system requires policymakers in all units to be sensitive to the demands and constraints imposed on them by others. Huitt has both academic and policymaking experience, and judges that one's definition of political feasibility is an art. "It is a seat-of-the-pants judgment, based on the experience of the person making it." The "cost" questions that Huitt's artist asks himself are: "Will it 'go' on the Hill?" "Will the public buy it?" "Does it have political 'sex appeal?'" Perhaps Huitt is too close to the experience of policymaking to risk generalization. Yet he outlines some of the parameters that bind the policy making artist, and he may contribute to someone else's definition of empirical generalizations.

James W. Davis and Kenneth M. Dolbeare assess the policies of the U.S. Selective Service System by the standards of its official goals, and recommend substitutes for present policy. On the basis of data collected by the Selective Service System, the Defense Department and their own survey of draft boards in Wisconsin, Davis and Dolbeare conclude that the System's goals of "equity" and "efficiency" are not realized by existing (i.e., pre-1967) procedures. The goal of equity fails on account of the high incidence of low-income and black inductees; and the goal of efficiency fails because there are no systematic efforts to define the civilian manpower needs of the country, or meet these needs through the criteria that local boards use to grant deferments. The authors ask if they, *as professional political scientists*, can recommend one policy-alternative over another? They argue that policymaking involves a balance between *assumptions accepted, conditions perceived, goals established, value priorities asserted, means available,* and *effects and by-products considered acceptable.* Further, they claim that scientific research in three of these elements (defining relevant conditions, means available, and effects and by-products produced), will support recommendations. The "fudge factor" in their argument is the notion of scientific analysis that "supports" policy recommendations. They do not claim that the skills of political scientists lead unerringly to one policy-recommendation instead of others. They only *reject* some policies by showing their *inconsistency* with current conditions, means available, or likely effects and by-products. They are less successful in producing unchallengeable recommendations.

In separate papers, Lewis A. Froman, Jr. and Robert H. Salisbury suggest that public policy be used as an independent variable to probe other features of the political system. They depart from the more-typical view of policy: as a dependent variable to be explained by reference to other political, social or economic phenomena. Froman evaluates several conceptions of policy for their potential utility as independent variables, and Salisbury offers some hypotheses about four "types" of policy: distributive, redistributive, regulatory, and self-regulatory. Their principal argument is important: different types of policy are likely to stimulate different kinds of interactions among other features of politics. Although both papers are useful preliminary exercises for research, their theoretical categories are fraught with some of the problems that Froman offers in his own evaluative check-list: mutual exclusivity; validity; reliability; and ease of measurement.

These two books reveal some of the breadth obtainable from a concern with "public policy." Like "decision-making," "group conflict," "political system," and "role," it is a focus that lends itself to many analytical purposes. Even more than these, "policy" offers a link between the work of academic political scientists and the "practitioners" who must formulate and assess substantive policies. Some of what we design for the purpose of understanding the policymaking process may be useful to those who toil as policymakers. Dror, in a section "On Improving Public Policymaking," and several essays of the Ranney collection discuss the formal linkages between academic policy analysis and policymaking. Like other efforts of this kind, however, their recommendations are less than satisfac-

tory. The roles of the academic and the practitioner are basically different. While one strives for more complete (*i.e.*, general) understanding, the other wants specific prescriptions for discrete problems. Yet the policy-oriented political scientist is joined to the policymaker by common concerns. The analytical techniques, and some findings of political scientists may aid the specific concerns of some policymakers. Perhaps the only general recommendation we can offer to the policymaker is that he obtain sophisticated training in policy-oriented political science, and ask for some of the always-elusive time in order to consider his lessons as he evaluates existing policy and formulates new proposals.

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It can hardly be doubted that political science (abetted in some cases by other social sciences) is rapidly becoming enamored of the study of policy. The advertisements in any recent issue of this *Review* bear testimony to this. Just glancing at the stack of books that I have accumulated within the last few months I come across such titles (all published in 1968) as *Politics and Policy, The Study of Policy Formation, The Policy Process in American Agriculture, Science Policy and the University, The Policy-Making Process, City Politics and Public Policy,* and *Public Opinion and Public Policy*. Naturally, the quality of work on policy varies greatly. The perspectives and interests of any two social scientists with an avowed policy orientation may be radically different. Fuzzy concepts, irrelevant data, insufficient data, and detailed treatments of boring questions abound. But so do stimulating and fruitful analyses. It seems clear that the rediscovery that politics is, after all, principally about outputs, outcomes, authoritative allocation of values—in short, policy—has produced exhilaration among some of our colleagues. Exhilaration, of course, does not always produce insight but, to state a proposition: the more the exhilaration the greater the chances of insight.

It is most productive to attack the task of commenting on *Public Policymaking Reexamined* and *Political Science and Public Policy* by placing them in the context of the "lessons" they teach about the fruitful study of policy. These lessons come from both the strengths and weaknesses of the books.

Lesson #1

Since "policy" is not a self-explanatory term it needs some clear conceptual discussion. The scope of "policy studies" also requires considerable attention, because it does not define itself. At minimum, distinctions have to be drawn between the study of policy decisions, policy processes, policy statements, policy inputs, policy outputs, policy outcomes, and perhaps, policy arenas or issue-areas. In addition, some distinction needs to be made between policy description, which involves the chronicling of alternatives; policy prescription, which involves the advocacy of alternatives; policy analysis, which involves advice on the choosing of alternatives; and policy theory, which involves the explanation of why certain alternatives are chosen and others are not.

There are useful conceptual formulations scattered throughout these volumes. Austin Ranney's essay in the volume he edited is the best. In a few pages he clearly details the component parts of a "public policy," and distinguishes between policy content, policy process, and policy outcome. Robert Salisbury's essay in the same volume does a particularly useful job of distinguishing between three positions on the meaning of policy (pp. 152–153).

James Rosenau, again in the Ranney volume, argues that the term "policy" is so vague, or at least has been made so by the meanderings of political scientists, that it is not really useful. At least in the foreign policy area he proposes the use of a different concept—the "undertaking"—in order to counter traditional murkiness. The core of this interesting argument follows (p. 222): "An undertaking is conceived to be a course of action that the duly constituted officials of a national society pursue in order to preserve or alter a situation in the international system in such a way that it is consistent with a goal or goals decided upon by them or their predecessors . . . [U]ndertakings, unlike policies and decisions, encompass goals and their implementation. Their distinctive quality is that they focus on what government does, not on how it decides to do it or on what it commits itself to do."

In order to explore all of the important connections between policy and society the scope of political science analysis focusing on public policy has to include inputs, outputs, outcomes, and arenas or issue-areas as well as statements, decisions, and processes. The last three categories are much narrower and more restricted than the first four. By focusing exclusively on statements, decisions, and processes their impact is never known. This is akin to knowing all about the workings of an internal combustion engine without being aware that it can be used to drive an automobile or that an automobile if properly powered is more likely to bring greater pleasure

and safety than one that is either overpowered or underpowered.

The Dror book is essentially an elaborate investigation of process alone. His conceptual apparatus is generally clear, but it is not very useful in understanding anything about policy. As the title of his book makes clear, Dror is interested in "policymaking." His definition of "policy" is extremely limited and limiting: "public policy, in most cases, is primarily directed at government organs, and only intermediately or secondarily at other actors." (p. 15). He divorces the assessment of the "quality" of public policymaking from the "output" of policymaking. What he is concerned with is the process itself and whether, on the basis of a number of specific criteria, it is satisfactory. Output usually cannot be known and so the quality of the process is all that can be judged. This leads him to such conclusions as: "Compared with the past, public policymaking in all modern states is continually improving." (p. 101). The divorce of quality of process from quality of output and outcome leads to distorted results. If the quality of the process in the Pentagon rises, does it make any difference if the inhabitants thereof reach decisions that incinerate all of us? If the Departments of Housing and Urban Development and Health, Education, and Welfare evolve high quality procedures and yet the ghettos continue to disintegrate is this really good policymaking?

Dror properly asserts that "the significant output on which evaluation must focus is . . . its substantive effect on some real social situation." (p. 36). But he quickly concludes that such substantive effect cannot really be evaluated and so he turns his attention to evaluating process patterns, structural elements, inputs, and limited aspects of outputs—"nominal output" (that is, decisions as stated by the decision-makers), feasibility, and probable real output (a guess about what might happen).

Dror's efforts are useful is reminding us that the "process box" is still important in the study of policy. Their utility is highly limited, however, because he refuses to move outside the process box in any significant way.

Vernon Van Dyke, in his well argued essay in the Ranney book, summarizes an attractive line of argument about the scope of "policy studies":

> I have no criticism of the stress on process or of the accepted limits within which process is studied. The question is whether political scientists should not take a broader view of their tasks where policies are concerned—and I mean policies designed to have effects outside the governmental system itself as opposed to policies related to the governmental process. I believe that they should; not that every political scientist should accept a policy orientation for himself, but that the proportion of those who do should increase and that everyone should regard a policy orientation as legitimate and proper.
>
> The basis for the view is the proposition that our concern should be the promotion of human welfare. . . .
>
> Our concern should also be with the development of general knowledge, with theory, with science, but this proposition should not supersede the earlier one. It states an instrumental value, not the goal value. (pp. 36–37).

Lesson #2

Despite my approving quotation of Van Dyke, I think that in general it is not productive to worry endlessly over the "proper" role of the political scientist in relation to policy. These volumes are filled with numerous, and varying, self-conscious statements about the details of the political scientist's commitment to policy studies. Surely, broad statements, such as the one just quoted from Van Dyke, are useful. Hopefully, all political scientists could agree to the stipulation that ignorance of policy issues is no longer necessary (as it once was widely, although not universally, assumed to be) in order to maintain "scientific" purity. Indeed, I hope that the converse would now be acceptable to most political scientists: ignorance of policy issues is likely to impede scientific progress in the discipline. The essay by Lucian Pye in the Ranney book argues both propositions convincingly.

But beyond these general kinds of statements, detailed agonizing leads to more confusion than clarification. Lincoln Bloomfield's attempt (in the Ranney book) to dichotomize the world of scholarship and the world of policy serves as an example. At this stage in the development of the discipline I think it is more useful to judge a series of individual relationships between scholarship and policy than to detail a set of rigid boundaries beyond which the political scientist may not stray. Salisbury's call for "critical pluralism" (p. 156) at this stage in the development of policy studies is persuasive to me.

Lesson #3

In some work, policy should be treated as an independent variable; in other work, the customary treatment of policy as a dependent variable may make more sense. Three essays in the Ranney volume (those by Salisbury, Rosenau, and Lewis Froman) argue for the utility of treating policy as an independent variable. Salis-

bury, for example, builds an extension of an earlier scheme proposed by Theodore Lowi for categorizing policies and predicting structural and even environmental variables by the initial act of categorization. Froman discusses a number of alternative schemes with his customary brevity and clarity. Rosenau stresses the utility of this approach in the foreign policy arena.

Another essay in the Ranney volume, by James Davis and Kenneth Dolbeare, does more than theorize about the utility of viewing policy as an independent variable; it actually reports some empirical work on draft policy in which the payoff can be seen. The authors explore the consequences of present policy and the likely consequences of alternatives. This is analytically sound and also allows them to make recommendations on how best to reach certain explicit and implicit values at which the present system presumably aims but cannot achieve.

Lesson #4

There is a particular necessity for the development and use of a "scientific consciousness" in studying policy. This includes, crucially, a belief that knowledge is cumulative. The phrase in quotation marks comes from the Rosenau essay, as does the explanation of its meaning: "Stated most succinctly, a scientific consciousness involves an automatic tendency to ask, 'Of what larger pattern is this behavior an instance?'" (p. 204). It involves the framing of questions in the form of hypotheses and a conscious use of different kinds of variables, the arrangement of which can be altered for purposes of both generating and answering interesting questions. Rosenau says that with only a few exceptions all of the literature on foreign policy lacks this consciousness.

Ideally, the scientific consciousness will appear full grown in work that considers the broad sweep of policy. But, in most instances, it is likely to emerge in bits and pieces. Rosenau complains of the highly limited utility of case studies in the foreign policy area and concludes that virtually none of them have been written by authors with a scientific consciousness. But perhaps there is hope even for the case study.

Lesson #5

Description and prescription are both legitimate enterprises in the study of policy. The prescription can be of specific values or purely instrumental, in which case any values will do. There are dangers in the prescriptive endeavor but there is also much to be gained.

Dror illustrates the dangers. He limits his models to the "instrumentally normative." "They do not state what the substantive values of a system should be, but instead show methods for increasing net output, whatever the values prescribing that output may be." He says that net real output cannot really be measured or known very often and, therefore, he is likely to have considerable trouble knowing if his normative suggestions really work. Thus he seems to be prescribing for government policy makers and is likely to be content if his suggestions are adopted and result in more activity that can be judged by his "secondary criteria" mentioned before, but almost never by his "primary criterion" of net output. He prescribes to attain "quality," and he says "quality is analytically and conceptually the same as the idea of net output" (p. 33). But "net output" can almost never be used in evaluating the policy process. In short, 1) quality must be prescribed, 2) quality equals net output, 3) net output (quality) can almost never be evaluated. He has no way to evaluate the results of his prescriptions. This undoubtedly gives him free rein in prescribing!

Within this unique framework Dror makes some prescriptive statements that are interesting: for example, "One very interesting feature of an optimal policymaking structure is that it should be rather redundant: the contributions to the various phases should duplicate and overlap each other." (p. 211). It is too bad that his own logic prevents a testing of the value of such a prescription. Many of Dror's recommendations lack freshness or interest, however. For example, he tells us that "High-quality manpower should be drawn into the public service." (p. 253) and "Optimal public policymaking requires senior civil servants who have a great deal of knowledge and skills needed for policymaking." (p. 255).

The Davis and Dolbeare essay best illustrates the benefits of prescription. Their careful discussion of the conditions under which prescription should be offered deserves careful attention. For example, they specify three ways in which a social scientist might choose among policy alternatives: "First, he might frankly declare what values he considers appropriate, and seek to defend their propriety in some way. . . . Second, he might assume various possible values alternatively, in effect leaving the final choice to others. . . . Third, he might so assiduously develop data and refine interpretations of causes and effects in the areas of his peculiar strength (conditions, means, by-products) that the range of value choices would be drastically narrowed and so sharply focused that recommendation would involve a minimum of value preference." (pp. 115–116). They take the third option them-

selves in prescribing in the military manpower field.

The whole policy area is only now starting to be mapped out by political scientists. It seems to me that we have here a unique opportunity to demonstrate our belief that knowledge really is cumulative. I hope that the development of policy studies does not repeat the intellectual history of some other portions of the discipline, in which every new author starts out with his own theory, many elements of which cannot be tested by his own empirical work and will probably never be tested by the empirical work of anyone else either. Thus, I hope that what is valuable in these two books will not be read, reviewed, and forgotten but will instead appear as integral parts of future work.

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Political Order in Changing Societies. By SAMUEL P. HUNTINGTON. (New Haven: Yale University Press, 1968. Pp. 448. $12.50.)

In this new, and provocative set of essays, Mr. Huntington focuses attention on the phenomenon of political stability and on the factors that shape a stable political condition. He suggests that the degree of political order is regulated in large part by the relationship between the rate of institutional and organizational formation within the political system and the rate of social mobilization. He writes: "Just as economic development depends, in some measure, on the relation between investment and consumption, political order depends in part on the relation between the development of political institutions and the mobilization of new social forces into politics." (p. vii) It is this framework of analysis that serves to tie together the seven long essays making up this book.

Mr. Huntington's central thesis is, of course, well known to all who are acquainted with the literature of political development. In essence it argues that development and the resultant political order are obtained when institutional and organizational growth are *compatible* with the rate of social mobilization, the generation of new "social forces" and the resultant political participation. When the rate of social mobilization greatly surpasses the rate of institutional formation for a substantial period of time, the political system will not be able to cope with demands made upon it and "political decay" will set in. It is undesirable, therefore, that the rate of social mobilization exceed the rate of institutional formation, but on the other hand, it is equally important that it not lag far behind, for if it does modernization may well slow down and stop.

This framework is then used in Huntington's well known comparison of American and Western European political development. The author notes that compared to patterns in the political history of Western Europe, in the United States national political institutions were weaker and political participation grew larger, earlier. As a consequence, Huntington concludes that the United States has been less politically developed throughout its growth than the Western European countries have been at corresponding points. Our political system, Mr. Huntington suggests, has strong elements of a Tudor polity, brought here by Englishmen at the very time when their compatriots back home were getting ready to abandon that system.

In the rest of the volume Mr. Huntington is preoccupied mainly with two sets of problems. One set deals with situations where social mobilization overruns the institutional framework and the other set deals with situations where the institutional framework provides a high degree of resistance of the process of mobilization and the expansion of political participation. The first set of problems is treated in two essays: 1) An essay on revolution "the extreme case of the explosion of political participation," and 2) an essay examining the behavior of political systems operating with weak political institutions and organizations. In the latter, new groups mobilized by socio-economic change, struggle in the political arena, totally untamed by organizational and institutional constraints.

Two other chapters examine the difficulties in making a strong institutional framework give way to the new social forces created by social and economic modernization. The author explains in most interesting fashion the vicious cycle faced by absolute monarchs in traditional polities where the weak modernization that takes place has been paralleled by a concentration of political power in the hands of the monarchy, but where that very concentration makes it difficult for modernization to continue. This fascinating problem of the political "take off" has been the subject of a good deal of attention by other scholars concerned with political development and their findings also support Mr. Huntington's observations. The other piece deals with the problem of reformers maneuvering between strong existing institutional resistance to any change and the demand for change by the new social forces.

This book concludes with a chapter dealing with political parties. The author argues that parties are the key political institutions in politi-

cal development for it is they that primarily carry out the function of mobilization and organization and keep each one at the proper developmental distance from the other. It is this triple function that, for Huntington, embodies the very essence of political development.

The reader will find in this volume many new insights, hypotheses and typologies. The best in this volume are chapters 1 and 3 where Mr. Huntington develops the theoretical framework for the study of development and order and explores the difficulties in getting to self sustaining political growth in traditional polities.

As with any other major volume, one can find a number of petty annoyances and some more serious complaints. The chapters are too long. (The first is an exhausting 92 pages). There is no bibliography. Some readers will be disappointed by the fact that Mr. Huntington telegraphed his punch by publishing three of the seven chapters long before his book appeared. (One of the three, "Political Order and Political Decay," now has been substantially changed with new ideas and new data.) The author has not included a treatment of bureaucracy as a major institution of modernization and this is to be regretted, for such a treatment would have complemented the treatment of the political party. For if parties mobilize and organize men, bureaucracies mobilize and organize non human resources as well. While mobilization affected by parties is massive and undifferentiated, bureaucracies mobilize selectively and rationally. The different functions suggest why before social mobilization begins in earnest it is bureaucracies that are preeminent, while it is parties that become central during modernization and why with modernity bureaucracies return to the center of the stage managing both human and non human resources.

I should conclude with brief comments on the concepts of political development and political decay that Mr. Huntington has advanced in his previous work and in this book. In general I suggest that the theoretical model of political development advanced by Mr. Huntington is most useful in those cases where social mobilization is massive and non selective and the demand for participation is intense. It is much less useful when social mobilization has barely begun, or has not begun at all, for it seems absurd to talk of backward countries as developed because nothing is stirring and the political system is not under pressure it cannot handle. For these and for many other classes of cases, different conceptions of development seem to be needed.

My comment on the concept of political decay is necessarily equally brief. The crumbling of political institutions under pressure of new groups seeking to storm a political system is more than likely to create disorder. But disorder —even of the kind erosive of existing political institutions—may not always be an indicator of decay. The destruction of a political system gives a greater choice in the development of new institutions, and if these institutions are in fact developed it is questionable whether one ought to describe what has happened as decay. In other words whether "decay" takes place rests in part on whether the beginnings of new institutions can be identified. One suspects that the concept of "decay," at least in a small part, is a function of time.

One must certainly acknowledge Mr. Huntington's contribution. His concepts of political development represented—I think there is general agreement on this—a step forward on previous attempts to define the phenomena of modernization. And his concept of political decay at the least began to close a gap in the literature on development. Both concepts have been widely used. This pioneering volume, examining as it does the relation between development and stability, is an interesting and exciting addition to the literature. The interesting framework, the current urgency of the problem of order in countries at all levels of modernity, the fundamental nature of phenomena examined as well as the forthright argument, ought to attract to this new volume the thoughtful attention of political scientists and of that much larger audience concerned with the creation of political environments responsive to massively and rapidly changing needs.

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The Impact of Negro Voting: The Role of the Vote in the Quest for Equality. By William R. Keech. (Chicago: Rand McNally, 1968. Pp. 113. $3.95.)

Americans tend to have an almost religious faith in the efficacy of voting. From colonial demands for "no taxation without representation" to the Progressive stress on direct democracy, the ballot has been seen as the basic means for the removal of social ills. Today, as black Americans demand fulfillment of the promise of equality, voting is again emphasized. As President Johnson asserted in signing the 1965 Voting Rights Act, "the vote is the most powerful instrument ever devised by man for breaking down injustice and destroying the terrible walls that imprison men because they are different from other men."

William Keech, of the University of North Carolina, is skeptical of these unquestioned as-

sertions. In this literate and carefully constructed study, he analyzes the real impact of the black vote in two communities, Durham, North Carolina and Tuskeegee, Alabama. Negroes began to register and vote in Durham in sizable numbers more than thiry years ago, and now constitute at least a fourth of the city electorate. Growth in the Negro electorate of Tuskegee began later, about the end of the second world war, but eventually led to a black majority on the voting rolls. In both communities, therefore, we can see the impact of a large body of voters and make temporal comparisons. Moreover, in both communities unusually favorable conditions existed for the exercise of political influence. The Negro population was relatively well-educated, potential leaders had independent economic bases, and efficient political organizations were able to deliver a solid black vote. Durham and Tuskeegee thus provide crucial tests of the impact of a large black electorate under relatively auspicious circumstances.

Keech uses longitudinal comparisons, simple quantitative data, and detailed examination of policy outputs to answer the questions, "What impact does voting have on Negro social and economic status? What concrete advantages result? Does the vote have an influence over the distribution of advantages that is separate from the influence of those social, economic and political factors that are associated with Negroes getting the vote in the first place?" No formal causal modeling is attempted, since there are only two cases at hand.

Following Matthews and Prothro, Keech sees considerable limits on the efficacy of the vote. He concludes, "The vote is a far more potent instrument for achieving legal justice than social justice. The gains I have found to be most susceptible to Negro voting have consistently been those which most clearly involved fair and just administration of existing laws. Social justice, however, demands more than this. It often demands changes in existing law. . . . If social justice demands the eradication of the effects of past discrimination, the vote is even less useful." (p. 106–7).

The specific findings show great variability in the effect of the vote. In Tuskeegee, little change in the status of blacks occurred even when they constituted 35–45% of the electorate. Only after the Supreme Court invalidated an outrageous gerrymander and Negroes became a voting majority were public policies affected. The effect then was considerable and dramatic, but the vote failed to protect the interests of a large and oppressed minority. As Keech wryly concludes, "Saying that votes bring fair treatment of Negroes in Tuskeegee is different only in degree from saying votes bring fair treatment of Jews in Tel Aviv." (p. 94). In Durham, changes attributable to the ballot do occur, but other improvements in the position of blacks are attributed to non-electoral methods, such as litigation, street demonstrations, sit-ins, boycotts, and federal intervention.

The black vote is found important in these instances: the election of city officials, the passage or defeat of local bond issues, cessation of police brutality, equal treatment in the courts, school construction, provision of public services such as fire protection, recreation and urban renewal, and appointments to public offices. (In Tuskeegee, these influences are found only after Negroes became a voting majority.) Electoral power is found to be of little or uncertain importance in code enforcement, street paving, garbage collection, welfare payments, unemployment compensation, compensatory policies to remove the effects of past discrimination and, most vitally, integration of schools and public facilities. In the integration of public accommodations, the vote is found important in the later period in Tuskeegee, but of secondary importance in Durham.

In reacting to democratic myths of the omnipotence of the vote, Keech may have overstated his case. In doubtful instances, his tendency is to minimize the effect of voting power. The ballot has not brought full equality to America, but the gains secured in Tuskeegee and Durham, as specified above, are substantively as well as symbolically important. The value of the vote can be best gauged by imagining how few of these gains would have been realized without the ballot. Only through the democratic force of numbers could blacks balance the advantages secured to whites by economic and social power. While full equality has not been achieved, the incremental changes attributable to voting have been in that direction.

Seeking to show the power of the vote in these particular circumstances, moreover, is an unusual severe test. The governmental authority of municipalities is severely limited, and cannot be affected in many cases by any voting group, white or black. Concentrating on the local level also slights the effect of the vote on the national government. Thus, federal intervention may have been crucial in securing nondiscrimination in public accommodations, but surely one reason for this intervention was the pressure of integrationist votes directed through the national civil rights movement. Furthermore, there are unusual limits to the power of black votes represented by their visibility, the intensity of white resistance, and the historical depth of the American racial problem. Keech does acknowledge

these points briefly, but he does not fully qualify his generalizations of the importance of the vote accordingly.

We must also recognize that the value of the vote is partially intangible, and cannot be completely found by assessment of specific policy outputs. Thus, methods such as street demonstrations gain potency partially because they are supported by potential electoral power. Another value of the vote is negative—it provides a means of defense and protection for groups. While it may not secure direct benefits, it can lead elites to refrain from imposing disadvantages. Acting under a rule of anticipated reactions, officials may regulate their own conduct even without specific demands by the enfranchised.

Keech's work remains a well-designed study, in which theory and data usefully interact. It provides a solid model for the teaching of social science method, and yields important empirical conclusions. In our present period of ideological research, it is a fine example of compassionate but objective scholarship.

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Gulliver's Troubles, Or the Setting of American Foreign Policy. By STANLEY HOFFMANN. (New York: McGraw-Hill Book Company, 1968. Pp. 556. $11.95.)

In this large and ambitious book, Hoffmann attempts the conceptual integration of the external and internal restraints that impinge on American foreign policy. The study is concerned with American national purposes and with establishing "what the United States can or cannot do, given the kind of nation it is, in the kind of world we have. Purposes that go against the grain of a nation's deepest beliefs or habits, or against the grain of the world in which it is trying to fulfill such purposes, are not sound. Power at a nation's disposal ought to be used in full awareness of the external conditions that define which uses are productive and which are not, as well as of the domestic predispositions and institutions that channel national energies in certain directions or inhibit the country from applying them in other ways" (p. xiii).

In the first part of the book, Hoffmann examines the "grain of the world:" the contemporary international system is characterized by revolutionary dynamism, qualified or muted bipolarity, heterogeneous membership, and ideological clashes—making for an overall milieu which "is likely to be immoderate in ends and means" (p. 19). Hoffmann distinguishes between three related levels of the system, each of which exhibits different structural attributes. Most fundamentally, the system is bipolar in terms of the nuclear destruction the superpowers can unleash, but the very restraints imposed by the nuclear stalemate have given the nation-state a new lease on life and have allowed, on a second systemic level, the emergence of political polycentrism. Polycentrism, in turn, has encouraged the trend toward nuclear proliferation which lends a multipolar attribute to the third systemic level.

The peculiar national disabilities that make it so difficult for the United States to operate effectively in this system are treated in Parts II and III, which examine American national style and American institutions. The complexities that result from the intricate interdependence of the three layers of the contemporary international system would tax the skill, patience, and imagination of any nation. They are especially challenging to the United States because of a debilitating set of attitudes which stem from the American national style (a function of America's past and principles) and American governmental institutions. The major institutional problem is the dispersal of power among, as well as within, the elements of the governmental structure and bureaucracy. Deficiencies of foreign policy style are reflected in legalism, reliance on formulas, short-range planning, an engineering type of pragmatism, excessive optimism, a black-and-white image of the world, the conflict between quietism and activism and between the instinct of violence and the drive for harmony, and many other afflictions.

Bleak as this picture may be, we need not despair. American disabilities have worked well in the past—"the American style was admirably suited to the late 1940s and early 1950s" (p. 212)—and can be made to work again in the future. What kind of future international milieu to expect, and what role the United States should properly play in it, are the subject matter of Part IV, the concluding section of the book. Essentially, Hoffmann calls for a more modest American involvement in Europe ("a certain withdrawal") which would allow the emergence of a "European Europe," integrated along confederal lines and protected by a Franco-British nuclear umbrella under American and Soviet guarantee. The program is very similar to the Gaullist blueprint for Europe, purged of its most blatant nationalistic overtones. Aside from furthering the establishment of a harmonious Europe, a curtailment of the American role in Europe would have the additional advantage of freeing American resources for more urgent containment projects, such as the restraint of China. (Hoffmann believes, for example, that the primary question facing NATO today is "whether its members can agree on a policy in

the 'new' area of potential hot war, i.e., with China" (p. 461)). Hoffmann is critical of the over-reliance on military instruments of containment, but considers it "essential that the United States continue to resist the challenges of its enemies whenever they try to reap gains by using or threatening to use force across national borders; otherwise, revolutionary ideologies will have little incentive to become at least externally moderate" (p. 360). Hoffmann does not say how (or why) the ingrained deficiencies of American statecraft would qualify the United States for this global task.

There are many interesting and provocative insights in this book, and Hoffmann generally succeeds in portraying the complex interrelationships of world politics, especially in his fine discussion of the contemporary system in Chapter II. Yet the reader is left with a profound feeling of dissatisfaction and exasperation. Hoffmann tells us that his "own interests go in the direction of theory (in the rather loose and modest sense of a network of questions about the principal features of international relations, a systematic effort to understand its determinants and logic) rather than in the direction of policy," and that "one should not expect an apple tree to produce cherries—one should judge it by the quality of its apples" (p. xix). But aside from sketching systemic relationships on three levels Hoffmann provides little theory, even in a loose and modest sense, and instead we get a long discourse about the direction in which American policy should proceed. Hoffmann introduces only two major theoretical concepts—the distinction between moderate and revolutionary international systems and that between "possession" and "milieu" goals—and neither contributes substantially to the subsequent policy discussion. When it comes to a "systematic understanding of determinants" of the international system, which presumably requires a comprehensive discussion of basic factors, Hoffmann tells us with disarming modesty that although he is "interested in economic affairs" he is "quite incapable of making any worthwhile contribution in that domain—hence my decision to leave it out altogether" (p. xix). Hoffmann has a similar aversion to strategic-military matters, which receive only scant attention. This aversion is apparently based on Hoffmann's strong objections to "foreign-policy planning which suffers from that disease—extrapolation from the military or economic areas—[which] leads to overconcentration on discrete sectors or to elaboration of strategic doctrines reflecting a strangely bloodless universe" (p. 320). But the intrinsic importance to the analyst of military and economic matters is in no way diminished by objectionable thought processes they may have inspired. By ignoring specific economic and military-strategic relationships of the international system, it is Hoffmann's analytical universe which becomes "strangely bloodless" and overly cerebral.

One of the major flaws of the book stems from Hoffmann's vacillation over what analytical principles he should bring to bear on the subject matter. One would think that, for purposes of theory as well as practice, the analyst should aim for balanced judgments. Hoffmann, commenting on his criticisms of American foreign policy, makes it plain that this is not his intention: "to borrow a nice metaphor from André Gide, I was concerned with the lion's fleas, not with denying that there is a lion under the fleas" (p. 212). There is nothing wrong, of course, with a critically slanted, imaginative assessment of American foreign policy. But problems develop when an admittedly distorted and highly impressionistic portrayal is subsequently incorporated in an analytical framework whose implicit appeal is to standards of objectivity and plausibility. A portrayal which sees only warts may be appropriate for a critical tract, but it is not very helpful for theorists or policy analysts who presumably strive for a more realistic presentation of features. Hoffmann, who wants to be analyst and theorist as well as critic, does not fully appreciate and guard against the tensions between the intellectual attitudes that these different roles seem to engender. The result is a methodological potpourri. The three large narrative building blocks of the study—the discussion of the international system, of America's stylistic and institutional shortcomings, and of policy proposals—are so dissimilar in methodological assumptions and criteria for evaluation, that they resist conceptual integration and indicate the analyst's loss of control over his material. (This methodological imbalance is underlined by Hoffmann's disconcerting habit of either making the most sweeping assertions, as in his discussion of American national style, or hedging any analytical judgment with the most involuted qualifications, at times to the point of evasiveness.)

This jarring structural defect is the more unfortunate because the book, failing in its task of analytical integration, has not much else to offer. There is little new historical material or historical interpretation, the long list of alleged American disabilities is familiar even to undergraduates from their texts, and the policy proposals are of questionable value and appeal. Especially annoying is the methodological legerdemain with which Hoffmann handles the concept of national style. Understandably, he disavows the notion of national character since it

"has turned out to be of dubious validity" (p. 87), and he enumerates the similar objections that can be raised against the notion of national style, even though he proceeds to ignore them. (Here, as in several other instances, Hoffmann seems to believe that merely acknowledging conceptual difficulties will make them go away). Despite his protestations, Hoffmann's analytical method is in no way different from the highly impressionistic, organismic, and reified views of society that are typical of traditional national character studies. Since Hoffmann is primarily concerned with socio-psychological and intellectual shortcomings of American foreign policy, the 300-page discussion of American style and institutions skips along on the surface and never reaches into what Hoffmann calls the "blood and nerves of politics." The result is a strangely ethereal, almost dainty, view of the political process. Offering no linkage between socio-cultural abstractions and foreign-policy-making, Hoffmann seeks to reassure the reader by saying that "when we speak of 'America' in sweeping terms fit to infuriate social psychologists, we shall be using a short cut; we shall be talking about the beliefs, attitudes, and acts of those Americans who are responsible for and concerned with the nation's foreign affairs" (p. 89). But Hoffmann does not analyze decision-makers or opinion-makers in terms of American national shortcomings, and we are left guessing whether Dean Acheson is guilty of excessive optimism, whether Senator Fulbright entertains a black-and-white vision of the world, whether Curtis LeMay holds a utilitarian faith in rationality and harmony, or whether Adlai Stevenson felt a conflict between his instinct for violence and the drive for harmony.

Finally, the book is much too long. It may be that Hoffmann, freed from the discipline imposed by the essay, his usual format of writing, was tempted into a long excursion whose point of destination and avenues of approach were insufficiently charted. Those of us who like his essays will join in wishing him a speedy return home.

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Politics and Policy: The Eisenhower, Kennedy, and Johnson Years. By JAMES L. SUNDQUIST (Washington, D.C.: The Brookings Institution, 1968. Pp. 560. $8.75.)

Speaking of the assassination of John Kennedy and the first three years of the Johnson Administration, James L. Sundquist asks the question: "Is the American system of government so constructed that it must depend upon tragedy and political happenstance to render it capable of responding decisively to the nation's problems?" The originality and care with which Sundquist pursues this problem make his study a significant contribution to the growing literature on policy-making in the United States. Indeed, it is fair to say that his effort to raise and research important policy questions make his book indispensable reading for anyone concerned with American national government.

Politics and Policy is two books in one. Part I of the book consists of case studies of six central domestic policy arenas during the Eisenhower, Kennedy and Johnson years. Separate chapters are devoted to fiscal and structural measures for dealing with the problems of unemployment, poverty, education, civil rights, medicare and the outdoor environment. In Part II the author analyzes the political institutions and processes which explain how these policy innovations originated, got stalled, compromised and, finally, enacted.

The case studies are good narrative accounts of the obstacles facing new federal programs and, like most case studies, their primary value is the exposition of the nuances of the decision-making process in Washington. Sundquist tells the story well, even though the plot is a little worn, and there is a great deal here for the beginning student of government. The chapters on unemployment, poverty and medicare are especially useful in this regard.

What distinguishes this book as an important one for political scientists is the author's attempt to analyze the 1953–1967 period as one in which Democratic activists, largely stymied in Congress, developed policy positions which: (a) differentiated the Democratic party clearly from the Republicans; (b) found wide approval among the general public; (c) received a mandate for enactment in the elections of the 1960's; and (d) were enacted by the majority party under the leadership of Kennedy and Johnson. Addressing himself to the responsible parties debate, the author describes his major thesis: "As it happened, the Democratic party in the 1950's and 1960's did exactly those things the Schattschneider committee said should be done and Herring said could not be expected. It *did* develop a program to which it committed itself—and which it put into effect after winning office." (p. 391)

Sundquist has little difficulty showing that in the 1950's the engine of change for national policy centered in and around Democratic activists on Capitol Hill. Activist Democrats, their interest group allies, the Democratic National Committee and the Democratic Advisory Council formed a policy-oriented subsystem which was

at odds with the Eisenhower Administration (as well as, many times, the Democratic leadership of Congress), which did organize policy alternatives, and which did come into its own in the 1960's. Yet the question remains, Was there a direct link between the activity of Democratic party elites, an issue-oriented electorate, and the policy outputs of the mid-1960's?

Relying on public opinion polls and electoral data, Sandquist tries to demonstrate that there was. Concurring with Key's argument in *The Responsible Electorate* that, among those voters who switch parties from election to election, policy attitudes count, he "attempts to go a step further to determine *which policy or program issues are most influential and to assign relative weights to them.*" (p. 435, italics added.) This is a tall order, and he is only partially successful in completing it. But even partial success on a venture this bold is worth careful consideration.

To show that the issues he examines in Part I made a difference in the electoral process, Sundquist devised an "index of relative pulling power" based upon party switchers in congressional elections. For example, among those who voted for Eisenhower in 1952 but wanted the Democrats to win their states's congressional seats in 1954, there were more defectors among those who saw unemployment as the most important problem facing the country than among those who stressed other problems. In this example the index shows an advantage to the Democrats (whose position on the issue was more popular) of 1.3 percent. Similar calculations are made for the other issues.

Given the relatively small differences in the percentages used, given the problem of sampling error, and given the recognized difficulty of controlling for the effects of party and SES variables this effort is not likely to go unchallenged by political scientists. The author, who is well aware of the problems but discounts them, indicates as much when he concludes that the 1954 Democratic gain from the unemployment issue "may have been something less than 1.3 percent but it was surely something more than zero." (p. 439.) Still, at the margins of the electorate and among switchers it is probably true that issues do count heavily, and Sandquist has made an interesting attempt to tell us how much.

The author is on firmer ground when he shows that the Democratic activists were far more successful in proposing popular domestic policies than were the Republicans with their anti-spending, anti-inflation appeals. Regardless of the mandate content of any particular election, Democratic activists led by Kennedy were committed to certain kinds of action in the early 1960's and they could point to broad public support for their objectives. Congress, of course, was a stumbling block, but Sundquist correctly notes that a great deal of legislation was either passed or about to pass before the 89th Congress completed the job. What is truly remarkable is not that 295 Democrats in the 89th Congress passed a lot of bills but that so much was passed in 1963-1964. The 88th and 89th congresses together marked the end of one period in domestic policy-making, and the author's account of this is the best one to date.

Throughout this book Sundquist draws heavily on his experience as an activist staff man on Capitol Hill and in the executive branch. He offers new insights into the policy-making process in Congress and the executive, he has a perceptive discussion of the "dual legislative process" which occurs first in the executive and then in Congress, and he avoids the trap of exaggerating the static or deadlocked features of the national policy-making process. Indeed, if anything he is more sanguine about change than most students of national politics.

Politics and Policy is much more than the armchair reflections of a former activist. It is an attempt to describe, analyze and evaluate the national policy-making process, including parties and the electorate. Except for an occasional tendency to accept the rationalizations of activist politicians at face value (e.g. Adlai Stevenson's claim that policy alternatives can only be made clear in Congress between presidential elections, p. 406), the author's analysis is firmly grounded on empirical data. Students of public policy may fault his stress on policy initiation and process to the neglect of assessing the impact of those policies on the intended beneficiaries, but for an over-view of the process few books can compare with this one.

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The United Nations Force in Cyprus. By JAMES A. STEGENGA. (Columbus: Ohio State University Press. 1968. Pp. 227. $6.25.)

Cyprus: Constitutionalism and Crisis Government. By STANLEY KYRIAKIDES. (Philadelphia: University of Pennsylvania Press, 1968. Pp. 212. $6.50.)

Between 1954–58, the United Nations was unable to adopt any resolution dealing with the substance of the Cyprus question. Then, in 1964, the UN Security Council suddenly became involved in Cyprus by recommending the dispatch to the island of a peace-keeping force on a voluntary basis for the organization's members. The two books reviewed deal, respectively, with the stimulus for this Security Council action and the organization's response to the stimulus. Both

are based on the assumption that past conflicts between Greeks and Turks, not past instances of their peaceful cohabitation—for instance during the Balkan wars and World War I—render wellnigh impossible any future cooperation between Greek and Turkish Cypriots on the island. Thus, implicitly or explicitly, they are pessimistic about the prospects of conciliation and a future settlement, incidentally bolstering the Turkish and British viewpoint on the Cyprus problem.

To start from the UN's positive response to the Cyprus question rather than from the stimulus of the Cyprus crisis of 1963. Mr. Stegenga's monograph, as Inis Claude observes in his foreword to it, is "as timely as a piece of research as one could wish," and a "first-rate case study." After a cursory introductory chapter about the background of the Cyprus question. Mr. Stegenga, by a careful perusal of primary published sources and judicious use of secondary materials analyses within the international context the establishment of UNFICYP, the third peace-keeping force the United Nations ever set up, as well as this force's operation. In his assessment of this peace-keeping force, he is careful to state that his appraisal is necessarily tentative, since the Cyprus question is still in flux. After speculating about possible alternatives to this action and asserting that this UN peace-keeping experiment should alert the world's statesmen "to this novel technique of abusing the world body" (with President Makarios being regarded as guilty of this alleged abuse) he tries to assess this force in terms of operational efficiency and of effectiveness. In terms of efficiency, he rates UNFICYP as excellent, but in terms of effectiveness, his verdict is judiciously mixed, yet leaning to the favorable. On the one hand, he holds that this force contributed to bringing about a truce; to reducing killing and violence; to restoring law and order, and a large measure of normal economic and social conditions throughout most of the island; and to relieving the British forces on the island which were ostensibly seeking to perform a peace-keeping function. Together with the United States pressure on Turkey, this force also helped avert a grave threat to Mediterranean and international peace.

On the other hand, Mr. Stegenga points out that UNFICYP is merely a holding operation, not an instrument for bringing about a settlement—with this latter function to be performed by mediator provided for in the relevant Security Council resolution which set up UNFICYP. By its very existence, this peace-keeping force is thus vulnerable to the charge that it may well be inhibiting a settlement "by helping to protect and consolidate an abnormal *status quo* and by reducing the sense of urgency on both sides." (p. 186).

Speculating in conclusion on the likely character of possible future UN peace-keeping forces, he believes that such forces "will be more similar to UNFICYP than to UNEF. . . ." For in a world in which internally significant outbreaks of violence will continue, the future peace-keeping forces are more likely to be law-and-order forces rather that border-patrol or barrier forces, such as UNEF.

An inexplicable omission from this otherwise excellent study is the lack of any analysis of the important status of forces agreement between the United Nations and the Republic of Cyprus, the host state, thanks to which this UN presence on the island became possible. And a tendency already alluded to—to blame President Makarios for the constitutional crisis that led to the international Cyprus crisis of 1963—mars, in this reviewer's opinion, this otherwise objective study, which maintains that the Cyprus Constitution of 1960 was quite unwieldy. Neither Mr. Stegenga nor Mr. Kyriakides for that matter, refers to the fact that, before President Markarios publicitly presented in 1963 his thirteen points for a revision of the Cyprus Constitution, he consulted the British High Commissioner in Nicosia, the late Sir Arthur Clark, whose attitude encouraged the President publicly to put forward these points, triggering an almost instant Turkish government negative response, even before Turkish Cypriot leaders had registered their own views on these proposals. Perfidious Albion, said some. Others perceived in this inconsistency, a conflict of views between the Colonial and the Foreign Office. Finally, a reading of the speeches in general debate of the Greek representatives to the eleventh, twelfth and thirteenth General Assemblies would have revealed that the Greek party to the dispute, in contrast to the Turkish party, already since 1956 had shown a special interest in the establishment of UN peace-keeping forces in general, having in mind contingencies of Turkish coercive action over Cyprus that would threaten international peace and security in the Eastern Mediterranean.

Mr. Kyriakides' book, sub-titled "Constitutionalism and Crisis Government," deals first, in its first two chapters with the historical backgroup of the Cyprus question. The monograph's core—Chapters, 3, 4, and 5—seeks to describe the major features of the Cyprus Constitution of 1960, its areas of built-in tension, and the crisis in Constitution and government of

1963—a crisis which has become almost a norm by this time, Chapter 6—entitled "Cyprus Dilemma"—deals with the involvement of Greece, Turkey, and Britain in the Cyprus question. And "What Prospects for Cyprus" constitutes the book's concluding chapter.

In spite of this admirable outline, and the relatively superior treatment of the core topic, Mr. Kyriakides' monograph will hardly live up to the statement contained in its foreword that it will remain "the best documented analysis for many years to come" of the Cyprus Constitutional question,—unless, of course, no other study on this topic will ever be published. Its bibliography of primary and secondary sources is indeed extensive, not selective, as is Mr. Stegenga's. It is however far from complete. For instance, it altogether omits three Greek Government *White Books* (issued in 1956 and 1958) which have an important bearing on the background of the Cyrpus Constitution, aside from being quite revelatory of the question's international political development. Oddly enough, too, this extensive bibliography omits the relevant Cyprus proceedings of the eleventh and twelfth General Assemblies, where the Cyprus question was exhaustively considered. A cursory perusal of these materials would have revealed that the very condition of a Cyprus Constitution—the establishment of Cyprus as an independent state—originated in the Kashmir-fevered mind of India's Krishna Menon at the ninth General Assembly in 1954.

But bibliographical gaps aside, Mr. Kyriakides' book has several other shortcomings of commission or omission. For instance, in his introductory chapter, ignoring the millet system of the Ottoman government, he ascribes the elevation of the Church by the "Porte" to the maladministration of the local pashas (p. 5). And, disregarding the analogies of *enosis* in the Ionian Islands and in Crete, he suggests that the demand of the Cyprus Church for *enosis* was "a systematic attempt to prevent the development of a distinct Cypriot personality." (p. 9) Other shortcomings were due to his regarding newspapers as primary sources. (p. viii).

In his core chapter on the Cyprus Constitution, Mr. Kyriakides fails to discuss the Joint Constitutional Commisson set up under the London Agreement of February 19, 1959. This Commission worked for almost a year in preparing the text of the Constitution of 1960, incorporating in it the provisions of the Basic Structure of the Republic of Cyprus agreed upon between Greece and Turkey at Zurich on February 11, 1959. Had he done so on the basis of available published materials, he would have shown how the tension that led to the Constitution's breakdown was quite evident already at its drafting stage.

Perhaps the most distinctive feature of Mr. Kyriakides' book is the public opinion survey he conducted in 1965 in Cyprus. This survey, conducted only among Greek Cypriots, revealed, first, that the majority of the respondents blamed the Zurich agreement and NATO as the main cause of the Cyprus crisis; second, that a plurality of the respondents considered independence as the most realistic solution to the Cyprus crisis; and, third, that a majority of the respondents considered *enosis* with Greece as the ideal solution. The apparent discrepancy between the second and third finding reveals that self-determination as another option under the category of "realistic" solutions, was regarded by the respondents as a means toward an end—*enosis*. It testifies to the influence of the Greek Cypriot leadership's Greek Cypriot public opinion. For since 1954, this leadership had dropped the slogan of *enosis* in favor of the slogan of "self-determination," which, unlike *enosis*, is embodied in the UN Charter (Article I, paragraph 2), thanks to the Soviet Delegation's efforts at the San Francisco Conference.

Mr. Kyriakides' other hypothesis-finding that both Greek and Turkish Cypriots had hardened their positions, would suggest that the two ethnic groups on the island were unable to understand each other better than they could understand, respectively, Greeks and Turks, i.e. that a common Cypriot consciousness, regardless of ethnic origin, religion, language, was not evident when this survey was made. Whether such a national Cypriot consciousness will ever develop, no one can tell. In the past, states have set up buffer states on territories in dispute, as did, for instance, Argentina and Brazil, with regard to Banda Oriental, now Uruguay. That Greece originally favored the whole island's annexation because of the desire for *enosis* of the majority of the island's inhabitants at a certain point in their history; that Turkey, too, (both authors forget this), originally wanted to annex the whole of Cyprus on the basis of a historical title or on grounds of geopolitics and strategic security, may be the best omen for the Republic's continued independence, in spite of George Ball's dictum to the effect that Cyprus was a piece of real estate with a great past and no future. From such continued independence and all the vested interests independence creates, a national Cypriot consciousness could eventually develop transforming the Greeks and Turks of Cyprus into Cypriots.

Hunter College STEPHEN XYDIS

The Spirit of Chinese Politics—A Psychocultural Study of the Authority Crisis in Political Development. By LUCIAN W. PYE. (Cambridge, M.I.T. Press, 1968, Pp. 255. $8.95.)

This is the most controversial book on China published in 1968. The author is a distinguished American political scientist whose work has been in the forefront of methodological innovation. The book therefore demands attention if not agreement.

Lucian Pye's previous publications have focused upon the problems of political culture, often emphasizing Southeast Asia. He now addresses himself to contemporary China in an "essay about the unique national and personality traits that have inspired and shaped Chinese political culture from the time of the Manchu's to the present rule of Mao Tse-tung." (pp. vii) His task "is to describe the constellation of sentiment and attitudes that we feel must have existed for the Chinese political system to have developed as it has." (pp. viii) "The central theme of this essay became the authority crisis in modernization and the problem of psychic aggression for a people whose traditional culture was uniquely designed to repress all manifestations of that basic human drive." (pp. xviii) To accomplish this task, Pye has chosen to write an essay, leaving the problem of data to others.

In brief the argument is as follows. China, spared the identity crisis of transitional systems, instead has faced the necessity for new forms of authority which will permit the Chinese to reassert their historical self-confidence and also provide the basis for a reorientation of the society. In Pye's view the traditional Chinese state was self-contained, with a high stress on hierarchy, and ideology, and the sovereign importance of self-cultivation. Once this system breaks down, the problem of controlling aggression complicates the process of establishing new forms of authority. The first five chapters of the book are a broad analysis of this argument in Chinese historical experience. When the Chinese persisted in a political style, after the reality had changed, they released a flood of emotion that is described as the discovery of "hate."

The argument than shifts to the psychological roots of the authority crisis. In Chapters Six and Seven, Pye confronts the Chinese family, its socialization pattern and particularly its authority images. This requires a consideration of feelings toward authority and an exploration of their origins and functions in the Chinese personality. Pye's analysis derives the Chinese authority feelings from an essential emphasis on filial piety, an absolute denial of the legitimacy of all forms of aggression, and the vision of a rigidly defined order of role relationships. These characteristics are then inculcated through family socialization that invokes shame as the mechanism.

From this historical and psychological analysis come the difficulties of will power, action, and organizational behavior which impedes development. Pye's illustration is a case study of the communes (which is easily the most interesting part of the book). The book concludes with an overall summary of China's problems of the future based upon the book's appraisal.

The reader may have reservations about this book from three different viewpoints. First of all, there is the problem of "national character" studies. In describing the modal value for a society it is not possible to ignore the distribution of the characteristic in the society. Professor Pye has not set about such a study here. But in other attempts to examine the distribution problem the results have undercut the usefulness of the concept. Given the problems of operationalizing such a theory, in the Chinese case this may well prove to be a major stumbling block. In addition, as Pye notes in Chapter Six, there is a constant shift between adult action and family setting. The problem, of course, is to establish the ties between adult performance and the child rearing patterns of the family. Results in such large studies as Torney and Hess *The Development of Political Attitudes in Children* (pp. 217–220) caution against overestimating the role of the family in transmitting such attitudes. Kent Geiger in his study of *The Soviet Family* makes a similar judgment. Though these are not studies of Chinese society, their direction is clear. Consequently while Professor Pye suggests an alternative vocabulary and an interesting perspective on adult behavior as contrasted with the terms of the organizational theorist or the rational choices of the economists, it is not clear that the individually oriented theories are meaningful when applied to a societal pattern.

A second level of analysis centers upon goals. The positive argument for modernization and development is inherent in Pye's work. It is not necessary for him to deal with all the definitional problems of the terms, but to judge the Chinese in terms of a "modern open society" (pp. 45) or to describe the Chinese Nationalist and Chinese Communist leadership as "half educated rulers and strong armed bullies" (pp. 39) is rhetorical and certainly not helpful to the appraisal. Furthermore, it might have been useful to discuss Chinese achievement in terms of stated Maoist goals rather than so heavily in terms of Western political science.

The third level that must be of concern is the view of China which seems to emerge. Here the stylistic language handicaps analysis of an argument and forces the reader to demur. This is of particular importance since much of the case rests upon the special nature of the Chinese in contrast to other transitional systems. Two examples will indicate the problem. On page 30

"Nowhere in Chinese politics do we have a frank acceptance of the fact that men may be crass and self-centered and that it is only prudent in designing public institutions to insist upon checks and balances and to provide objective rather than subjective restraints on the use of power."

In fact the Chinese bureacracy was filled with such procedures as the law of avoidance in the civil service and the retirement provisions for mourning. Offical Chinese credo did indeed stress the belief in natural goodness, but there were a variety of provisions to protect the system.

Another example from contemporary China occurs on pp. 118.

They (referring to the Communists) have needed leaders who can speak the language of revolution and who in their own lives are capable of carrying the spirit of revolution against parental authority to the point of direct confrontation. It is significant that a disproportionate number of the top leaders of Communist China had violent and even traumatic confrontations with their fathers.

The book then discusses Mao, Chu Teh and P'eng Te-huai. The reader must feel compelled to ask, "disproportionate compared to what,?" and how do we know or can we know that the confrontations were traumatic given the state of biographical art on China.

Pye's analysis depends upon the acceptance of his view of China. If one is compelled to deny or modify those views, what remains may be innovative ideas about psychological constraints in politics, perhaps the necessity to re-think or define assumptions, but not an explanation of Chinese politics.

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Spending in the American States. By IRA SHARKANSKY. (Chicago: Rand McNally & Company, 1967. 170.)

One of the most fruitful bodies of empirical research in contemporary American political analysis has been the comparative analysis of policy outputs in the American states. From the standpoint of the systematic study of politics this research is noteworthy not only because of its substantive findings but also because it represents a pattern of systematic cummulative development not often found in political analysis.

Employing a comparative approach, a rough systems conceptualization, the statistical manipulation of readily quantifiable data, and focusing on public policies as outputs of state political systems, this body of research has investigated the relative impact of a variety of factors in determining what states do. One issoe that has assumed central focus in this research is the relative influence of political factors versus non-political environmental conditions in determining state activities. The early studies tended to conclude that economic and environmental factors were more influencial as independent variables than any of a wide variety of political factors. Some research, including the book under review here, suggest that broad generalization needs important qualifications.

Spending in the American States can be evaluated best in the context of this research. Ira Sharkansky has been one of the most prolific and creative contributors to comparative state analysis. The central question posed is, "Why do some of the American state governments spend more money than others?" Like most of the work in this field Sharkansky employs a correlational analysis of a number of political factors, environmental conditions, and state programs and expenditures to investigate this question. Measures of per capita state spending and changes in per capita spending over time are the major dependent variables used. The influence of the past, the impact of political and socio-economic characteristics, international affairs and the national economy, and regional affiliation of the states are successively analyzed in terms of how they affect state spending. State expenditures are then related to state services.

One of the major findings is that previous state spending is the most powerful independent variable related to per capita state spending. The amount of money a state spends per capita, relative to other states, at one point in time is very highly correlated with the relative amount of money it spent previously. These relationships remain strong even over a sixty-three year period. Sharkansky relates this finding to notions drawn from studies of financial decision-making and budgetary processes, that current budgets tend to be formulated around a base of the budget of the immediately preceding period.

Following the pattern of earlier studies, Sharkansky relates per capita state spending to a va-

riety of political and non-political factors. Most of the political variables are found to be of little importance. Unlike earlier studies, however, he finds that social-economic variables are not very highly associated with his measure of per capita spending. He explains this difference from other studies by his use of a measure based solely on money spent by the states. Measures used by others have tended to include both state and local expenditures. Poorer, less densely populated, and less industrialized states tend to rely more on state (rather than local) spending than do the wealthier, more heavily populated, and industrialized state. Thus, the wealthier state governments are not necessarily the highest per capita spenders. Previous spending, the state percentage of state-local expenditures, and local government expenditures are found to be the variables with the strongest independent relationships with the level of state spending.

The most intriguing findings are those relating state and local government spending to state services. Contrary to expectations, Sharkansky finds no consistent pattern of high per capita spenders having the most extensive public services. States that spent the most per capita do not necessarily pay the highest welfare benefits, have the most state parks, or the highest teacher-pupil ratio. Relationships here appear to be rather complex, and the author only scrapes the surface in explaining what his data suggest.

This book offers new and important insights into the correlates of state spending. Conceptually, methodologically, and substantively it adds to the body of comparative state analysis. Some of the findings are important—even though often puzzling and nonconclusive. Both those who have kept up with this line of research and those who are new to it can benefit from reading this book. There are new and challenging formulations and findings for the former, and enough review of earlier work and good discussion of the conceptualization of issues and statistical techniques for the latter.

The major shortcomings of this book—one that is common to much of the comparative state analysis—lie in the quality of explanation developed. The author sometimes seems to be really stretching to come up with plausible explanations for some of the findings. This reader finds some of the explanations offered less than convincing, and sometimes misleading.

For example, when confronted with the finding that expenditures for public welfare and education are less strongly associated with socio-economic measures than are other areas of spending, the author suggests that the lack of relationship in welfare spending may result from state ideological phenomena which very independently from economic, political, and governmental characteristics. This, obviously, does not explain very much. It seems to me that the cause of this non-relationship is the formulas for federal participation in the major state welfare programs. As I interpret the data used, the federal portion of welfare monies is included in the measure of state payments and expenditures. By and large federal participation in these programs is set up so that those states which give the smallest amounts of money to the largest number of people receive the highest share of what they spend from the federal government. States can rank high in the indices used by Sharkansky either by giving large sums of their own monies in large per recipient payments or by giving small payments to large numbers of people, maximizing the amount of money given out while keeping state contributions low. Thus, high ranking states in expenditures per capita include a combination of poorer states like New Mexico, Louisiana, and Arkansas for whom 65 to 74 percent of money spent for the welfare programs comes from the federal government, and wealthy states like California, Massachusetts, and New York which receive from one third to one half of the money they give out in welfare programs from Washington. Most likely it is this factor that accounts for the low statistical relationships mentioned above. When only state contributions are included the relationships are much closer to the expected ones, with wealthier states tending to spend more per capita.

Patterns of federal participation may also help account for some of the other relationships found. This may be one factor helping to account for the generally weak association found between spending and the quality of services provided by the states.

This book takes us some important steps further toward understanding the factors that influence state policy outputs. It also suggests how complex the analysis and explanations are. Despite the fine contributions of this work, however, this reader is struck by how far we still have to go in developing good explanatory theories of why states do the things they do. It is helpful to know that states which spent large sums of money last year spent large sums of money the year before and ten years ago, and that high spending states tend to be more centralized in the operation of governmental services; but it is still far from a satisfactory answer to the question of why some states spend more than others.

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Korea: The Politics of the Vortex. By GREGORY HENDERSON. (Cambridge: Harvard University Press, 1968. Pp. 479. $11.95.)

In this provocative and seminal book, a seasoned observer of the Korean scene boldly sets forth his interpretation of Korean society and politics since the fourteenth century. At the same time, the author presents his "theory of the vortex" arguing, in essence, the disadvantages of hyper-integration. It is a penetrating and stimulating book that deserves a wide audience.

The theme of the book, "the politics of the vortex," is deceptively simple. Korea, being such a homogeneous and well-integrated society both in terms of ethnic make-up and culture, failed to develop any meaningful middle groups either in terms of vested interests, religious separations, basic policy differences or ideological divisions. Individuals are totally atomized as a result and the highly centralized state "the eye of the storm") became a "great vortex summoning men rapidly into it." "The atomized mass streaming toward power" is the essence of Henderson's theory. The ethnic and cultural homogeneity along with centralization are blamed for Korea's lack of liberal and innovational spirit, quarrelsome factionalism, and underdevelopment. With this theme, the author examines the traditional society, Korean politics between 1860 and the present, political culture, and the varieties of ways (political parties, communism, and the military) through which cohesion is sought.

I could not help admiring the author for his breadth of learning and his thoroughness in utilizing the existing body of knowledge about Korea. Although the author relied mostly on materials published in English, he has consulted a fair number of publications in both Japanese and Korean. In addition, he had added materials gathered from his first-hand observations during this stay of more than seven years in Korea as a U.S. Foreign Service Officer. While the author maintains uniformly high quality throughout the book, I found his expositions of "colonial totalitarianism (Ch. 4), the period of American military occupation (Ch. 5. "The Gates of Chaos"), and the military (Ch. 12) particularly illuminating.

Henderson's theory of the vortex, however, raises some important questions that need to be answered. Most scholars of Korea would agree that the vortex of the kind described by Henderson did indeed exist among the ruling class (but not the masses) of Korea, and the forces responsible for it still persist. But was it because the masses have been atomized, as Henderson asserts? Have the masses in Korea in fact been atomized? Would decentralization and the development of "meaningful middle groups" cure most of the ills of Korea?

Since Henderson's theory of the vortex is an adaptation of Kornhauser's theory of mass society (William Kornhauser, *The Politics of Mass Society,* 1959), we may approach these questions by exploring the question of whether such Confucian societies as Korea and China indeed fit into the model of mass society. (Henderson argues that China and Russia also fall in the category of mass society. p. 333)

Although Henderson chose to stress one aspect of Kornhauser's theory, i.e. the absence of the structure of intermediate groups between the family and the nation, Kornhauser gave considerable weight to the "psychological properties of mass society," i.e. *self-estrangement of the individual* who fails to receive support for a sense of his own worth because of lack of opportunity for participation in society. (Kornhauser, p. 108). Kornhauser argues further that "in pluralist society [where intermediary groups do exist] there are alternative loyalties (sanctuaries) which do not place the nonconformist outside the social pale" (p. 110). What this means is that intermediary groups were but one of the means to placate the sense of self-estrangement among the members of the society. The question to be raised here is whether the structure of intermediary groups is the only means of preventing alienation.

My answer to this question would be negative. While the individually oriented Western societies required intermediary groups to provide a sense of participation, Confucianism created a "subject culture" with detailed hierarchical norms which did not encourage the aspiration for participation among the masses. As long as the rulers enjoyed "the mandate of Heaven," the masses did not feel estranged, and they were not readily attracted to mass movements. No doubt a great number of the ruling *yangban* class felt frustrated because of their inability to obtain government appointments, but very few from this class demonstrated the nonconformist behavior of the estranged.

I would argue, therefore, that Korea and China do not fit into the mold of mass society, and, under diffused culture, the masses were not self-estranged or atomized. In other words, the masses cannot be atomized in a well-integrated society. The situation after the turn of the century when disintegrative forces were injected into Korea is, of course, another matter. The causes for the vortex must be sought in factors other than hyper-integration.

Henderson was close to negating his thesis when he discussed the preponderance of anomic violence between 1945 and 1948 under American

occupation. Anomic violence, it should be noted, is a characteristic feature of mass societies.

> The rise of anomic violence was the inevitable result . . . of the rushed urbanization, industrialization, and mobilization of 1937-1945, plus the economic collapse, population dislocations, and lack of leadership during the first year of Liberation. (p. 147).

In explaining why the Koreans were swept into the vortex of politics, Henderson rules out external influence and economic factors as secondary and rather unimportant. (" . . . external factors are for Korea and her internal courses of secondary importance." p. 7) But, can we not say that the extreme degree of centralization and factional struggles among the elites came about because there was a dearth of "political goods" caused in part by a series of foreign invasions? Future research in economic history may reveal the close connections between economic conditions and political behavior. Although Korea was under complete foreign control only rarely in its history, the effect of these occupations may have been larger than Henderson's assessment, particularly in the shaping of the social and political environment.

Thus I find the "theory of the vortex" rather unsettling. I also believe that the stress on the theory occasionally marred his otherwise superb analysis of Korean politics. However, one need not accept the theory of the vortex to learn from the book and enjoy it. It will be a long time before any work of such scope and magnitude will replace it.

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BOOK NOTES

POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

Politics and Experience: Essays Presented to Professor Michael Oakeshott On the Occasion of His Retirement. EDITED BY PRESTON KING AND B. C. PAREKH. (Cambridge: Cambridge University Press, 1968. Pp. 424. $11.50.)

Reviewing collections of essays in brief compass is always a difficult matter, particularly when the purpose of the collection is not primarily the exfoliation of a theme or topic but the honoring of a man of many interests and influences. Such is the case with this Michael Oakeshott *Festschrift,* containing as it does thirteen essays ranging from philosopher William Dray's exposition of Professor Oakeshott's philosophy of history to Dorothea Krook's contemplation of the *kibbutzim.*

If there is a common theme it is the consideration of politics in four dimensions, of politics in time, inspired by Oakeshott's life-long interest in human experience, tradition, and politics. Books of this sort are vaulable if readers find particular essays valuable. I found all of the essays interesting, but like, I suspect, a good many other Americans I found Sheldon Wolin's "Paradigms and political theories" most valuable.

Wolin takes as his point of departure the now well-known thesis of historian of science Thomas S. Kuhn in *The Structure of Scientific Revolutions.* While Kuhn's book has been widely discussed by political scientists, most notably in two recent APSA presidential addresses, what has not yet been understood is that the real relevance of Kuhn's book to political science is not so much what Kuhn says about the developmental patterns of various natural sciences, but the example of Kuhn's own style of analysis. Science, considered not as a slogan or a policy or a set of abstractions, but as a phenomenon in the real world is a human institution. In this sense—and Kuhn himself clearly understands this perfectly well—science compares to community and scientific activity to politics. Thus, the way a historian-philosopher of science examines the development of his kind of human institution can be instructive for political scientists in examining their kind. Professor Wolin here opens the door to this angle of understanding.

Wolin invites us to consider the great political theorists on the model of Kuhn's practitioners of "extraordinary" science—the creators of scientific paradigms—and the statesmen, jurists, lesser theorists, and citizens who follow the master's guidance as comparable to Kuhn's picture of the scientific community, the members of which practice "normal" science. Now it is of course quite possible to be picky about this imaginative analogy, but Wolin and Kuhn are surely as capable as you can I of listing the differences between political institutions and activities and scientific institutions and activities. Let us, however, resist the temptation to show off how much we already know, and try to learn something new instead.

What is crucial about Kuhn's analysis is that he (along with such other contemporary philosopher-

historians of science as Stephen Toulmin and the late Norwood Russell Hanson) sees science as a living, human activity and not as a doctrine. Understood as a feature of cultural evolution science is not *a* method to be put on or taken off like a costume, (or like the wearing of a particular hat, to invoke a more widely used image) but an activity of mankind developing in time. Wolin in effect says, "Yes, science is a human activity in time and so is politics; and what is more, both can be understood as paradigm inspired."

"My proposal," says Wolin, "is that we conceive of political society itself as a paradigm of an operative kind. From this viewpoint society would be envisaged as a coherent whole in the sense of its customary political practices, institutions, laws, structure of authority and citizenship, and operative beliefs being organised and interrelated." (p. 149) In a passage that should make a good deal of sense to students of political development Wolin suggests that, "In the natural course of its history a society undergoes changes which impose strains upon the existing paradigm. A society may find the paradigm being challenged directly, or it may experience difficulty in coping with the results of change. New social classes may have emerged; new economic relationships may have developed; or new racial or religious patterns may have appeared. In much the same way that a scientific community will seek to adjust its paradigm to account for 'novelty,' a political society will seek to adapt its system to the new developments brought by change." (p. 149)

What from this angle of analysis we are being invited to do is not to look around in the political science or social science literature to find a paradigm (considering therefore structural-functional analysis, systems theory, etc. as rival candidates), but to consider Kuhn's own analysis as a paradigm (or better perhaps the application of a paradigm) not for politics itself but for political science. Kuhn is not much interested in promulgating universal laws of "scientific behavior" in the style of the more conventional philosophers of science. On the contrary he like Toulmin and Hanson repeatedly denies the relevance of rigid "single use" conceptions of science. Kuhn's analysis unlike so much of behavioral science takes its lights not from Newtonian physics and its descendents but from evolutionary, developmental biology.

Wolin's essay pushes us toward the fundamental question: should we try to understand political activity and scientific activity on the model of universal law physics or should we see men as products of evolution developing through their unique cultural capacity the changing adaptive devices called politics and science? It is a question, I think, that Professor Oakeshott would approve of, and, I suspect, there is no more fundamental question for political science. The other essays raise related questions albeit from quite different perspectives. It is quite likely that you will find something of value.—THOMAS L. THORSON, *University of Toronto.*

Power and Discontent. By WILLIAM A. GAMSON. (Homewood, Ill.: The Dorsey Press, 1956, Pp. 208.)

In introducing his book, Gamson describes his effort as "a discursive essay, with fluid boundaries, on a subject of endless complexity." (p. VII). Judged within such a context, it is a stimulating piece of work.

The presentation grew out of the author's study of community conflict. Originally, it was to serve as a theoretical introduction to an empirical analysis since published.

Two conflicting "perspectives" on power and conflict, influence and social control, are explored. Both orientations are elaborated and an attempt is made to unite them.

The influence perspective focuses on partisans in conflict attempting to realize a proportionate share of social rewards. Gamson argues that the writers who employ this perspective—the Laswellions of *Who Gets What? When? How?* elite theorists, the competitive power school of political party analysts, Marxians, and class conflict theorists more generally—share basic assumptions, related enough to justify considering them as a group. These mutual concerns are the focus on actors, not systems; the attention given to the scope and strategy of conflict, rather than its regulation; and the potentially advantageous use of discontent (perceived as an opportunity for change as against a threat to social order). The extent to which these assumptions, in actuality, are fundamental to each of the schools of thought contained within the category needs elaboration. Also helpful, but less essential, would have been the permutations that occur in each school, assuming that there are basically identifiable and consistent subsidiary assumptions for each.

The social control orientation emphasizes conflict resolution and regulation in contrast to power accumulation. The focus is on systemic questions, and indeed this is one of its distinguishing features. Such problems assume importance as the achievement of collective goods, the integration of the system so as to achieve conflict management, and the characterization of discontent as an element of instability within the governing framework. Unifying concerns among those who employ this approach include the focus on the system or its agents, rather than actors within it; the attention devoted to the regulation of conflict, rather than its strategy or tactics; and the construction

of discontent as a problem in system management, rather than one of competitive attempts to direct influence. Parsons, Easton, and Smelser are employed to illustrate these contentions.

The integration of these points of view would appear to be a monumental job, one demanding a clear assertion of underlying assumptions, a development of the theoretical priorities of the proponents of the subcategorizations, and a clear definition of terms. Such a complete synthesis awaits to be done. Gamson concentrates primarily on a clarification of terminology and in stipulating the relationship between and overlap among concepts relevant to both orientations. In the process the author treats such questions as the links between authorities and partisans, the relevance of a sense of trust for political stability and group competitiveness, influence and its use, social control and the channeling of discontent, predictability and resource mobilization in influence attempts, and the implications of the ideas expressed for social change.

Gamson measures the effect of influence as the difference in probabilities between two estimates, the first being the outcome with no partisan pressure considered and the second being the addition of group action to the equation. If the exertion of influence stiffens opposition, leading to a less desirable outcome than would have been expected without partisan interference, then "negative" pressure has been exercised. Resources to be employed in the transaction process are bargaining elements controlled by the influencer and capable of being brought to bear on authorities. The cost of resources employed in such a manner refers to those committed in the bargaining process.

The book explores the various possibilities such assumptions can lead to in analyses, adding confounding variables such as the constraints and inducements that can be employed and the possibility of persuading authorities without introducing new factors into the situation.

The study emphasizes the arena of conflict more than the content of the issues contested. This focus appears particularly suggestive for indicating the parometers of theoretical efforts along these lines while avoiding the complications surrounding arguments of influence attribution and the resolution of single or sets of issues supported in varying degrees by influentials. The scope of influence relates to the areas in which resources can be applied.

Influence and other aspects of questions relating to bring pressure on decision-makers are conceptually clearer and the ideas better elaborated than those dealing with the problems relevant to authorities. While the latter are treated, the consistent emphasis is on the control aspects and binding decision-making as it relates to conflict management. One part of the presentation that would appear particularly rewarding, the prediction of influence attempts and their probable success, while imaginative, is rudimentary in development. If this approach could be clarified and its empirical referants better stipulated, the conceptual precision of the schema would be advanced and empirical testing would be possible, major steps toward a truly integrative theory.

The author's principal enthusiasm is for questions relating to the use of influence. His development of ideas contributing to this orientation is provocative. The treatment of concepts relevant to authoritative decision-making is narrower in focus and more conventional. Over-all, the book represents a rewarding exploratory attempt of synthesizing two conflicting approaches to the study of social concerns. This is not the final statement on the subject, and it does not pretend to be.—WILLIAM J. CROTTY, *Northwestern University.*

Simulation in the Study of Politics ED. BY WILLIAM D. COPLIN. (Chicago: Markham Publishing Company, 1968. Pp. 365. $10.95.)

At worst, this edited volume might be regarded as a non-book drawn from a conference that was a non-event organized to promote a non-method, which deserves a one word non-review: "nonsense." Such a reaction would give an indication of the scholarly isolation of simulation researchers and the prevasive view of simulation as a methodological fad, or even as a pseudo-scientific fraud. But it would unfairly snuff out a useful attempt to generate a more constructive critical dialogue. The original conference was conceived as a means for discussing and evaluating some of the current work on simulation models of social processes. A common focus was set which asked the contributors to describe the assumptions underlying the simulated environments within which human or computerized decision-makers operate. An organizational structure was devised with five substantive panels, each with a non-simulator discussant, plus a graduate student panel on developing simulations and a demonstration-run of Coplin's own World Politics Simulation, with a dual culmination of an overview by Harold Guetzkow, as grand discussant, of the five areas, but also extending on into other areas of political science, and of a discussion of the "policy" potential of this work in government. The purpose was to break down the barriers between substantive areas and between simulators and non-simulators. The formal organizational dialectic of conferences usually vanishes in the haze of the cocktail lounge, whose informal dialectic goes unrecorded, leaving only the papers and formal comments, to be revised at leisure and eventually published, as in the

present case. A non-event produces a non-book, to apply Hennessey's Rule, "with no structure, no development, no general argument, and no discernible substantive editing."

Coplin's short introduction and summary do not suffice as the analytical overview which is necessary to break down the substantive barriers, nor is there much exertion of editorial influence within substantive sections. Still, the stimulation of a generally good collection of papers, even if it is a non-book, serves a useful purpose. Even those whose eyes glaze at the sight of mathematical symbols and behavioral jargon should be able to get through most of the papers and any of the comments without discomfort or frustration. And while there is some substantive imbalance, most of the papers have a wide empirical focus and general theoretical relevance.

As for the case made for simulation research, this varies by area. In the first and longest section of the book, the two substantive papers discuss the Northwestern Inter-Nation Simulation (INS) and TEMPER, unquestionably the most influential simulations developed in international relations. Hayward Alker dutifully goes through the INS model in detail, although he admits that this is probably redundant. As it happens, a shorter rendition is also given by Harold Guetzkow, in an abridged and slightly updated earlier paper substituted for his conference comments as grand discussant. Alker concludes with a brief sketch of a possible underlying theory, and a final paragraph on possible empirical verification methods (whose bibliographical footnote is, alas, missing). While this is not up to Alker's usual standard, it is not the piano player that needs shooting, but the piano—the INS. Walter Clemens has much the same problem in his propositional analysis and critique of TEMPER, that "white elephant" of computer simulation developed for the Defense Department. As Kenneth Waltz points out in his comment, neither the simulation programs nor the "theories" they represent are really theories at all, and the simulations are "very expensive crystal balls," and just as sterile from an analytical standpoint.

Going further, Clemens questions the empirical validity of the assumptions listed in TEMPER's non-theory. TEMPER is locked into a "cold war" structure, making it even more of a curio. Morten Gorden, who helped develop TEMPER in, as he puts it, his "rash youth," candidly admits in his rejoinder to Clemens that "just as generals prepare to fight the last war, simulators may model the last reality." But was the contemporary diplomilitary wisdom upon which the INS and TEMPER were based *ever* empirically valid? While Clemens disposes of TEMPER, empirical validation is only briefly referred to by Alker and Guetzkow; but one of the studies mentioned, Richard Chadwirk's effectively demolishes both the claims for formal theoretical coherence and for empirical adequacy of INS "theory." The absence of a fuller treatment of this existing validation work detracts seriously from the value of this section of the book, and does little to allay the suspicions of non-simulators about the scientific legitimacy of simulation research in this area.

Happily, most of the remaining papers are superb. In the section on urban affairs, John Crecine's paper on municipal budgeting is a major methodological and substantive milestone, and the short comment by Ira Sharkansky is intelligent. The best paper for the general reader is Paul Ray and Richard Duke's discussion of gaming simulation, drawn from experience with their METRO simulation, a miniaturized world of a middle-sized metropolitan area. Ray and Duke make the point that partially computerized gaming simulations are not makeshift way-stations to completely computerized elegance, but complex experimental tools for the investigation of decision-making behavior. Gaming and computer simulation form complementary components of an integrated and comprehensive research approach, by which a more systematic theoretical and empirical attack can be made on large, complex realities. These two papers on urban affairs, and the excellent review by Howard Rosenthal of simulation in elite mass electoral research (unfortunately, conference comments by Philip Converse were not included in the book), and the report by Rufus Browning (a pose-conference addition) on his partially data-based computer simulation of political recruitment provide the real value of the book. Not only are the papers informative from a methodological point of view, but their substantive content deserves a wider attention among political scientists than has been the case up to now. Two of the student papers are included, one based on the Michigan election studies, by Coombs *fils*, Fried, and Rabinowitz, suggesting a modular approach to election simulation; and the other, by Ronald Brunner, discussing a phased branching design as a possible general framework for the simulation of political development. Finally, there is an interesting discussion by Charles Moore of the heuristic programming approach developed in the Carnegie Tech management science tradition of decision-making analysis, which, if not really simulation, does attempt to state empirically valid decision routines (HP's). In their comment, Meyer Zald and Rolf Schliewen query whether this rather "mentalistic" emphasis on verbalized decision routines does not ignore latent constraints, and also confine itself to routinized decision-making, thus missing the more important organizational phenomena, and perhaps even imposing decisional

phasing upon behavior through the way in which a decision is defined.

While I believe that non-simulators will learn something from this set of essays, I wonder if the simulators will. Computers are not magic theory machines which immaculately conceive Truth. Waltz ought not to be ignored, because he is not initiated into the mysteries, especially since these usually conceal a big speculative "fudge factor," as Ray and Duke point out. They also emphasize that the "man-machine interface" is the creative nexus of the simulation approach to the study of decision-making, and not just an unsatisfactory makeshift. In this regard, there is a naive belief that loading a gaming simulation down with rich detail makes it more "realistic." Even when this detail does not introduce the biases of some brand of conventional wisdom, it gets in the way of meaningful comparison of experimental results. Despite Harold Guetzkow's strictures to the contrary, gaming simulation is an *experimental* method. When there is no decent fieldwork source of empirical data, as is usually the case in micropolitics, complex experimentation utilizing simulated environments will necessarily have to provide the incubation phase of behavioral political science—CHARLES A. POWELL, *University of Southern California.*

Islamic Surveys: Islamic Political Thought. BY W. MONTGOMERY WATT. (Chicago: Aldine Publishing Company, 1969. Pp. 186. $4.75.)

Without much doubt, it can be stated that Islam is rapidly becoming a doctrine of world wide significance. The increasing interest in Islamic studies is both academic and political. Islam is the youngest of the great world religions originating in Arabia and founded by the prophet Muhammad with many features common to Judaism and Christianity. It is also a political force shaping the destinies of many millions in the Middle East and elsewhere who adhere to the Muslim way of life.

The author of this book is highly qualified to write on Islamic thought. Mr. Watt is a Professor of Arabic at Edinbrugh University, the editor of the series *Islamic Surveys,* and the author of two additional works in the series. His profound knowledge of the Arabic language and his familiarity with the Muslim world contribute to the crispness and clarity of his writing in this area.

The reader of *Islamic Political Thought* should note on the inside title page the subtitle *The Basic Concepts.* This is essential in order to appreciate why this broad topic is exposed in relatively few pages (129 pages of text). This reviewer gathered two fundamental impressions. First, it is a difficult book for a political scientist to review as it is essentially a collection of basic historical facts. Second, for someone already familiar with Islam, what Mr. Watt has to say is elementary. Perhaps this is the reason for the subtitle. For the reader not familiar with Islam, *Islamic Politic Thought* will serve as an excellent starting point.

The exposition of the basic concepts of Islamic political thought rest on the fundamental premise, which is the theme of the work, that in Islam religion and politics are inseperable. Consequently, the political events of the past, primarily during the life time of Muhammad and the early Caliphs (*Kulafa' al-rashidun*) constitute the roots of Islamic political conceptions since "the concepts implicit in men's practice are more important than the writings of political theorists." (p. x) The eleven chapters of the book adhere faithfully to this premise.

In describing the evolution of Islamic political practice, the author is meticulous about giving his English reader precise definitions of basic concepts and Arabic words. He is cautious in defining these terms within the cultural setting of the period. The early Islamic state in its political concepts was to a large extent based on the nature of the tribal pre-Islamic society and primarily on such concepts as the *umma* or the community, and *jihad* or 'holy war'. The early Islamic community "was essentially a federation of tribes," and the *jihad* was the extension of raid or razzia which was an important feature of the life of the nomads in the Arabian desert. (pp. 14–5) The raid in the Islamic state was based on a Quranic verse 5.35/9, "O believers, fear God . . . and strive in His way." The common interpretation of *jihad* is that it offered the non-Muslim the choice between Islam and the sword. It is important to note that this choice applied only to pagans, but not to Jews and Christians as they were part of the 'protected minority'—*ahl al-kitab*—(people of the Book), or people with a monotheistic religion.

Under Muhammad, the Islamic state retained its pre-Islamic structure. Mr. Watt points out that "Apart from being the Messanger of God, Muhammad was not superior to other clan chiefs." (p. 21) His relatively weak position began to change partly because of his victorious military expeditions (the battle of Badr in 624), and because after he went to Medina in 622 the religious and the political were increasingly combined as revealed by the political relevance of his preaching. The point is that the Islamic religion simply added another dimension to the solidarity of the pre-Islamic community.

Mr. Watt devotes most of the pages of his book to the early caliphate and the Umayyad and Abbasid dynasties. He does this for Islamic political theory first centered around the caliphate, its purposes and dimensions as argued by the jurists and

on the political events that led to the creation of the Umayyad and Abbasid dynasties. In this discussion of the first four successors of Muhammad, the author notes that these rightly guided caliphs (*Kulafa' al-rashidun*) are held in special standing by Sunnite Muslims. The author reiterates his theme that "It is hardly an exaggeration to say that Muslims normally express political theory in the form of history." (pp. 36–7) The important historical incident that gave rise to the Shi'ite sect and to much Islamic political theory was Mu'awiya's refusal to acknowledge Ali, the last of the *rashidun* as caliph. Such an acknowledgement was essential since the practice until that point had been the choice of a caliph by acclamation (*baya*) of the believers. This position taken by Mu'awiya, the governor of Syria, led to bloodshed between the supporters of the two men and their differences was finally subjected to an arbitration.

Mr. Watt does not go into any details concerning this very important event perhaps because a universally acceptable account of it is non-existent among Muslims. He merely concludes the historical fact that the arbitration was favorable to Mu'awiya who, after the death of Ali, inaugurated the Umayyad dynasty and with it the arguments regarding its legitimacy. The author goes on then to consider the nature of the Muslim community under the Umayyad dynasty and the Abb sid dynasty that followed. He centers his exposition on the organization of these dynasties including the expanded position of the ruler, the military, judicial, administrative and religious; on the expanded notion of the community that included non-Arab Muslims and particularly the influence of the Persians and Turks under the Abbasids; and the numerous political ideas developed by the various Shi'ite sects notably their concept of the *imam* (caliph) who must be a decendant of Ali. Mr. Watt excludes from his exposition any meaningful discussion of the ideas of Muslim philosophers such as al-Farabi on the grounds that "they did not appreciably affect the main stream of Islamic political activity." (p. 81) But in the last few pages of the book Mr. Watt, in an attempt to show how Islam adapts to the contemporary world, argues that adaptive changes have been affected by the ideas of philosophers such as al-Farabi and Avicenna and such theologians as al-Ashari. (p. 127) He also excludes from his discussion such recent reforms as Muhammad Abduh and Rashid Rida. Instead, Islamic *Political Thought* seems to be centered around the notion of the *umma* or community of all Muslims and what makes for its solidarity, that is, political events; and on the *Sharia* (The Divine Law) and Tradition as being the model of Islamic social and political life, that is, the political ideas. In smort the book is concerned with historical events and those political ideas that have been operative in the historical process.

The attentive reader will note that Mr. Watt is an extremely clear writer. He scratches below the surface primarily in his discussion of the Shi'ite ideas. The book is free from any typographical and transliteration errors. Although *Islamic Political Thought* is an elementary book on the subject, it is nevertheless a valuable contribution to the English literature on Islam.—Sami G. Hajjar, *University of Wyoming.*

General and Social Systems. By F. Kenneth Berrien. (New Brunswick: Rutgers University Press, 1968. Pp. 231. $9.00.)

Man's inhumanity to man is perhaps exceeded only by his inability to understand himself and others. Berrien's book aims at providing a tool for dealing with this inability by applying general systems theory (GST) to "self" and to groups. Although his major audience is intended to be "those psychologists concerned primarily with group phenomena" (p. vi), he attempts to construct a theory which "is a sumarized abstraction of phenomena running the gamut from cosmology and molecular physics to some references to international conflict'" (p. 188). Surely, the political scientist can fit himself somewhere in this endeavor! The important questions are: Where and Why?

I invite only *some* of my colleagues to fit themselves into the first major part of the book (Chapters II-V), in which Berrien deals with the major concepts of GST as he delineates definitions, assumptions, and propositions for use in the theoretical framework. These colleagues are those who are not familiar with GST and its intellectual foundations. For them, the introductory chapters provide an accurate, readable, and comprehensive treatment of the major concepts of GST and a thorough bibliographic picture of its high priests, most of whom are dutifully cited throughout the body of the text; indeed, the attached bibliography is itself worth the price of the book for the uninitiated.

For the political scientist who has gone through the literature of GST, these chapters can be passed over. They contain nothing "new," "fresh," and "original" as the publishers and Kenneth Boulding would have us believe. For example, Berrien's central concept—"primitive randomness evolves into organized complexity"—was articulated decades ago by Teilhard de Chardin; in the latter's *Phenomenon of Man* a theory of complexification was developed in which orderliness and complexity evolve from randomness. Teilhard analyzed this process in the universe from the point of view of the "within and the without" of things —a point of view not entirely dissimilar to Ber-

rien's discussion of the growth of systems as equivalent to "the expansion of a system in size in which all parts are functionally articulated" (p. 75). In addition, we need not read Berrien's discussion of the probabilistic conditions of systems (pp. 48–50; 201–202) to realize that the universe is neither determinate nor mechanistic. The statistical physicists Gibbs and Bolzmann discovered decades ago the probabilistic nature of the universe and developed the concept of entropy as a framework for its measurement. Norbert Wiener's *Cybernetics* ante-dates Berrien's work by two decades and contains a discussion of feedback, storage, and memory, as does Ross Ashby's *Introduction to Cybernetics*. Moreover, Ashby's *Design for a Brain* contains a discussion of stability and adaptation that is not only much earlier but also much better than Berrien's. Finally, von Bertalanffy's *General System Theory* said—again decades ago—much of what Berrien covers.

It is in Chapters VI-IX—the chapters dealing with the application of GST to social phenomena —that the value of the book and Berrien's genius lie. I invite *all* of my colleagues to fit themselves into the book here. I offer this invitation for two major reasons. First, Berrien's analysis of social phenomena is a good example of theory construction. His incorporation of definitions and assumptions into relational propositions which generate expectations with empirical relevance for a number of research areas in political science is significant: his discussion of norms and roles in Chapter VI has important implications for small-group behavior and for coalitions; his discussion of the all-important GNS/FA ratio in Chapter VII and of the concepts of growth and adaptation in Chapter VII and VIII have great relevance to organization theory and political development; and his discussion of conflict in Chapter IX permits reasoning about domestic and international violence. These sections are suggestive of a variety of relationships which can be formulated easily into warrantable generalizations for research.

The second major value of the book to political scientists lies in Berrien's recognition of *choice* as a probabilistic factor in social systems (p. 93). It seems to me that this recognition has an important implication for political science. Political scientists are accustomed to dealing with the concept of system in one of two distinct ways. The first way is for analysis; that is, one can look at *any* set of phenomena systemically by dividing those phenomena into two categories: variables (the constituent elements of the "system") and parameters (those elements that are "outside" the system and form its boundary). Hence there are analytical systems. Second, there are natural systems, such as molecular and biological systems, where relationships are concretized and provide a basis for reasoning by analogy about social systems.

But it seems that the probabilistic element of human choice gives rise to a third kind of system —artifactual systems, i.e., systems which are products of human choice. Thus artifactual systems are set up *to be* systems, and the interesting theoretical question here is: How do individuals cope with systems which they themselves create? A political system is an artifactual system aimed at facilitating the exercise of human choice. *The Federalist Papers* articulate the logic of one particular kind of artifactual system.

The recognition of artifactual systems bears two important consequences for research in political science, and Berrien helps us to understand them. The first is a shift in research focus, which I shall call constitutional decision making. Berrien discusses deoxyribose nucleic acides (DNA's) as determinants of system form. Could it be, as Berrien seems to indicate, that constitutional decision making functions as the societal counterpart of the DNA template (p. 134)? If so, then can we assume that the structural forms and growth of any artifactual system will necessarily be conditioned by its basic constitutional framework? This is an interesting implication in that studies focusing on political development dismiss constitutional decision making as irrelevant in the dynamics of structure-function in developing societies. The second is a shift in research strategy. Artifactual systems are subject to frequent step-level functions and to changes in parameter values. Conventional research designs usually are cross-sectional, largely as a result of the "correlation game" played by so many political scientists. This "game" can not take account of dynamic relationships over time. Hence Berrien advocates the adoption of longitudinal research designs which can assess variable relationships over time and sees two promising theoretical developments along this line (pp. 157–161): game models and computer simulation. Both developments are examples of what I have called artifactual systems; both permit of continuous instead of periodic, cross-sectional measurements; and both allow for the studying of "real" groups over time.

These are the major reasons why my colleagues should read this book. Each of them may find a deficiency in it, to be sure; for reasoning at high levels of abstraction may involve the thinker in the fallacies of reification and analogous extrapolation. But Berrien treads cautiously and is aware of the epistemological problems raised by GST. For this reason, he only asks of his readers to judge, not whether his theoretical structure is true or false, but whether it will be more or less useful to them in their reasoned inquiry about complicated human relationships. In my judgment, it is

an extremely useful tool for the political scientist bent on developing a theoretical competence in his field.—JOSEPH D. OLANDER, *University of Miami.*

Rousseau's Social Contract: An Interpretative Essay. By LESTER G. CROCKER. (Cleveland: The Press of Case Western Reserve University, 1968. Pp. 198. $6.95.)

Professor Crocker combines an impressive familiarity with Rousseau's writings and a polyglot awareness of the secondary literature with psychological criticism and notions of political philosophy to arrive at a novel interpretation of Rousseau as an "authoritarian personality" who anticipates "human engineering" and the "science of behavior." Unfortunately, none of these factors help him to present a convincing explanation of the *Social Contract.* Moreover, the numerous references to secondary studies break the unity of his thought and prevent ready comprehension.

The first chapter explains the historical and psychological setting of the *Social Contract.* Thus, the early works are said to portray Rousseau's first thoughts about man and society; these ideas are developed more fully in the middle period and applied to actual states in the later works. Crocker believes that Rousseau sought a special means of controlling men in all of his writings and finally endorsed authoritarian controls in the practical works. Rousseau's identification of a combat between reason and nature, man's need for a political leader, and the principle that the people are not to be trusted by statesmen is a result of psychological inadequacies according to Crocker: thus, feelings of inferiority prompted his prophetic tone, a deep-seated longing for order compelled him to construct a rigid value system, and the desire that men be independent is evidence of emotional instability.

Crocker is critical of the institutions set forth in the *Social-Contract* and the theories behind those institutions. That Rousseau's State is as self-denying as Sparta is a consequence of the social pact. The community's authority is paramount, and the alienation of liberty is complete because the illiberal conditions of the General Will swallow up individual freedom. Far from preserving man in the independence he enjoyed before society, the total civic participation demanded by popular sovereignty places total control over him. Finally, the passages on the legislator and civil religion convince Crocker that Rousseau sought to establish domination by an elite and to introduce thought control.

Although aware that Rousseau is not responsible for misinterpretations of his thought, Crocker nonetheless considers it worthwhile to identify its "implications and potentialities" by bracing its influences (where the thoughts "have taken root") and to deepen and strengthen his own conclusion by studying analogues ("to what the ideas may conceivably lead"). Rousseau's influence is thus traced to the aristocratic reactionaries, as well as to the radical extremists, of the French Revolution and to precursors of Marxism, as well as of Nazism. Crocker then seeks facile analogues in *Brave New World, 1984,* and *Walden II*—concluding that *all* of these parallel Rousseau's ideas by inventing new possibilities for exerting power over men and reflecting the attitudes of the "totalitarian mind."

Four characteristics of totalitarian democracy—charismatic guide or leader, ideal of community, goal of unanimity, techniques to mobilize and control popular opinion—found in Rousseau's social thought by Crocker persuade him it represents a classic example of such a regime. Moreover, Rousseau's personality is a classic example of the totalitarian mind because: his need for reassurance led him to retreat from reality and to insist on dogmatic certainty; he longed for lost innocence; he was an outsider in his society; and he had a psychological fixation on illusion and deception. Rousseau's political scheme is all the more repungnant to Crocker because it is apparently without limits: he does not count on laws to govern men—he wants to reshape them. Finally, the society of the *Social Contract* destroys the antithesis between liberty and authority by abolishing liberty.

The many problems in Crocker's interpretation prevent its acceptance. For instance, he identifies difficulties in Rousseau's goals without considering that Rousseau also recognized them; concern with Rousseau's possible psychological troubles replaces the investigation of serious philosophical issues (pp. 13–14, 28–29). Rousseau's ideas are often dismissed because they conflict with contemporary opinions; the premises of contemporary opinion apparently need not be investigated (pp. 34–35). To explore the meaning of one book, the *Social Contract,* Crocker treats all writings of Rousseau as equally valid; this method allows him to use letters and first drafts, not to mention works with entirely different purposes, in order to affirm his own questionable interpretation.

There are some errors of scholarship in the book. Despite a plethora of footnotes, Crocker often fails to reference citations from Rousseau or secondary authors (pp. 46–47, 50, 57, 62–63, note 51, note 53, and p. 92). Translations are sometimes inaccurate and complete clauses are often omitted (pp. 69, 70, 73, 74, 85, and 97). At other times, Rousseau is quoted out of context to defend Crocker's own interpretation (pp. 70-71, note 110, pp. 87, 88, 95–96, 176, and 178). Three misprints occur in references to the *Social Contract:* p. 75,

references should be to II, 7 and II, 12, paragraph 5; p. 176, reference should be to III, 15.

In sum, the advantages obtained from Crocker's familiarity with all of Rousseau's work and with his life, as well as from Crocker's ability to indicate the pitfalls into which other Rousseau scholars have fallen, do not compensate for the errors of interpretation resulting from his questionable psychological and unexamined political notions. Before the erudition of Professor Crocker can benefit students of Rousseau, a careful study of the *Social Contract*—a study explaining its purpose, its parts, and how they fit together—needs to be made. Then we will be able to judge what light the first draft, subsequent letters to close friends, and passages from other writings shed on the message of the *Social Contract*.—CHARLES E. BUTTERWORTH, *Federal City College*.

Some Conceptual Approaches to the Study of Modernization. By DAVID E. APTER. (Englewood Cliffs: Prentice-Hall, Inc., 1968. Pp. 380. $7.95; paper $4.95.)

During the past decade David Apter has been one of the major figures in the field of comparative politics and his insights into the study of modernization have influenced not only a whole generation of graduate students, but his peers as well. His early articles calling for the development of unambiguous concepts and empirical theory have been reprinted in a number of collections, as have some of his studies of bureaucracy and traditionalism in Ghana and Uganda. In 1965, Apter brought together much of his prior research in *The Politics of Modernization*. Because of the availability of Professor Apter's earlier work, it is very difficult to see the value of reprinting his articles once again in *Some Conceptual Approaches to the Study of Modernization;* although the collection does enable one to follow the intellectual development of Apter himself.

Two of the major topics discussed concern 1) the relative capabilities of different types of political systems to achieve modernization and 2) a general approach to the study of politics. In attempting to provide an answer to the question of the capability of various types of political systems to achieve modernization, Apter argues that "the modernization process creates such problems of coordination and control that 'democratic' political systems, in the usual sense of that term, are not very relevant." (p. 329) What is required is high control system with a political religion (ideology) that provides for some popular support.

"Modernization" for Apter represents the ability of a social system to innovate constantly without falling apart, including the innovation of beliefs about change and of social structures, and requires a "social framework that can provide the skills and knowledge necessary for living in a technologically advanced world." (p. 197). However, the entire modernization process implies the introduction of roles that originate in industrial society into a non-industrialized one (p. 302). Since the latter is incapable of satisfying the expectations of the occupants of the modernized roles, control problems arise that require restrictive and authoritarian solutions.

In his discussions of the problems of modernization, Apter introduces and refines his categorization of political systems according to two basic variables; authority and norms or values. Authority patterns can be hierarchial—i.e. authoritarian —or pyramidal—i.e. representative, while value systems are consummatory/sacred or instrumental. The four combinations of authority patterns and norms result in four basic types of political systems: 1) mobilization, 2) theocratic, 3) bureaucratic, and 4) reconciliation. In the short run, he argues, authoritarian systems are more effective in achieving modernization, but by their very nature they lead to losses of information about the rise of values opposed to those of the elite and to the increasing political significance of claims for interest representation—i.e. they increase the likelihood of revolution.

In the only selection written specifically for this volume, Apter attempts to develop "A Paradigm for Political Analysis." He hopes that by focusing on the problem of choice political scientists will be able to reintegrate their discipline. Laudable as his purposes may be, it is very unlikely that Apter's proposal to treat choice as a problem of authority in its normative, structural and behavioral dimensions will ever achieve its purpose. First of all, like much of the author's work, many of the ideas in the essay are neither clearly expressed nor fully developed. For example, what exactly does he mean when he says: "We need to establish boundaries for [political science] around authority and to treat choice as a program of authority in its *normative,* or moral dimension (embodying both its wider objects and obligations), its structural dimension (involving the patterns of social relationship and organization which, analytically defined, contain social life), and its *behavioral* dimension (including the selection between alternatives presented by the other two)." (p. 363). In spite of the lack of clarity of Apter's paradigm, it is evident that he is pointing to a significant—perhaps the most significant—problem in political science; namely, the increasing divergence between normative political theory and empirical studies of political behavior.

Toward the beginning of this review the question of the value of reprinting Apter's articles was posed, since so many of them are available elsewhere. Actually the volume represents much more

than a catalogue of the intellectual development of Apter; in some respects it represents the development of studies of modernization over the past decade—from the rather optimistic views concerning the applicability of democratic institutions in developing areas and the hope that a study of developmental politics, unencumbered by preconceptions based on American and European examples, would enable us to understand better the "fundamentals" of politics to a much more sober realization that "the political aspects of development are far more complex than they appeared to observers a decade or so ago." (p. 331).

In these articles Apter deals with a number of significant problems of political analysis—such as the role of ideas in politics, the diversity of the modernization process, the need to "retraditionalize" political systems, especially the necessity of adapting them to traditional values. However, many of these topics are merely referred to and Apter's insights into such phenomena as the development of a middle class are not fully developed.

As do most collections of articles, this volume suffers from constant repetition. For example, Apter's methodological framework is presented, with minor variations, in most of the articles. Editing of the original articles to reduce this repetition would have improved the readability of the book; however, this would probably have resulted in a new edition of *The Politics of Modernization*.—ROGER E. KANET, *The University of Kansas*.

From Pluralism to Collectivism: The Development of Harold Laski's Political Thought. By BERNARD ZYLSTRA. (Assen, The Netherlands: Van Gorcum & Co., 1968, Pp. 236. Hfl. 25, 90.)

One welcomes today an intellectual portrait of Harold Laski (1893–1950). His shifting perspective and the intensity of his internal debates are now reflected in the ferment of an entire discipline. As Zylstra shows, the early Laski argued against the myth of the sovereign state, advancing instead a radical pluralist conception in which the state is just one among many associations serving human needs and competing for citizen loyalty. He later reinstated the government as the single most important agency for promoting and protecting interests common to the entire society, viewing it now as an arena where multiple groups advance claims and, ideally, as an umpire with the ability to balance competing claims according to the dictates of distributive justice and public interest. Finally, after the Depression and the rise of Hitler, Laski adopted an increasingly Marxist interpretation of arrangements in Britain and the United States, calling for massive social change through militant political pressure.

Why the progressive radialization? From Zylstra's exposition we catch a glimpse of Laski's own explanation. Equality of opportunity for the development of personality consistently remained the fundamental, if vaguely defined, social criterion for Laski. His early formulation of the pluralist *ideal* afforded opportunity for individual development within voluntary associations while providing limits to arbitrary state action. But, shortly, Laski began to face "hard facts." Political pluralism operated *in practice* within the confines of an increasingly centralized corporate economy; even a socialist party in office "cannot help but be aware of the fairly narrow limits within which it may successfully maneuver, and the danger to which it is exposed if it embarks upon measures which disturb the 'confidence' of men of property" (Laski, 1947). Pluralism remained Laski's ideal, but until the economic preconditions for pluralism were established the existing political balance would consistently work to the disadvantage of broad segments of society. Only militant pressure from below could redress the existing imbalance; only the government, responsive to these pressures, could institute the needed legal changes. The government's role relative to all other associations must expand appreciably to reduce economic inequality, and thus existing pluralist practice must (temporarily?) suffer while its proper preconditions are established.

"When Zylstra, though, seeks to explain Laski's radicalization, he traces the source to Laski's "apostate" secularism, especially to his ideal of social equality. Equality, Zylstra argues with some cogency, must have "qualifying reservations" if it is to be a viable political norm. There must also be some external authority to limit the role of the state if we are to avoid, as he thinks Laski does not, unintentional avowal of a totalitarian idelogy. Only one authentic basis is available: "No political ideal—whether that be equality, freedom, or the public good—can be a safe guide in the direction of the affairs of state unless it is subjected to the inner structural principle of the body politic, founded on the divine order of creation. Without this norm political reflection and action tend to fall into absolutization of either the individual or the community." This central postulate is asserted with vigor but defended with diffidence.

Zylstra's book moves on two levels. On the first we find a useful exposition of Laski's intellectual development and an insightful, but often overstated, critique of Laski's egalitarianism. On the second, we are told that Laski's "failure" exemplifies fundamental deficiencies in the entire secular tradition. Ignoring consideration of the social and economic conditions of pluralist politics (a central concern of Laski's), Zylstra concludes that pluralism must succumb to "autarkic" individualism or oppressive collectivism unless the "cosmonmic"

perspective is employed to "delimit" thought and practice. The latter argument, in my view, is hardly persuasive. Even if it were socially feasible to retinstate the "cosmonomic" view, its adoption would alter the style and unexamined bias of contemporary debates more than it would advance their rationality.—WILLIAM E. CONNOLLY, *University of Massachusetts, Amherst.*

The Political Sciences: the General Principles of Selection in Social Science and History. By HUGH STRETTON. (London: Routledge and Kegan Paul, 1969. Pp. 434. £ 2-15-0.)

This is a "study of just what valuations do and how they do it" in the social sciences. By "valuations" the author means not only good-bad valuations, but also (and indeed more so) important-unimportant, significant-insignificant, relevant-irrelevant valuations. Hence his subtitle; for he is concerned with the selection of a subject for investigation because it is judged to be important, of a piece of evidence because it is deemed to be relevant, of an objection to an explanation because it is judged to be significant.

These are his terms of reference. Within them, what is his thesis? He contends that the field of value has no limits, or, to put it in another way, that it is not possible to be value-free, in history or the social sciences or anywhere else. Therefore, to opt for "a value-free social science" is not to opt out of all values; it is to opt for a particular set of values. Moreover, Stretton asserts, the set of values chosen is bad in two distinct ways: it is bad technically, in that it has a built-in bias toward certain kinds of explanation, irrespective of their appropriateness to the subject, and blinds the investigator to other kinds of explanation by training him to ignore them; and it is bad morally, for the values that are inculcated are bad values. The second (and less important) of these objections has two parts: an objection that

"to teach students that their morals are extra-curricular is to deny those morals the very service which education and research should give them,"

and that, since values are inescapable,

"moral awareness had better be built into the process of research itself; which need be no more obscurantist than laboratory rules about safety or painless vivisection";

and an objection (likely to be quoted by hostile reviewers as a sample of Stretton's offensiveness) that the absence of such built-in moral awareness produces "professionally irresponsible skill for sale to the highest bidders." The second of these objections is less well argued than the first, and, while it could be detached without affecting any other part of his argument, its inclusion may reduce Stretton's chances of a hearing for his more central points. Even the first objection, though argued much more persuasively, is a detachable digression in what is essentially a book about the *technical* effects of value on *selection* in the social sciences. It is important to note, though, that Stretton rejects *Verstehen* arguments, not only of Croce and Collingwood, but even of Berlin and Winch, that all genuine study of society is understanding and valuing and nothing else.

For such an uncompromising book, it is remarkably free from straw men. Stretton asserts, for instance, that "strict behaviorism has few champions left." None the less,

"social scientists keep trying to exclude (values) by increasingly curious principles of selection,"

for

"a rule that for every effect, causes should be chosen for proximity, weight, and certain knowability . . . is always stupid."

Because

"moral abstinence, besides being a moral act like any other, often does serious *technical* damage to explanations by excluding many of their technically important parts,"

and has been linked, he says, with

"a revival, with new terminology, of some very old explanatory systems, especially single-factor systems."

No improvement is possible, if Stretton is right, through increasing professionalism, for the academic social scientist enjoys no great advantages, and suffers some serious handicaps, by comparison with more committed persuaders, who are

"prepared to stake more . . . on the truth and sufficiency of (their) knowledge. Its truth and sufficiency, moreover, may be tested by judges more severe than the publishers, prescribers and promoters within the pure scientist's profession."

And we are offered the paradoxical corollaries that persuaders like Lenin and Hobson, though not so good (as social scientists) as men like Langer and Viner, are a necessary precondition for their existence as social scientists; that a combination of the care of the social scientist with the passion of the persuader could produce something which would be both better propaganda and better social science; and that the uncommitted director of an uncommitted social science foundation would economise his resources by employing committed, not uncommitted researchers.

In place of "irrational imitations of the forms of physical science," Stretton offers two models for imitation, one more familiar than the other: economics, and law. Economics, "the best social science,"

"has prospered as it forsook generality, deduction from simple psychological axioms, symmetrically unified theory, and the search for sufficient explanation, for more practical attention to the facts of life and more specialization of diverse models and theories to the diverse particulars of relations in life."

And law is "a neglected good example to other disciplines," in that the lawyer, for all his solid achievement in abstraction, generalization, and even prediction, is commendably flexible, and knows when to stop abstracting and generalizing and simplifying, and when

> "to complicate his laws; to abstract them less and limit them more precisely; to classify more detail in finer categories instead of dismissing more detail from fewer principles."

The thesis of any such book as this can be readily abstracted—if the author has explained himself clearly—and briefly stated. The book's usefulness, however, depends on how fully and well the argument supports these contentions. Stretton wryly refers his readers to the *Massachusetts Review,* (Autumn 1961),

> "where the footnote-checker will be rewarded by much of the argument of this present book in seven pages."

But he does his book less than justice, for the *argument* is precisely what we cannot get in seven pages. That is why, particularly on a subject like this, a long book is likely to be better than a short book, and a book by a single author (if he has the necessary range to write it) better than a collection of essays by several hands; and it is the reason why this book is likely to be a useful one, and why Part 3 is such an important part of it. For it is particularly in Part 3 that Stretton undertakes the kind of detailed and extended examination of individual books, without which it would be very difficult for him to explain precisely how he intended his general points to be applied. And if these books, "too complicated for summary and too good to deserve it, . . . would be a good reading list for a first course in social theory," then the same books, together with Stretton's critique of them, would be no bad basis for a graduate seminar in the social sciences or in any one of them. For it is a stimulating book, not in the vulgar sense in which that phrase is applied to abusive books likely to provoke counter-abuse, but in the sense of examining important questions fully enough to provoke still fuller examination of them.

This is not to deny the book's faults. It is not established, and it is not necessary to Stretton's argument to suggest, that particular economic explanations are chosen (always?) by economists to give support to a chosen economic "therapy," though it may often happen thus. Even if—and particularly if—it were always so, we would still want to know how economists come to fall in love with their chosen "therapy." Again, it is not necessary to his argument, and it is a misleading way of describing More's use of *Utopia,* to say that it was "desired however impractically," and "imagined *because* (my emphasis) desired." But above all, it is not necessary for his argument for him to throw down so many gauntlets. He need not begin his book by comparing "scientism" with "a cargo cult in Melanesia, invoking magical plenty whenever conventional production is scantiest," or end it with complaints that "scientistic" social scientists use their position to exclude from the profession all but the like-minded. Even where he is funny (as where he rephrases an excerpt in language more to his liking, and adds the note: *tr. H Stretton*), he may be needlessly imperiling the hearing of arguments that he considers most important to be heard.—E. D. WATT, *London School of Economics.*

Sarvodaya: A Political and Economic Study. By ADI H. DOCTOR. (New York: Taplinger Publishing Co., 1968. Pp. 229. $6.25.)

"Sarvodaya," a Sanskrit word literally meaning "the rise of all," was given currency by Gandhi during India's independence movement. He used it to designate his political and economic thinking and techniques. Later his disciple, Vinoba Bhave, adopted the term to refer to the Bhoodan and Gramdan movements.

Sarvodaya is hardly a logically consistent philosophy of political and economic life. Much of it was improvised by Gandhi to meet the challenge of changing circumstances. Adi Doctor, who teaches at Marathwada University in India and who wrote this work for his doctorate in 1964, must be given credit for making an admirable effort to lend coherence to what was not only unorganized but quite obviously unorganizable. Gandhi maintained that "he was less concerned with being consistent with what he had previously said, but more concerned with being consistent with truth as he saw it from time to time" (p. 153). Whether one is pursuaded or irritated by the rationality or irrationality of Dotor's subject, one cannot deny that he has presented in this slim volume an able and faithful interpretation of all that goes under the banner of *Sarvodaya.* Perhaps he has succeeded so well that he has given the appearance of order to contemplative chaos.

Doctor begins by giving us a description of Gandhi's vision of an ideal society, the Ramrajya. Here the author is on somewhat surer footing because we know Gandhi's premises and their logical extentions. But when Doctor returns to the real world and begins to deal with the specifics of achieving that ideal society, he loses his grip. By now, the reader knows only too well what objects Gandhi's ideal society should not have. What he does not grasp with any degree of clarity is how the absence of these objects can be brought about or lived with.

Briefly then, at the political level, *Sarvodaya* views governments as engines of violence. Vio-

lence must cease in Ramrajya. But who, one might ask, will make decisions in the absence of governments and how. Presumably in Ramrajya there will be no conflicts because each will have according to his need. Decisions will be based on unanimity should conflicts persist, and those making the decisions will be elected by the unanimity of all participating. Should anyone resist a unanimous decision, others will *Satyagrah* to bring about "voluntary" compliance by the resisting party.

At the economic level *Sarvodaya* perceives machines as instruments of indiscriminate exploitation of men and material. But, it may be said, even Gandhi used and advocated the use of the spinning wheel. Ah, *Sarvodaya* is "not opposed to all machines" (p. 80) as long as the benefits of machines "accrue evenly to all" (p. 79). How will that be, one might ask, given human nature? *Sarvodaya* proposes that nurture will take care of human nature. Educate men to minimize their needs because we live in a world of scarcity. Resources are more likely to be in equilibrium with needs if needs are minimal. There will be no residual frustration if these minimum needs are met. In the absence of frustration and no more needs to fulfill, people will share their excess possessions or hold them in trust for others and thus live in harmony with each other rather than in a state of perpetual competition and conflict.

Bhoodan movement, dedicated to the proposition that "Sab bhoomi Gopal ki" (all land belongs to Lord Gopal) and therefore to all men who are God's children, sought to bring about a redistribution of India's agricultural land. Those in possession of more land than they themselves could till gifted it to Vinoba Bhave, the leader of Bhoodan, who in turn distributed it among the landless. But the case studies cited by Doctor point out that a substantial portion of the gifted land either remained idle under its new owners or reverted back to its original owners. Among the probable causes, Doctor mentions no disposition for agriculture among the many who received the land—they rather remain landless laborers—and the lack of capital goods with which to cultivate. And as Doctor puts it, "The typical Indian village of today, is more often than not, torn asunder by factionalism and governed by narrow considerations of caste" (p. 163).

Is the *Sarvodaya* view of political and economic life then totally invalid for today's India? Perhaps not. Rationalization of poverty has always fascinated the poor for they must live with their poverty. It may be many generations before India even approximates the material prosperity of the Western world. For these generations in waiting, *Sarvodaya* has a message, marvelously stated in the Bhagvadgita,

Advesta sarvbhutanam maitrah karun ev ch Nirmano nirhankarh samdukhsukh chami

(12th chapter, 13th sloka)

He who seeks Nirvana has no enemies for he is a friend to all beings; nor does he desire, for, to him, pain and pleasure are the same.

BHAGWATI P. K. PODDAR,
Youngstown State University

AMERICAN GOVERNMENT AND POLITICS

Frontiers of Judicial Research. By JOEL B. GROSSMAN AND JOSEPH TANENHAUS, editors. (New York: John Wiley and Sons, Inc., 1969. Pp. 492. $14.95.)

The twelve research papers which make up this book were presented at a conference held at the University of Iowa in October, 1967, supported by a fund honoring Benjamin F. Shambaugh, a past President of the American Political Science Association. What the organizers of the conference did, in effect, was to bring together a group of interesting, productive scholars and give them the opportunity of setting forth in tentative fashion their current research interests. It is of course not to be expected that each of these projects constitutes a "research frontier" in any meaningful sense. Several of the papers are truly innovative in theoretical or methodological terms. Several constitute progress reports on major ongoing projects, while others describe completed research. The comments which follow are necessarily highly selective. This is an important collection, and those concerned with judicial behavior will certainly wish to consult it directly.

Three of the papers will be of special interest to methodologists. Grunbaum presents a model of Supreme Court decision-making based on simulation techniques. The model is intended to incorporate both attitudinal and *stare decisis* considerations, and employs a combination of independent and conditional probabilities.

Schubert's study of the Australian High Court introduces smallest space analysis—"a new multidimensional scaling approach which is particularly appropriate to the investigation of matrices of agreement scores that reflect weaker than interval measurement." Smallest space analysis allows Schubert to investigate asymmetrical matrices, he compares matrices based on both opinion and voting data. His comparison of voting agreement in split decisions and opinion agreement in unanimous decisions led to the conclusion that the same

two attitudinal scales are components of both voting and opinion behavior. It should be noted that Schubert deals only with patterns of opinion *agreement*, and that other aspects of judicial opinions may manifest different political behavior.

Ulmer employs the discriminant function to explain Justice Frankfurter's civil liberties decisions. He treats cases as complex stimuli which flash different signs to different people. To avoid the claim that the signs with which judicial votes are correlated are subjective responses by the Justices, Ulmer derives them exclusively from lower court records and the briefs. On the basis of six extra-legal signs so derived, he was able to explain Frankfurter's votes in 83.8% of the civil liberties cases of 1959–1960.

In recent years it has become increasingly fashionable to study non-American judicial systems. Four of the papers in this volume are so directed: Schubert's work in Australia, Danelski's study of the Japanese Supreme Court, Kommers' paper on the West German Federal Constitutional Court, and Morrison's study of the Swiss Federal Court. The thrust of contemporary scholarship in the field of comparative politics is obviously non-institutional, and it will be interesting to see whether truly comparative cross-national studies can be developed by scholars who are oriented towards specific institutions. For example, Morrison demonstrates that the Federal Court is not a significant factor in the Swiss political process. This would appear to be the end of comparative *judicial* analysis. But his finding suggests a host of questions to the student of comparative politics, more broadly defined.

Danelski and Kommers report that one factor inhibiting the development of a strong judicial role in Japan and Germany, despite Constitutional provisions creating judicial review, is the widespread realization that the courts are in face political bodies. It is interesting to ask whether a judiciary can develop sufficient legitimacy to permit it to play an active part in politics in an age of legal realism. Perhaps popular belief in "legal magic" is a necessary pre-condition of effective judicial review.

The impact of legal realism on American state judges is discussed by Vines in the one truly comparative analysis in the book. His study of judicial role perception—based on interviews with 26 of the 28 justices of the supreme courts of Louisiana, Massachusetts, New Jersey, and Pennsylvania—takes the theoretical orientation of Wahlke's *Legislative System*. Vines demonstrates that more than half of the justices define their roles in terms of "law-interpreter" rather than "law maker" and that they minimize their policy making function. While there are considerable differences in the role perceptions of judges in different states, these differences do not seem to be related to the method of judicial selection, political party affiliation, or the ideological content of the judges' opinions and voting patterns.

Vines' research opens a wide range of questions. What are the sources of differing judicial role perceptions? Do judges differ from other political elites in this regard? Does the general public—as measured in Murphy and Tanenhaus' groundbreaking study, parts of which are reported in this volume—perceive the role of the judiciary in different terms than do the judges? What effect does this have on the Supreme Court's ability to manage the judicial bureaucracy?

Dolbeare convincingly demonstrates the necessity of directing systematic attention to the actual impact of judicial decisions on public policy, and he has begun to lay the groundwork for such a study. Of particular interest are his comparisons of state and federal courts in terms of the "output-impact focus." State and federal courts are used by litigants with different policy orientations; their impact on public policy differs in important respects; and federal courts are less fully integrated into the urban policy process than are state courts. Dolbeare may have overstated the case for a "bottom-up" view of the judicial system by emphasizing the small number of cases in which trial courts are reversed. Reversal by the Courts of Appeals in 9% of the cases considered may have been enough to keep the district judges "honest." We will need direct evidence from the trial judges before we can generalize about their awareness of hierarchical pressures.

One paper in the collection is seriously flawed. To test the hypothesis that experience with a court influences attitudes towards the judiciary, Jacob administered questionnaires to persons who had undergone bankruptcy or garnishment proceedings, and to a randomly selected control group. When comparing their responses, he treats *minute* differentials—differences as small as six tenths of one percentage point—as meaningful. Having explicitly rejected tests of statistical significance, Jacob makes no attempt to explain how he knows that differentials which appear trivial to the naked eye are of any interest. Contrary to his assertion, his data suggest that attitudinal differences between the debtors and the control group are related to differences in education, income and occupation.

Even more unfortunately, one of his basic conclusions is contradicted by his data. Jacob asserts that "those with experience in the courts felt less efficacious toward them than those without such experience." But data he presents in separate tables demonstrate that this is a half-truth. Those whose courtroom experience was in a garnishment proceeding do have lower scores on the judicial ef-

ficacy scale than does the control group. But in five out of eight categories, those whose courtroom experience was in bankruptcy proceedings score higher on the judicial efficacy scale than the control group, holding education, income and occupation constant. Thus Jacob's data support an alternative hypothesis: a "successful" (i.e.: bankruptcy) experience with a court increases one's feeling of efficacy toward it.

Finally, without detracting from the value of the Shambaugh Conference, one may question the utility of a volume such as this one as a mode of communication among scholars. The price of the book is such that only libraries and reviewers will own it. Research papers of this length could have been published as journal articles. Perhaps the latter course of action would have better served the scholarly community.—EDWARD N. BEISER, *Brown University*.

Organizing Men and Power: Patterns of Behavior and Line-Staff Models. By ROBERT T. GOLEMBIEWSKI. (Chicago: Rand McNally and Company, 1967. Pp. 277. $6.00, $3.50 paper.)

Robert Golembiewski's unusually prolific efforts in contributing to the literature of administration have continued to a point where a pattern is clearly discernible. Both the approach and objective of his work have remained rather consistent —he is derivative and synthetic in approach and his objective is to revise the practice of administration. His recent work on the problem of organizing the staff function in management bears the cut of this stamp in bold relief.

The theme of the book is straight forward—it is an argument for a "new concept" for the staff function—and it is organized generally to a format often employed by Golembiewski—critique of the concepts of the traditional theory (both conceptually and in terms of the contemporary realities of organization life), explication of the alternative concept, evidence on the effects (advantages) that accrue from its application in practice, and the place of the concept in "modern organization theory." The concept for staff role in the "traditional theory" Golembiewski self-descriptively dubs the NII (Neutral and Inferior Instrument) model and it is seen as increasingly unworkable as the pace and scope of change, size, and technological level characteristic of organizations increase. In addition, authority relations—and authority is of course *the* problem of the staff-line relationship—are changing in modern organizations, in the direction of being "less direct, arbitrary, and authoritarian. A number of (to use one of the author's terms) "minianalyses" covering specific aspects of organization and the management process are offered to make the argument in more detail—sources of line-staff tension in everyday operations and the tendency of the NII model to cause vertical fragmentation of activities which increasingly require horizontal integration with the growing interdependence of specialized parts in common flows of work.

With the defects of the NII model effectively demonstrated, Golembiewski then brings his alternative on stage: the "Colleague Concept." This concept is shaped to the objective of moving "away from unnecessary stress and repression, and this by maximizing the chances of horizontal integration." The structure which it implies emphasizes coequal cooperation of line and staff units through a "colleague group" and "unit team" structure. While the book notes that this concept is only an approximation, specific guidelines are offered for implementing the alternative approach as well as an extensive argument for its efficacy in reducing tensions and in yielding other beneficial side-effects. The analysis concludes with a description of how the "Colleague Concept" fits the new research on organizations—that it is consistent with the now famous "Theory Y" of management, fosters creativity, eases work stress, facilitates use of computers, and increases integration.

As was noted, this book is very much within the "Golembiewski genre," but it nonetheless diverges from this pattern in two important aspects. One of these is the emphasis on power in organizations—an emphasis which the author asserts was neglected in the traditional theory but is the "warp and woof of this analysis." This "emphasis" amounts to the assumption (made by our founding fathers in drafting the Constitution, see notes) that men are intrinsically both good and evil and possess an insatiable desire for power. The task for organization theory—specifically in designing line-staff relations—is to take these forces as given and make them work *for* instead of against organization purposes. By assuming this emphasis as a starting place, Golembiewski appears to move away (and thereby detracts) from a most important theme that formed a significant part of his contribution in *Men, Management, and Morality*. In that book (and even implicitly in the argument he makes here) the position is taken that the "nature" of men and hence the quality of their lives can to a significant extent be set by the structure of human organizations. This posture supports the normatively uplifting and positive view that people and life can be bettered and that men can perform a positive role in the improvement. And it further highlights the importance and relevance of the study of organizations in an organizational society. The tie-in Golembiewski makes to the founding fathers marks the nature and weakness of his basic assumption in supporting his analysis. It could be argued that it is the ethically ambivalent and somewhat pejorative view of the nature

of man reflected in our constitution which has in part resulted in the social and political crisis we are currently facing in the United States. The model of interpersonal relationships implied by the Colleague Concept assumes more of people than the "power orientation" allows. Indeed, it seems clear that if people are the way the "power orientation" view defines them, the colleague structure urged by Golembiewski would operate in a fashion characterized by growing power imbalance or instability, and oftentimes friction and stalemate.

Another difference of this book with much of the author's earlier work is a deficiency of creative synthesis. One of the strengths of his past work has been the ability to go beyond simple use of a wide range of existing research into both novel arguments and imaginative synthesis of fresh insights, conclusions, and implications. This book offers little of this rewarding stuff, save for brief notes such as those on the relation of staff-line structures to use of computers and to potential for creativity.

In fact, the College Concept is so close to the prescriptions clearly implied by the organization research existing when this book was written that it evokes somewhat the feeling, to use the style of its author, that a "house has been burned to roast the pig." This impression is reinforced by the informal, embellished, "bomb-the-point-home" style used to carry the argument—which seems to be an attempt to speak in ever so convincing terms to the practicing manager.

Because the content and tone of the argument tend so much in this direction, in turn, one finds as he nears the end of the book that two very large, old, and nagging questions have again surfaced in the midst of a treatise on administration. One of these questions concerns the serious and persistent gap between the "theory" (or study) of administration and its practice, and the other the proper definition of the relationship between the study of administration and political science—with its corollary concerning the practice of administration and the real world of politics.

Why is the theory-practice gap so persistent? We have known what Golembiewski told us in this book, at least by a general pattern of research findings, for a good while. The "traditional theory" has been whacked at from every corner of the field and long since put down and out in "modern organization theory." Why was a book written in 1967 which not only details an alternative approach to a significant management problem but in addition goes on to labor the underlying theoretical argument—and why does the reader feel, albeit somewhat uncomfortably, that the author needs to do thus? The answer, of course, is that our common experience tells us that the world is still being run in accord with the traditional theory. Golembiewski's strategy in confronting this problem is to reinterpret, reword, and sell the knowledge developed in the academic enterprise in the marketplace of management. At some point we must begin at least to wonder whether the basic problem of communicating to the world outside resides in the institutions and professional contexts within which this knowledge is developed—and that the only effective coping strategy is one which addresses the problem at this level.

The other question (administration, study and practice, and political science and politics) is posed in two ways. One is incidental to this book —the fact that the examples drawn and the data employed fall almost exclusively in the area of industrial manufacturing concerns—but the other is general. Given the fact that a central aspect of modern society is large scale, widespread organization, with deep consequences for our political system that are being noted by, among others, an increasing number of commentators on the pluralist technique, it could be asserted that a major mandate to those who are writing the contemporary administration literature is to tie their studies into this broader, immediate, and extremely important area of concern. This was accomplished in *Men, Management, and Morality* effortlessly and as a by-product. As Golembiewski shows, the staff junction problem is a major source of organizational ineffectiveness. Therefore in spite of the fact that his analysis is technical, relevance to the broader question of political viability is inherrent in it and should have been drawn. For this reason one would hope that in the future this author would move toward the direction of his earlier work, where relevance to political science and politics was clearer.—ORION WHITE, JR., *The University of Texas at Austin.*

Judicial Power and Reconstruction Politics. BY STANLEY I. KUTLER. (Chicago: The University of Chicago Press, 1968. Pp. 178. $5.95.)

To many casual and not-so-casual students of the American Political System, the Civil War and Reconstruction era finds the Supreme Court where Professor Kutler expected to find it when he began this study, at "the nadir of judicial power and influence." This volume of essays should mark the end of such an ill-conceived notion.

Professor Kutler securely identifies the judiciary as a participant in those extraordinary political times and much more of a citadel to be captured by the Republican Party, than an institution to be either ignored or destroyed. Professor Kutler lays bare the often delicate interplay of Court and Congress which provides us with a rare laboratory in which to examine the role and "toughness" of

the Court under stress during a period when politics was inflamed and polarized.

Kutler further demonstrates how Congress inexorably increased the reach of the Federal Judiciary in our system through attempts to expand Federal protection in the face of state hostility to Congressional policy. While the base of power thus grew, the Supreme Court was busy at the apex giving substance to judicial review as ten Acts of Congress fell in eight years where only two had fallen in the previous sixty-one years. Some of the more valuable portions of the book expand on this theme in some of its less obvious, but more profound, implications.

In sum, this period was marked by the growth of judicial power rather than by judicial abdication. True, this was achieved in the face of *Dred Scott* and during adverse times. But Kutler demonstrates that the political conditions of the war and reconstruction created many of the circumstances leading to this growth of judicial power. As importantly, the author makes clear that to prosper in such soil, the Court's roots must have already grown deeply and well.

Even though all these good things and more abound, an uncertain sort of tentativeness lingers about the book. A major problem may well be that Kutler casts the work as if he were refuting a well-developed, systematic interpretation of the Court's role during the period. Actually, no *interpretation* of the Court's role in these years has ever been developed. The author thus assigns himself to attack random opinions and formless misconceptions without supplying an adequate form of his won. A certain vagueness results from this concern with destroying that which isn't there. Ink could probably have been better spilled by constructing an interpretation while whacking away at the misconceptions. It seems that several unfortunate streamlets flow from this spring.

First, the more specific themes in the book appear haphazardly. The author apparently committed himself too completely to refuting this instance or (as is more often the case) casting doubt on that one, to bring the strands of the work safely through the book. For example, after seeming to dispose (incorrectly, I think) of the notion that the Court was constitutionally opposed to even some Reconstruction legislation and that Radical Republicans need not have been concerned about the Court, the author tells us suddenly in the concluding chapter that "the practical limits of judicial power and an emotionally charged, polarized political situation inhibited the justices regarding the Reconstruction legislation. . . ." (p. 164)

Secondly, the refutations come too easily at times and some seem grounded in shifting standards. For example, the standard used to debunk the so-called danger to the Court from Congress seems so high that one wonders at times whether the author would consider the Court in danger anywhere short of its actual destruction. Further over, however, we learn that the stripping of the Court's jurisdiction to hear the McCardle case was a serious slap at the Court. The whole, in short, suffers from the format's dictation of the way the parts will be handled, and detracts from the consistent development of the really important themes.

I must note that the author seems to be considerably more in debt to Harold Hollingworth's dissertation (*The Confirmation of Judicial Review under Taney and Chase*) than is acknowledged. Professor Hollingsworth develops the theme of judicial activism during this period, and merits some generous acknowledgment from Kutler.

Finally, the reviewer is dogged by the nagging thought that this book may be a bit premature. Evidential points which are available in materials the author researched do not appear at important junctures, and some problems in analysis seem unnatural from the pen of one so skillful. Had the author devoted more than the "few years" he mentions in the introduction of work in a field new to him, certainly we would have a richer product. But one should no doubt rejoice more and complain less. For, not only is the product rich and important as it stands, but no student of our political process can again ignore the Reconstruction Court.—DAVID F. HUGHES, *Centre College of Kentucky.*

A Decent Home. The Report of the President's Committee on Urban Housing. (Washington: U.S.G.P.O., 1968. Pp. 252.)

The Housing and Urban Development Act of 1968 established a goal of 26 million dwellings, included six million subsidized units for low and middle-income families, to be built or rehabilitated in the next decade.

President Johnson's Committee on Urban Housing issued its report, *A Decent Home,* in December of 1968. The Committee was charged, in standard Johnson rhetoric, "to find the way to harness the productive power of America—which has proved it can master space and create unmatched abundance in the market place—to the most pressing unfilled need of our society." It will come as no surprise to anyone that the Committee failed to discover THE WAY. What the Committee did do, to its unfading credit, however, was to produce one of the most useful, readable and thorough analyses of the inputs of the American housing system to appear in recent times.

Following the format of many such studies, *A Decent Home* opens with a concise committee summary containing findings and program recom-

mendations. The latter range from the suggestion that "all Federally subsidized and Federally insured or guaranteed housing, except public housing, should be financed by bonds insured and guaranteed by the U.S. Government," to the slightly time-worn but worthy advice to channel more Federal funds into research and development activities. The committee report is followed by a series of staff reports which are, not unexpectedly, the treasure troves of the piece.

The staff studies cover many, though by no means all, of the housing system's variables. Absent, for instance, is any discussion of the role of architects or for that matter any reference to the galling problems of housing design. And having once more quoted the goal of the National Housing Act of 1949, "A decent home and a suitable living environment for every American family," the staff chose to ignore the problems of environment altogether while it concentrated upon the problems which stood in the way of constructing the house itself. As a consequence, one finds a very worthwhile paper on housing finance and an excellent overview of the housing industry, but nothing, for example, on neighborhood planning.

In the Housing Industry section, one is treated to a well controlled study of private and public sector overlap. The housing process is divided into four phases—preparation, production, distribution, service, and the participants and sources of rules are charted for each phase, e.g., title company, sub-contractor, broker, insurance company; real estate law, union rule, recording regulation, housing code. All of these elements are not accorded adequate coverage. The productive aspects alone are given a full treatment.

If one trusts to the conventional wisdom that the building industry has not changed in character since the time of Prince Hal, or that union members in the construction trades are America's most overpaid and underworked drones, he is due for some well researched counter-evidence. The home building business is, in the words of one staff study, "fiercely competitive," and it has introduced incrementally a series of technical innovations, both factory and *in situ,* which have substantially altered the methods of home erection. Indeed, it is the housing consumer who is conservative, for he insists that new materials be used in such a way that they resemble old. Thus aluminum siding must be shaped to give the appearance of wood, and new types of interior wall materials must mimic the qualities of plaster.

This is not to say that greater efficiencies cannot be introduced so that the nation has a better chance of attaining its ten-year goal in housing, but the studies reflect a doubt that significant additional economies can be achieved by factory assembly, one of the more common suggestions currently received. Further efforts in that direction will, it is feared, pile up costs of handling, warehousing and transportation. Rather, the report stresses fuller employment of the presently underutilized labor supply, in particular by adapting more of the available techniques of winter building. Such changes require alterations in union work rules and local ordinances as well as industrial decisions.

More than any other single individual, HUD Secretary George Romney will be held responsible by those who keep such accounts for the Nation's success or failure in meeting the housing target. If Secretary Romney's good fairy were to grant him wishes three, one doubts, on the basis of the material in *A Decent Home,* that he could utilize such an ethereal grant-in-aid with any greater success than the last known character so confronted. The choices would be agonizing.

On the other hand, a careful reading of the Report should leave him clearer-headed about his options. He would be less likely to throw away his first wish on improved labor productivity in home-building which the Report thought to be almost as good over the years as general industrial productivity. He might wish for more and cheaper land in metropolitan areas. (In 1950, average site cost for a new FHA-insured one-family house was twelve per cent of the total house price. By 1967, the average site value had climbed to 20 per cent of the net cost of the house.)

If this reviewer could whisper in his ear, he would say, "Mr. Secretary, sir, whatever else you do, ask that good fairy to incline the heart and mind of Richard M. Nixon to avoid those monetary and fiscal policies which in the past have so severely restricted the flow of available funds into the housing market."—PAUL A. PFRETZSCHNER, *Lafayette College.*

Problems in Public Expenditure Analysis. EDITED BY SAMUEL B. CHASE, JR. (Washington: The Brookings Institution, 1968. Pp. 269. $6.75.)

The five papers and ten commentaries presented in this volume, along with the editor's introductory chapter, constitute the latest of the books published under sponsorship of the special Brookings program entitled Studies of Government Finance. The papers provided the background for the second Brookings conference on government expenditures. Like its sister volume *Measuring Benefits of Government Investments,* which resulted from the first conference, the focus is on the application of welfare economics to federal government expenditure policy— or, more specifically, the measurement of the benefits and costs of governmental activities. Unlike its sister volume, this book is largely concerned with certain of the problems of benefit and cost measurement that

cut across a number of program areas. Although the issues are mostly technical in nature, the book is not designed solely for the specialist but, instead, is aimed at the wider audience of those who have an interest in improving governmental expenditure policies.

The first essay is by Roland N. McKean and is concerned with the use of prices, both when markets do and do not exist, as inputs into cost-benefit analyses. McKean lays particular stress upon his view that choices of governmental expenditures are "group" choices, which means that there is no ultimate test of preferredness or any ultimately correct social welfare function. According to this view, the analysis of public expenditures should be conducted in a manner which is similar to consumers' research in that alternatives and their implications should be pointed out, but this analysis does not point to a "proper" decision since different tastes mean that the various implications will be evaluated differently. McKean goes on to point out that even if one accepts efficiency or Pareto optimality as a goal, there are problems associated with the use of prices which are inherent to cost-benefit analysis. There is no need to summarize the argument here but one should point out that McKean is more pessimistic than either of his discussants, Allen U. Kneese and Julius Margolis, who point to some of the successes of systematic analysis and instances where "artificial" measurement techniques have been devised which can serve as substitutes for the non-existent prices.

McKean's essay is useful and informative in the sense that it points up difficulties inherent in any cost-benefit analysis. On the other hand, inherent difficulties need be no cause for despair. McKean summarizes his attitude by invoking an answer Maurice Chevalier is said to have given when asked how he felt about growing older: "Well, there are lots of things wrong with it, but it's not so bad when you consider the alternative."

The second essay is James R. Nelson's useful discussion of the value of travel time. Historically, the savings of travel time has been considered one of the most important benefits of improved transportation and there have been numerous studies which place monetary values upon the saving of time. Nelson reviews much of this literature and points out the tremendous variation in the values which have been assigned to an hour saved. The most interesting part of Nelson's essay consists of a critique of the methodology by which time has been valued. He argues that the problem is far more complex than has commonly been realized, and he rejects the use of the wage rate as the sole criterion. He documents his case by citing and interpreting some very interesting evidence concerning air travel into and out of New York. In commenting upon Nelson's paper, Steiner develops an important if illustrative model which demonstrates why the search for a single value of time is a quest in vain. Neither Nelson nor his discussants, however, are able to provide a convincing argument which demonstrates what the appropriate methodology should be.

The third essay, by T. C. Schelling, is concerned with the problem of evaluating programs which reduce the risk of death. Schelling argues that the worth of human life is not at issue, but that we should be concerned with the probability of death. Hence, his argument centers upon the issue of determining a method of placing a value upon reductions in this probability. While the discussion is interesting and lively, Schelling does little more than point out the associated difficulties. His discussants, Martin Bailey and Gary Fromm, do not appear to agree with Schelling's definition of the problem nor with his approach to it.

The fourth essay, by Burton A. Weisbrod, discusses the possibility of introducing a consideration of income redistribution into cost-benefit analyses. Weisbrod correctly points out that at present the great majority of the cost-benefit studies ignore the issue of redistribution. Two suggestions are made. First, he suggests that cost-benefit analysts adopt the practice of systematically tabulating the (net?) benefits according to the recipients' age, income, region, and color. Weisbrod recognizes, but does not emphasize, difficulties associated with this proposal. Despite the difficulties, it probably has substantial merit. Second, Weisbrod follows a revealed preference approach for the development of a method of inferring the implicit weights assigned by congressional decision-makers to specified groups when certain projects are adopted. Having proposed the method, he then goes on to apply it for the case of four water resource projects. It should be pointed out that both of his discussants, Robert H. Haveman and Ruth P. Mack, raise certain objections to the procedure. However, neither was intrigued, as was this reviewer, with the fact that the estimated implicit weights in the example for whites with incomes less than $3,000 per annum and non-whites with incomes greater than that amount were negative! One can speculate on the meaning of this result.

The final essay, by James T. Bonnen, fundamentally differs from the others in the volume in that it is concerned with a specific program rather than a general problem of cost-benefit analysis. Bonnen analyzes the distribution of gross (certainly not net) benefits which result from the program of cotton price supports. He is careful to explain the outlines of the working of the program. His methodology for estimating gross benefits appears to be straight-forward, although the details

of his calculations are not shown or explained. Bonnen is concerned solely with the distribution of gross benefits and does not consider the allocational aspect of the price support program since it clearly would fail any cost-benefit test. The calculations reveal, the arguments of those who support the program notwithstanding, that the gross benefits are not evenly distributed. "In general about 50 percent of the benefits of the cotton price support program go to the top 10 percent of the larger cotton farmers in each region . . ." William M. Capron, while discussing Bonnen's paper, observes that the estimated distribution of benefits has "little relation" to the rationale by which Congress has been persuaded to appropriate the necessary funds but no one dwells on the implications of the point.

One cannot conclude that this book has been successful in developing generally acceptable solutions for many of the unsettled issues of cost-benefit analysis. Such was probably not the intent anyway. However, it is a useful book and it will probably find its way onto the reading lists of those who teach cost-benefit analysis, and it should be read by all those who are concerned with the analysis of public expenditures.—Otto A. Davis, *Carnegie-Mellon University.*

Modernizing American Government: the demands of social change. Edited by Murray S. Stedman Jr. (Englewood Cliffs: Prentice-Hall, Inc., 1968. Pp. 182. $4.95.)

Scholars and politicians would both agree that if our ancestors were to suddenly come forth from their graves today, one of the few things they would recognize about modern life would be the structure of governments. In this compact, easily readable collection of essays, Professor Stedman and other contributors describe how our political institutions have coped with new problems and social changes confronting American society in the post World War II era.

The essays are empirical and operational in their outlook, rather than theoretical, as each writer notes proposed reforms for transforming our governmental institutions and assesses the prospects for achieving meaningful changes in the future to meet the demands of social change. As a supplemental text for American government courses, undergraduates will find this short volume ideal for determining whether our governmental structures are meeting the pressing demands of the twentieth century. A selected bibliography, organized around the basic political institutions discussed by the writers, would have been a welcome addition to the book in encouraging further study.

Professor Stedman sets the stage in his introduction by highlighting the impact of future developments on our society. The conflict and tension that exist between political institutions and the forces of change are carefully noted. Each contributor to the volume evaluates the prospects for modernization for the different governmental institutions using Stedman's definition of political modernization as "the process by which the power—allocating institutions of society improve their ability to meet the new demands resulting from social change." (p. 7)

Professor Koenig's essay on "The Presidency" shows how the office of chief executive has been very adaptive to change since the end of World War II. Proposals for reform in the areas of presidential elections, campaign financing, and powers of the President are ticked off in rapid fashion. The call for greater presidential power over personnel, planning, and administrative direction of the executive branch is sounded, but the obstacles to achieving these reforms might have been more thoroughly discussed and evaluated.

The essay on "The Congress" by Professor Derge notes the difficulties the national legislature has in competing with the Presidency in the sphere of foreign relations, and presents some significant proposals for strengthening Congress's informational handling capacity. Derge is quick to recognize that many recommendations in the internal reform of the legislative branch require psychological changes in the climate of Congress which are not politically feasible today. In a day when many individuals feel alienated from the political system, Derge might have discussed proposals for embudsmen, consumer protection legislation, and bills for reducing poverty which indicate that steps are necessary to close the "communication gap" between the citizen and his elected representatives in Washington.

Professor Magrath's essay, "The Supreme Court and A National Constitution," succinctly describes how the Court has conferred supreme economic and social welfare policy-making on Congress and the Presidency, while concentrating its attention on preserving individual liberties and rights, and upholding majority rule in legislative apportionment cases.

"Administration: The National Security Agencies" by Professor Thumm is a concise, well organized essay which describes the various agencies and committees used by Presidents Eisenhower, Kennedy, and Johnson to provide coordination, planning, and systems analysis among all governmental organizations involved in foreign policy-making. The description of the failures of the State Department in making long-range plans is particularly illuminating.

Stedman sees in his discussion of "Political Parties" the need to centralize decision-making power if the national presidential parties are to be modernized. Characteristics of both a personalized

presidential party and an institutional presidential party are briefly noted, but some comment on third party developments would have strengthened his essay.

Professors Gilbert and Smith in "The Modernization of American Federalism" highlight some of the critical problems, such as socioeconomic disparities and wide variations among states as to fiscal capacity, which concern state governments today. The authors discern six significant trends of federalism since the days of the depression and show in the case of public health how private federalism has evolved in the United States.

In his essay on "Local and State Government" Professor McKee relates why political institutions at the state level have been successful in withstanding changes and shows how the theory of nonpartisanship advocated by the National Municipal League is incompatible with the popular conception of American federalism. The writer illuminates the problem of judicial reform resulting from the lack of well-understood procedures for relating the judicial function to the political process.

In a short summary, Stedman sees the prospects for modernization of political institutions encouraging with the continual expansion of urbanization and industrialization of Americal life. This succinct volume provides constructive suggestions for the restructuring of American governmental institutions that will enable national, state, and local governments in the future to meet the needs of our people.—J. DAVID PALMER, *Georgia State College.*

An Introduction to the Legal System. By JAY A. SIGLER. (Homewood, Illinois: Dorsey Press, 1968. Pp. 248. $5.50.)

This endeavor cannot shake free from the paradox which attended its birth. It is a book which proceeds from an empirical frame of reference, yet its basic premise—that jurisprudence is fading as an intellectual activity, being replaced by an analysis of the legal system—is nowhere established in the text by measurable evidence. The superstructure which Sigler felt it necessary to erect as skeleton for his presentation, a systems analysis approach, contributes little to the reader's perception of the actual role of law in society.

The author attempted a marriage of law, political science, and sociology using a cybernetic analogy. His vocabulary of "input," "output," "feedback," "information," and "homeostasis" was an effort to provide a common terminology for legal scholars from different diciplines. If successful this heuristic device (Sigler acknowledges that it is a way of ordering experiences, not a final version of legal reality) could be generative of fresh insights or suggestive of future research. Unfortunately, it seems an unnecessary distraction from the value of this book. (At one point there is even an apparent confusion between what is an input and what is a feedback.) What Sigler has given us is really a study of the functioning of the legal process in the peaceful settlement of disputes. It is also a report of research findings, "consolidating gains already attained, while suggesting future tasks." The systems framework is a device for orienting the reader in the stream of legal phenomena; it is less successful as a source of testable propositions. Sigler is most interesting when not preoccupied with verbiage. The work gets stronger as it proceeds.

One of the studies cited, which is worthy of more than a footnote's attention, is that of Phillip Ennis on crime and punishment as seen by the victims. Only 50 of the 2077 "crimes" alleged to have been committed resulted in conviction and only eighteen per cent of the victims questioned said they were satisfied with the outcome.

The highpoint of the book comes with Sigler's insightful examination of the behavior of the Supreme Court. He explores the Court's strengths and weaknesses—where it can advance and when it must retreat. (The author makes the assertion that the power of Congress to expel its members, or to otherwise discipline them, will not be interfered with by the Court. One wonders whether this precedent will be followed in the Adam Clayton Powell suit.) There is a comparison made of the way the Court has treated different minorities (Negroes, unrepresented voters, accused person, Communists); their interests are sometimes sacrificed by the Court to protect itself from its critics wrath. Sigler has a provocative discussion of the emergence of a new right to privacy with especial reference to the *Griswold versus Connecticut* case.

One of the strengths of this portion of the book is Sigler's discussion of compliance studies (examinations of the follow-through to Supreme Court decisions). Yet even in this section, there are several annoying errors as in his discussion on page 217 of a study of the consequences of Supreme Court reversal of cases and remanding them to state courts for retrial. Oliver Wendell Holmes is misquoted on the Supreme Court's role as umpire of the federal system. It was Brandeis, not Frankfurter, who wrote the oft-cited Ashwander rules.

Though the book is entitled *An Introduction To The Legal System,* the author's choice of material is almost exclusively American. At a time when a comparative focus is becoming common, Sigler's perspective is too limited for his intentions. The writer must have been aware of deficiencies in his work. His "Afterward" is in part a justification for

his approach. It is also a misplaced survey of philosophies of law and an annotated bibliography. His vocabulary is particularly unfortunate at this point. Most political scientists' contributions to the field are labeled as "traditional" or "conventional" scholarship. Only the behavioral approach escapes the stigma.—MARTIN GRUBERG, *Wisconsin State University at Oshkosh.*

COMPARATIVE GOVERNMENT AND POLITICS

Political Power in the Soviet Union: A Study of Decision-Making in Stalingrad. By PHILIP D. STEWART. (Indianapolis: The Bobbs-Merrill Company, Inc., 1968. Pp. 227. $3.95, paper.)

Obscured by secrecy and propaganda Soviet politics has been considered one of the mysteries of the modern world, and too often the Western view of Russia has depended on the cult of Kremlinology, i.e. divining the inner workings of the system from the entrails of the vast Soviet propaganda machine. It is through such painstaking works as this study by Philip D. Stewart of Ohio State University that hard data are being assembled to fill in and correct the impressions of Kremlinologists. Carefully reviewing *Stalingradskaia Pravda,* the organ of the party in the Stalingrad blast, for the years 1954–1962 and the official history of the Stalingrad party organization Professor Stewart has documented the changing pattern of political influence in one oblast in the center of the Soviet Union. He has also related the developments in this one oblast to the overall workings of the party as reported in the general party literature. Although it would be difficult to prove this a representative oblast, it is a major producer in both agriculture and industry, and there is no reason to believe its general pattern of operation is not typical of a large number of oblasts or regions in the U.S.S.R.

The compilation and analysis of the local material on the Stalingrad Oblast party organization is careful, well-organized and lucid. Given the paucity of the data (party activities are never considered part of the public domain in the Soviet Union) Professor Stewart has used his material to the maximum to give a very revealing picture of oblast politics, although on occasion he bores the reader by unnecessarily going into the small details of individual issues having an obscure relationship to his thesis. It is in drawing conclusions and hypotheses from the local material to help explain overall Soviet politics that the author seems not to have made the best use of his material.

One of the strangest of all Khrushchev's reforms was his decision to defy all tradition and bifurcate the party in 1962, into an agricultural and an industrial sector. The present Soviet leaders have dismissed this reform as an irrational act of a leader who had lost touch with reality. Western analysts are likely to treat it in the same vein or suggest it was to prevent agricultural interests from being lost as the party assumed more direct control over industry. The evidence in this volume, however, suggests a very different and almost the opposite explanation.

Professor Stewart asserts from his reading of the general party literature that although "prior to the industrial reorganization the competence of the obkom bureau to deal with problems of industry was limited to questions of secondary importance, then after 1957, the obkom acquired the dominant position in industrial decision-making in the oblast." (p. 119) From this general conclusion he deduced that "from the very start [1957] the bureau of the Stalingrad obkom assumed the dominant position vis a vis the Sovnarkhoz [the newly created decentralized organ for the control and operation of industry]." (p. 113) But the hard data which the author has assembled seems to belie this conclusion. In fact, the oblast party leaders were unable or unwilling to assume the kind of close direction over industry being decreed from Moscow.

For example, the author reports that after 1957, on the one hand, only a few party officials (not including the oblast's first secretary) and some relatively unimportant government administrators took part in the discussions on industrial questions before the meetings of the oblast party committee. On the other hand, the leading party cadres took part in discussing agricultural issues. (pp. 72, 82) Secondly, "the competence of the obkom bureau over the appointment and removal of industrial cadres is much less inclusive than its authority over agricultural personnel." (p. 123) It took two years, for example, for the city party organization to remove an industrial manager "over the protests" of the sovernakoz vice-chairmen and then only on appeal to the obkom bureau. Third, none of the obkom secretaries were formal members of the ruling body of the sovnarkhoz which controlled industry, although they may have participated in its meetings. Fourth, the evidence on the type of education and experience of party cadres makes it doubtful they have had the technical competence and prestige to supervise industry. Although many of the oblast secretaries began their careers as engineers or foremen in factories, they shifted over to professional party work well before the age of forty. (p. 153) The same is true of other party cadres such as the heads of party departments which oversee the economy. To "the

extent that Stalingrad is a typical case, then, it is rare that persons who have gained their experience outside the party apparat are appointed to head an obkom otdel." (p. 191) Furthermore, most of the higher education of party cadres has been completed in higher party schools where the major emphasis has been and continues to be political work. (p. 186)

Thus the evidence indicates that the party at least in the Stalingrad oblast avoided and/or were unable to take the responsibility for directing industry as decreed by the 1957 reform and that the industrial leaders of the new sovnarkhozes seem to have resisted such interference. If this were typical of other oblasts, this may have been the reason Khrushchev bifurcated the party, in order to place responsibility for industry directly and unambiguously on one group of the party at the local levels. Of course, with only this one case, such an explanation for splitting the party is only a hypothesis, but even this single case seems to show the rigidity which has developed over the years in the Soviet bureaucracy capable of defying decrees and reorganizations.

Although others may draw different conclusions from the materials, it is, nevertheless, the hard data which Professor Stewart has so prodigiously and carefully analyzed which makes it possible to hypothesize beyond mere speculation on some of the puzzling aspects of Soviet society.—DAVID T. CATTELL, *University of California, Los Angeles.*

Rebels and Whips: Dissension, Discipline and Cohesion in British Political Parties since 1945. BY ROBERT J. JACKSON. (New York: St. Martin's Press Inc., 1968. Pp. 346. $10.00.)

Most students of the House of Commons seem to have accepted the decline and fall of the 'independent' MP and the diminishing freedom of action accorded to independently-minded members of the two major parties as inevitable, given the growth of the mass party. The rigidity of party relations and the apparent permanence of party cohesion is accepted and applauded. Yet there has been little real empirical work seeking to ascertain the nature of this cohesion within the Labour and Conservative parties, and what preserves and maintains such cohesion.

It is the prime merit of Professor Jackson's book that he amply confirms that the nature of party cohesion is exceedingly complex, that it varies between the parties and is affected by a myriad of factors which include time, the size of the party majority or the length of time it has been in or out of power, individual and group attitudes towards party policies, and the nature of constituency influences on individual MPs. Jackson seeks to explain 'the dynamic relationships between dissension, discipline and cohesion by a detailed examination of the attitudes, beliefs and political behaviour of MPs during the years 1945 to 1964 (p. xi). Much of the study is devoted to a detailed and slightly repetitive analysis of party rebellions in this period, demonstrating that cohesion was often less (and rebellion greater) than is often believed, and that punitive action against rebellious backbenchers has been infrequent and of limited effect.

The study is, however, not merely concerned with rebellion. Jackson, in a fascinating analysis of leader-follower relations, shows that the Conservative party leadership relies more heavily on offering positive inducements to loyalty rather than imposing punishments, indirectly indicating certain basic cultural differences between the Labour and Conservative parties. The Conservatives seem more likely to use patronage as a sop, more likely to reward party loyalty, and prefer to leave or persuade members to resign rather than withdrawing the whip or expelling individuals from the party. Party loyalty is maintained through cultural rather than legal norms, as befits a party of tendencies rather than factions. The more diverse and factionalised nature of the Labour party has made it necessary to seek to maintain discipline, especially on non-sponsored members when the party is in office, though Jackson indicates that the PLP Standing Orders and disciplinary procedures are often as much an embarrassment as an advantage to party leaders. The fact that the Labour party began at the constituency level and developed into a parliamentary party, while the Conservative party began in Parliament and developed its mass party organisation from this, is reflected in the evidence that most Labour MPs that rebel have the support of their constituencies whereas constituency organisations in the Conservative party tend to be more loyal to the party leadership than to their individual member, especially when the party is in office.

Jackson's empirical work is careful and concise, and there is a minimum of factual error, but the concluding chapter is disappointing. He is content to demonstrate that rebellions and discipline are 'but part of the flow of information' (p. 309) in the process of adjusting party policies and is unwilling to discuss the implications revealed by his study. It indicates, for example, that party discipline is as much self-imposed by MPs as imposed by party leaders, notably because of the mutual desire to maintain party unity while allowing for party change. A useful, though ignored, contrast to this is the spectacle of the Liberal Parliamentary party which, while acting as a cohesive group has no formal party procedures to help enforce such cohesion and their party whip has no disciplinary role.

The adversary nature of Parliamentary activity

plus the cautiousness and ambition of many MPs to stay in Parliament and become Cabinet or Shadow Cabinet members, means that few MPs feel able to afford the luxury of continued dissent, and those who do often avoid isolated action. So, elaborate games of bluff and counter-bluff can mark the nature of the relationship between the whips and the party backbenchers, the nature of this depending upon whether the party is in or out of office. MPs revolt, but rarely to the point, or at a time, where they damage seriously the image of party unity, while party leaders threaten discipline but are reluctant to impose it even in the face of pressure from party loyalists.

Jackson's study indicates that as rebellions are often over policies, and excepting the Labour party in 1951–55 and 1959–61, both parties experienced more rebellions when in office than in opposition, most Labour backbench revolts being over foreign affairs and defense and the majority of Conservative rebellions over domestic policies. He amply confirms that major British parties are cohesive not merely because of the political culture, the structure of the political system and the growth of centralised mass parties, but also because of the need for an adequate adjustment or accommodation process within the parties to allow for policy changes without destroying party unity. This process is accepted by MPs as well as party leaders, though it can be subject to severe strains, especially when the morale of the governing party is low and there is backbench dissent over policies.

It is unfortunate that a fear of criticism for a lack of political science perspective or the 'feel' of British politics should have inhibited Jackson from fully exploiting the potential of his extensive interviews and thorough analysis and at least developing a few hypotheses about the internal nature of the Conservative and Labour parties and the implications for political recruitment in general and leadership recruitment in particular. The study is, however, of great value in defining the real ground on which questions of party cohesion and discipline might be considered, and includes an extensive bibliography. The period of Labour government since 1964 has provided a series of intra-party revolts and disciplinary dilemmas which confirm the complexity and intricacies of this important aspect of party activity.—JOHN D. LEES, *University of Keele, England.*

Patterns of Conflict in Colombia. BY JAMES L. PAYNE. (New Haven: Yale University Press, 1968. Pp. 358. $10.00.)

Considering the improving but generally parochial and unsystematic writing in the field of Latin American politics, Payne's analysis of political conflict in Colombia is a most welcome addition to the literature. It is cast systematically within a theoretical framework, prodigiously researched, comparative not only in terms of the Colombian data but cross-national as well, and skillfully utilizes historical materials to test hypotheses. While extremely provocative, the study is frustrating, nonetheless, because of both theoretical and methodological shortcomings.

In studying the turbulent, strife-ridden politics of Colombia, the author starts from the assumption that we may have assumed wrongly that Latin American *políticos* seek ideas or program rewards. Instead, he suggests they are motivated principally by a drive for status, defined as the prestige participants seek from holding public office, and not by the making or execution of policy. Using the motivational variables of status and program, the author formulates what he calls an incentive theory of politics. Conceptualizing status and program incentives as polar opposites on a continuum, along with the strength of the incentive, a model is presented that permits the comparison of political systems according to the motivational orientations of political leaders. Colombia is characterized by a strong status incentive, Antioquia—a Colombian department—by a weaker status incentive, and the United States by a moderately weak program incentive. To test the theory, a series of thirty-four hypotheses are formulated about incentives in the Colombian political system (Part I), structures in the political environment—the military, political parties, and political competition (Part II), and the consequences of high status motivation of leaders for political parties, Congress and the Executive (Part III). The data employed for testing the hypotheses consist of structured interviews with top-level politicians (130), extended interviews (about 50), extensive historical materials, and aggregate census data. Field research for the study was executed between February and September, 1965.

The basic hypothesis of the study, upon which its validity rests, is that "Status will be an increasingly strong incentive for leadership participation as (1) status-consciousness is higher in the society, (2) the status value of higher political offices is greater, and (3) recruitment to these offices is more open." (p. 28) The first and second conditions appear applicable to Colombia, although no convincing empirical data are offered, but difficulties arise in connection with the openness of political recruitment. Payne maintains that the third condition applies, noting that Colombia is *not* characterized by particularly low social mobility, which is a highly debatable point, as is his assertion that it is unprofitable and misleading to speak of social classes in Colombia, because there appears to be no felt sentiment of class-consciousness. (p. 45) To show the openness of political recruitment, the author discusses low status back-

grounds of prominent politicians, which I consider exceptional cases, the ambiguous results of a 1936 occupational census of political leaders, and the findings of his survey. The postulated high incidence of social mobility among upper-level politicians is an unwarranted conclusion because social mobility is measured in terms of occupational status. Assuming that a respondent is an ambassador and his father a journalist (+9 status unit change), it does not follow that the hypothetical ambassador displays high social mobility. How open is political recruitment if the father were an aristocratic *hacendado?*

After arguing that upper-level politicians are motivated primarily by a drive for status and active followers by the search for government employment, Payne turns to some critical structural features of the system. Chapters six and seven focus upon the relation between the military, elections, and political parties in Colombia, Ecuador, and Perú. To simplify the argument, military *coups* stem from civil opposition to the president, with the degree of civilian support for the president—conceptualized as the extent of party identification in the society—representing the key intervening variable. In those political systems where party identification is high, as in Colombia, military intervention is less likely than in countries with low party identification, such as Ecuador and Perú. In chapter eight, the author explains the intense political conflict of Colombia in terms of a series of political factors, which he labels a defensive feud—"a struggle between two groups in which each side recognizes the existence of a mutually acceptable solution less costly than fighting, but that solution cannot be reached." (p. 161) Such a situation prevailed in Colombia before the *Frente Nacional* as a result of strong incentives for political participation, the presence of two political parties, high party identification, decentralized parties, partisan communication channels, and a military not inclined to intervene, Payne praises the alterations instituted under the Frente Nacional for stemming political violence and predicts a "genuine crisis" when the Frente expires in 1974.

Within an environment marked by high status incentive, relatively open political recruitment, and high party identification, the author analyzes the behavior of political groups in the last section of the book. Colombian parties exhibit the following behavioral patterns: keen competition for office, high factionalism, allocation of leadership positions on the basis of electoral strength, cohesive factions on the national level except in elections, and conflict centering around the distribution of political offices, not ideological differences. Turning to the Colombian Congress, turnover is high and legislative behavior is characterized by high absenteeism, low committee workloads, disruptive, conflict-provoking patterns of behavior, and popular-demagogic orientation toward policy. Presidential behavior is marked by impartiality in distributing political offices to factions supporting the government, procrastination in decision-making until the factions can be appeased, avoidance of dramatic commitments to particular policies, and sensitivity to popular demands. While capturing the style of former President Valencia, none of these patterns appears relevant in explaining the behavior of Carlos Lleras Restrepo.

Several conclusions presented in this book are difficult to accept, not only because of dubious methodological procedures but also because of what I consider inadequacies of incentive theory. The use of incentives as independent variables in predicting political behavior is hazardous and begs the question as to why those incentives prevail. Any study that purports to explain questions raised by the author without considering key structural variables of the system, such as the distribution and exercise of political power, the degree and type of economic development, and the extent of foreign economic and political influence, lends itself to faulty interpretations. To demonstrate the point, Payne compares Colombian congressmen, whose principal motivation is status, with United States legislators, whose major incentive is program, and finds that their behavior differs immensely, concluding that incentives represent the crucial explanatory variable. But without placing the Colombian Congress within the context of political decision-making, assuming that the institutional context in Colombia is the same as in the United States, more satisfactory explanations may be overlooked. Structural features of the Colombian political system such as the predominance of the executive in decision-making and the generally subservient role of Congress are not seriously considered. The author attempts to substantiate his interpretation of congressional behavior by predicting, and finding, a lower rate of legislative turn-over in the deviant department of Antioquia, but my calculations suggest that this conclusion is erroneous for the Senate and the difference is minimal for the House.

While present-day predilections of political science thrust interview materials to the foreground, this study would have been strengthened had the upper-level survey been omitted, for shaky, even misleading, conclusions are derived from the data. To say, for example, that no ideological differences separate Colombian party leaders or that no reactionaries are to be found in the Congress on the basis of six poorly constructed questionnaire items is not very conclusive evidence. Even taking into account the above criticisms, along with scat-

tered factual errors, this book warrants careful study.—GARY HOSKIN, *State University of New York at Buffalo.*

The Hero and the Crowd in a Colonial Polity. By A. W. SINGHAM. (New Haven: Yale University Press, 1968. Pp. 389. $10.00.)

Claiming that "a sufficient body of knowledge has accumulated in some areas to begin to enable at least some tentative generalizations on the nature of political systems in the Third World" (p. 19), A. W. Singham, senior lecturer in government at the University of the West Indies has embarked upon a comprehensive and extremely well-developed study of the British possession of Grenada, a small Windward Island. From a detailed analysis of the political system of this colony from 1951 to 1962, the time intended to be that island's terminal period as a colony, Mr. Singham has guardedly but seemingly accurately extracted principles which may be applied in some instances to British Caribbean possessions, in others to all British colonies, and in still others, to the colonial system in general.

A number of interesting basic conclusions are drawn by Mr. Singham. Leaders of the emerging nations are "socialized" during the colonial period, and as a result, are "often incapable of making the necessary adjustments to achieve the difficult task of decolonization" (pp. 9–10). They remain unaware of the deep economic, social, and even personality changes which must be undergone, and "the difficulties and pain involved in genuine decolonization usually lead the colonial politician to an attitude of 'it's my turn now'" (p. 10).

Despite claims to the contrary, the authoritarian attitude of the colonial government is shared by the land's emerging native political leaders. They endeavor "to impose change from the top rather than through a process of genuine political socialization" (p. 10). These "elite" members of society become trapped between the traditional society and the colonial leadership and seek identification with the latter. Since they cannot determine whether they have been accepted by the rulers, and lacking the opportunity to be innovative, they often return to the traditional culture of the area once independence has been achieved.

Since Mr. Singham ably demonstrates that no traditional culture exists in the West Indies, he notes that these societies "still face the difficult task of creating a value system that will free them from the stigma of inferiority and will release the creative energies of their people" (p. 11). His study examines the difficulties created by this task, "particularly the role of the colonial political system and its effects on the leaders socialized in this tradition" (p. 12).

Political systems in a colony approaching independence are inevitably unstable, and "a crisis such as that experienced by Grenada is not an isolated phenomenon" (p. vii). Competition for legitimacy among, as Max Weber termed them, the traditional charismatic, and legal domination systems of authority, leads to unsolvable "contradictions." This study considers the relationships between these systems "particularly . . . between the charismatic and legal authority systems in Grenada" (p. 319).

Tremendous rivalry exists between the legal authority or governor who represents the imperial power and the charismatic or native leader who, rising from the masses, seeks the legitimacy claimed by the governor. Complications arise when the charismatic leader is also opposed by the bureaucracy since he poses a threat to the security which they have gained through their position as elites or mid-elites in society.

In the West Indies this picture is further complicated by the existence of intricate constitutional provisions, establishing among other institutions, complex ministerial systems which, instead of unifying the area as was hoped, actually "only further divided the islands and hampered cooperation" (p. 13). These systems place the native leader in a position of direct competition with the governor since the legislature becomes elective, suffrage is often universally granted, and the charismatic leader, or hero, invariably is chosen prime minister.

Through the manipulation of crisis situations, the hero maintains his power, for "the essential link between the hero and the crowd is the former's ability to mesmerize crowds and canalize their emotions for short periods of time" (p. 192). Thus, as politics "routinize" the hero loses power and it is to his advantage to maintain a limited crisis atmosphere.

Having considered these and similar concepts, Mr. Singham proceeds to skillfully dissect the political life of Grenada, a few highlights of which follow. In his examination of the insular economy he notes that it is agricultural with little industrial potential. Nevertheless, he indicates mixed feelings toward tourism. The economy is dependent and the island has been under the grant-in-aid system. A point worthy of note is that many farmers are agro-proletarians, who work for wages and also own small plots. As wage laborers they are more susceptible to labor organization and this makes their political organization easier for the hero.

The author also considers the social structure, attempting to "isolate the critical economic, social, and psychological variables that affect the political culture of a colonial society . . ." (p. 98). An ex-

amination of the constitutional order follows. Mr. Singham discusses changes which have occurred since the days of settlement, through the Old Representative System and its collapse, and finally through the establishment of the Crown Colony.

In his survey of recent constitutional and legal developments, the author emphasizes those points which led to conflict between the hero and governor (later the administrator). The 1959 constitution is examined in detail "since it created many of the preconditions for the breakdown of constitutional government in 1962" (p. 144).

The rise of the hero, Eric Matthew Gairy, is closely analyzed. Gairy emerged from the crowd due to the grant of universal suffrage and the crisis situation of 1951. However, subsequent to gaining leadership he "began to make quite reckless demands and to act as though he was above the law" (p. 172). He made little effort to organize urban workers and relied mainly on his personal appeal. This failure to develop an effective political organization greatly hurt his cause.

In 1957 Mr. Gairy was disenfranchized "and until 1961 remained outside the formal corridors of government power" (p. 177). When his franchise was restored he was proclaimed Chief Minister which placed him in direct confrontation with the administrator, James Monteith Lloyd.

Gairy's conflict with Lloyd, the bureaucracy, and the opposition is subsequently brilliantly considered. Charges filed by a Commission of Enquiry led to the suspension of the Gairy government by Mr. Lloyd in June, 1962. In the following election Gairy was defeated.

In this extremely fascinating study, Mr. Singham completely justifies his opening contentions. His study is logical, impartial, complete, and extremely well-documented. Particularly pleasing was the methodology employed which included in addition to extensive documentation, travel to the island, wide-spread interviewing, and the use of participant-observers.

Mr. Singham's repeated references to other studies undertaken in this and related fields and his at times lengthy comments as to their value and shortcomings is of immense value. Other notable features include excellent statistical charts, a section devoted to a description of the methodology employed, appendices containing questionnaires utilized, a bibliography, and a comprehensive index.

This book must be considered an outstanding contribution to the field and deserves attentive study by all students of Caribbean and colonial affairs.—FRANK PAUL LE VENESS, *St. John's University, New York.*

Political Communication: The Public Language of Political Elites in India and the United States. By SATISH K. ARORA AND HAROLD D. LASSWELL. (New York: Holt, Rinehart and Winston, Inc., 1969. Pp. 312. $6.95.)

If one looks occasionally (say, twice a month) at the front-page stories that have appeared over several years in the New York *Times* and the *Times of India,* paying particular attention to the quoted or paraphrased statements of political leaders, what can he learn about the articulate generalizations that characterize the American and Indian polities? The answer, according to the rather informal presentation of content-analytic findings by Arora and Lasswell, is apparently "Quite a bit." By culling and arranging quotations, and also interpolating fairly sizeable amounts of commentary, the authors have been able to construct interesting analyses of the inner and outer political arenas of the two countries.

More than two-thirds of the book is devoted to this sort of allusive recounting. (In addition, there is an introductory chapter that places the study within the Lasswellian framework of seven decision phases, eight value demands, and so on; a concluding chapter that refers to tabular arrays and discusses trends and possible developments; and two appendices, one statistical and the other methodological. I will focus on this appended material below.) One sees how the Indian and American elites each thematically view their own country, and then how they grapple with the various challenges furnished by the international environment.

Except for an occasional glance at the Indian situation in the American chapters, the portrayals of the two nations do not interpenetrate, nor are they formally parallel in detail. Not only are there only six chapters on India to eight on the United States, but their scopes, singly and together, often differ markedly. In a way such presentational divergence simply mirrors the substantive differences in the two countries' experience. They had Goa and we had (Joe) McCarthy, and not the other way around. But to say this is also to say that in general the book is no more truly comparative than most descriptions of two or more national systems.

The data base extends from January 1, 1950 to June 15, 1958. All front-page stories on the first and fifteenth of the month fall into the information sample analyzed. According to the authors, "The procedure was to note all national speakers (Indians in India, Americans in the United States and to use quoted statements or paraphrases). . . . Each sentence quoted (or paraphrased) is classified according to 'who' said it and 'what' was said" (p. 287). The basic unit, then, would appear to be individual sentences within the front-page communications (speeches, announcements, reports, etc.) of political leaders. I say "appear" because the

methodological appendix is so terse and devoid of coding examples that the actual procedures remain uncertain. To illustrate, over the eight-and-a-half years some 408 front pages (17×2×12) would have required examining. The sample of statements, however, totals only 2863—1446 from India, 1417 from the United States—or about seven for each entire front page. My initial impression, underscored by a subsequent perusal of a few issues of the New York *Times*, is that seven is rather low, particularly since "The 'front page' means the stories that appeared on page 1 *plus* the continuation of the stories inside the issue" (italics in original).

Apparently, quite a few remarks have been deliberately left unrecorded. One can perhaps guess correctly about some of the criteria for omission. For example, speakers who were not *prima facie* political were probably ignored. This would mean dropping statements not only by socialites and ballplayers, but also by most businessmen. Of the 143 Americans whose remarks are coded, only four (Bernard Baruch, John D. Biggers, G. G. Gurley, and John A. Stephens) are from business. Stephens is not directly quoted in the text, and Baruch is scarcely typical of the calling. Biggers, though, did advocate high tariffs, while it was Gurley, "president of an oil company, who wrote to Senator William Fulbright: 'We know there is some crackpot fronting for the Jews and we think it is you'" (p. 78). It is difficult to accept these few citations as adequately signifying the newsworthy political communications of American business.

A second criterion for exclusion would seem to be the speaker's lacking nationwide political scope. That would account for the absence of New York City's mayor—indeed, of the entire city government—and of all state legislators. While a few state governors (Byrnes, Dewey, Faubus, and three others) are included, they clearly have played important national roles. Presumably in India state officials more regularly act at this higher level, since the list of 84 Indian speakers includes three state governors, seven state cabinet officials, and five state legislators. What then are we to do with a reference like this: "Dr. William Jansen, superintendent of the New York schools, for instance, suspended teachers without pay for not answering questions that enquired into their political beliefs" (p. 80)? Perhaps the authors deem this an act of national significance, but it is hard to know for sure.

A third exclusionary possibility is that some convenient statement length other than the (grammatical) sentence is sometimes the unit analyzed. This might especially hold for paragraphs where later sentences substantially repeat earlier ones. Similarly, the rules for accepting an account as paraphrase might be more stringent than one would imagine. It also appears certain—though we are nowhere explicitly told this either—that all quotations attributed to unidentified "spokesmen" or "observers" are systematically eliminated.

Some readers might be disturbed that more than 55 percent of the statements from India derive from just one speaker, Prime Minister Nehru. Such a peaking of concentration in so fragmented a country, however, is almost surely a crucial political fact and not a mere investigative artifact. What disturbs me is that it is so difficult to know just what this study is a study of. Far more detailed appendices are clearly called for, ones perhaps that would exemplify the way in which a couple of front pages were actually coded. And yet, while a complaint of this sort is justified, it seems so strange, given all that the political science profession owes methodologically to the book's second author.—MORRIS DAVIS, *University of Illinois*.

Politics and Beef in Argentina: Patterns of Conflict and Change. By PETER H. SMITH. (New York: Columbia University Press, 1969. Pp. 292. $10.00.)

This volume is one in a series sponsored by the Institute of Latin American Studies of Columbia University; and it is one of a small but significant group of studies by North Americans on Argentine rural matters, treated in the light of their important urban relationships—all of them viewing the development of Argentina as a modernizing process.

Three decades ago the first such study appeared —Simon G. Hanson's *Argentine Meat and the British Market: Chapters in the History of the Argentine Meat Industry* (1938)—, which carried the story of the livestock and meat packing industries through the post-World War I depression period.

Two decades ago Carl C. Taylor's *Rural Life In Argentina* (1948) appeared, the standard treatise on the subject.

Most recently, James R. Scobie's two works have thrown much light on this area. His general history, *Argentina: A City and A Nation* (1964), is especially strong in analyzing the rural economy and society, without neglecting the city.

His *Revolution on the Pampas: A Social History of Argentine Wheat, 1860–1910* (1964) ably depicts the unbalanced growth of "urban sophistication" and "rural primitivism"—of a polity too long neglecting human and natural resources under the rule of a cattle-baron elite.

In Smith's book, the Introduction sets the stage with distinctions about Argentine political, economic, and social power; and seeks to locate the

origins and development of group conflicts and group behavior.

Chapters 1 and 2 outline the process of modernization, from traditional society through economic liberalism to a lopsided economic modernization and political centralization, with analysis of the participant groups and their particular ideologies of "antiimperialism."

The subsequent chapters consider the political and other responses of these groups to successive economic changes, with emphasis on behavior in the emergence and resolution of conflicts.

Smith's work is a treatise on economic history, but it is much more than that. As an analysis of the country's leading economic enterprise, it is also a case study in national development, in which the economic and the political are profoundly interrelated.

If we view the subject from the standpoint of the distribution of power, we can distinguish certain broad stages in its development. While its roots were established in the last third of the nineteenth century, we can view the first third of the twentieth century as the era dominated economically, socially, and politically by a rural-based, stock-raising oligarchy. Economic power largely coincided with political power, with superiority in the first leading to superiority in the second. The stockmen were divided into the stock breeders and the fatteners, with a shifting relative influence (economic and political) between the two. In close relationship to the two were the packing house-shipper firms, whose influence was largely economic.

During a second stage of development (the 1930's), the distribution of power shifts within the rural upper class between breeders and fatteners; but the class as a whole retained its overwhelming influence, with any serious challenge from workers and consumers still well in the future. During this and the preceding period tactical shifts included showy debates, congressional investigations, press campaigns, and moves toward government regulation of the packinghouse firms.

During the third phase, after 1944, the rural-based monopoly of power was replaced by the Peronist rule, which exalted the position of the masses and deflated the role of the stockmen.

Smith's analysis shows a long process of transition from a traditional to a 'populist' (Peronist) society, in which the urban middle classes long identified their aspirations and interests with those of the ruling groups. During this time socio-economic modernization outran political modernization until, under Perón, "the lower-class groups were able to offset economic weakness with political strength."

Of considerable interest is the author's analysis of Argentina's vigorous economic nationalism (anti-imperialism). The earlier (or 'traditional') anti-imperialism rejected the validity of modernization on the ground that it meant adopting alien, materialistic values (see Rodó's *Ariel*). The country "needed to recover the spiritual purity of its rural past," as symbolized by the *gaucho*, etc. Thus, an anti-economic doctrine gave support to a 'traditional' economy, and an ideological rift was revealed in the upper class, with some adopting a liberal belief in progress.

More recently, a 'modern' form of nationalism became dominant. It condemned foreign 'imperialism,' and endorsed material progress through national economic self-sufficiency (protected native industries, etc.).

This able study is well organized and well written and includes a good index and a good bibliography. The author has made effective use of Argentinian and foreign documentary and other sources.—WILLIAM P. TUCKER, *Texas Technological College*.

Korea: Democracy on Trial. By JOHN KIE-CHIANG OH. (Ithaca: Cornell University Press, 1968. Pp. 241. $7.95.)

In this book Professor Oh has presented the most thorough recent political analysis relative to the politics of democracy in South Korea. His central theme is that for the past two decades South Korea has been experimenting with a Western-type democracy but its prospect is still uncertain.

According to the author, the failure to realize the substance of democracy in Korea is due to the fact that the Korean polity neither possesses the political culture nor the ecological basis to make a Western-type democracy successful there (p. 191). Moreover, ignoring the country's political realities, the early political leaders—particularly Dr. Syngman Rhee and his close associates—were not only determined to create a democratic political system, but consistently maintained the myth of Korea becoming a "democratic state" (pp. 191–94). The United States, too, since 1945 has encouraged the democratization of South Korea, while the Soviet Union has established a Communist North Korea. Consequently, Professor Oh states that the original constitution imposed upon South Korea the "legal-institutional superstructure . . . which . . . appeared democratic and modern," but it could not function well because it was in conflict with the traditional political culture that ". . . had been long accustomed to debilitating authoritarian rule" (p. 191).

Unfortunately, the author does not explain fully the essence of the Korean political tradition or culture; as a result, the reader is left unclear as to how such tradition has prevented the successful

implementation of a "democratic" constitution. The present reviewer also feels that Professor Oh does not consider systematically the relationship between successful nation-building (or democracy) and the conditions and/or circumstances which must be reckoned with in nation-building (or democracy). It may well be that such a treatise is beyond the scope of the book, since the author has taken a deductive approach in dealing with the recent Korean political events.

In essence, the problem of "democracy" in Korea is two-fold. First, the Korean people—more particularly the leading politicians—have never arrived at a consensus about the basic structure and purposes of their political system. They had to build an entirely new polity out of both the Japanese colonization and the American military occupation, plus internal subversion and external Communist aggression. Altogether the country went through five major constitutional revisions within a period of fourteen years. During the First Republic the original cabinet form of government established in 1948 was changed to a presidential system in 1952, which came to an abrupt end in 1960 as a consequence of the Student Uprising. During the Second Republic, the governmental system returned to a cabinet form, but the Chang regime which headed it lasted less than nine months. The military regime which overthrew the Second Republic then restored once again a presidential system in 1962 and thus set the stage for the Third Republic coming into being the following year.

Second, the Korean people—particularly among the power holders—have not shown their determination to maintain and nourish the governmental system once it was established. Dr. Rhee first established this "undesirable" precedent. For example, he changed the constitution in 1952 to assume the total control of the Republic (with a presidential system) and then in 1954, among other things, to make himself a president for life (pp. 40–50). When the Rhee regime was overthrown by the students, the first thing the opposition did was to rewrite the constitution and to establish a cabinet system. The military once again rewrote the constitution in order to reestablish a presidential system. Thus far, President Park's "administrative democracy" has performed reasonably well to meet the developmental tasks of the country; but there is no assurance that the present regime will not temper with the constitution. In fact, in early 1969 some members of General Park's Democratic Republican Party openly talked about the necessity of amending the constitution to permit the General to run for a third four-year term in 1971. Here President Park can make a great contribution to the country's political stability, if he should allow for a peaceful transition of authority by dissuading his supporters from changing the constitution.

One major contribution Professor Oh makes in this book is that he has demonstrated with empirical data on Korean politics that there is a great deal of difference between the writing of a constitution and constitutionalism. In Korea the formal constitutional document became subject to major revisions whenever there was a change of regime. While the power holders paid lip service to the "sacred" constitution, at best it was a temporary document and the regime in power felt free to change it for the regime's own purposes. The country must yet develop constitutionalism—the permanent rules which not only impose limitations on public decision-making but also establish the procedures for exercising the political power granted to the government. Yet such constitutionalism cannot be developed in a political vacuum. The Korean experiences demonstrate the futility of adopting the best features of other constitutions to a strange environment. No constitution can function effectively if it does not reflect the political tradition or forces of that political community.

The author has considerable insights into the workings of Korean politics. The book is well researched and documented with English, Japanese and Korean sources. It makes a worthwhile contribution to the field of comparative politics.—JOHN C. H. OH, *Wisconsin State University, Whitewater.*

Towards the Integration of Indian States, 1919–1947. By URMILA PHADNIS. (New York: Asia Publishing House, 1968. Pp. 297. $6.50.)

Mrs. Phadnis has written a useful historical background to the integration of the princely states into the Indian Union. The work is essentially a history of relations between the states and the Government of India during the crucial period between the Montagu-Chelmsform reforms of 1919 and the termination of British rule in 1947.

The work can be divided into two broad sections—both chronologically and substantively. The first part (pp. 1–97) deals with the expansion of British control over the quasi-autonomous states through a liberal definition of the concept of paramountcy and with the concommitant attempts of the princes to preserve as much independence as possible. The imperial authority sought to keep "paramountcy" vaguely and broadly defined while the rulers of the states wanted a clearer and narrower definition.

The period between 1919 and 1935 was characterized by greater institutionalized interaction among the princes through the Chamber of Princes, but this did not result in significantly greater leverage *vis a vis* the paramount power. By the end of this period, the princes were con-

fronted with the problems posed by constitutional and political developments in British India as well as the growth of political consciousness in their own territories.

The second part of the book deals with the princes reaction to policies and events after the passage of the Government of India Act of 1935. The reaction of the princes to the idea of federation was unrealistic and ambivalent. Before passage of the 1935 Act the princes wanted only the weakest sort of federation, presumably because of their desire to preserve their autonomy *vis a vis* the Centre and to isolate their populations from British Indian nationalist politics. After the reforms were instituted, the states prevented the establishment of a federal center. They did, however, accept the goal of federation and the fact that they could not remain aloof from constitutional developments in British India.

The centripetal effects of India's role in World War II brought the states even closer to British India. This was particularly true of the smaller states, which lost much of their distinctness through cooperative schemes and attachment to larger political entities. After the War, it became clear that much closer integration with British India would be necessary. Although some princes tried to preserve their internal autocracy and a few even sought independent status, the prospect of the British withdrawal underscored the princes' dependence on the paramount power. Given their reluctance to cooperate with the nationalist forces in British India after 1935 and their failure to undertake social and political reforms, it is remarkable that the princes fared so well after 1947.

Although the author's treatment is chronological and not thematic, several broad themes emerge in each chapter. One is the artificial character of the states and the fiction of their internal autonomy in relation to the imperial power. The second is that despite the repeated attempts of various princes to preserve or enhance their quasi-independent status, they recognized the strength of the forces of centralization and integration. Thirdly, the princes were deeply divided among themselves and thus unable to present a united front in the face of the manifold threats to their existence. There were divisions between the larger states and those of middle rank as well as divisions within categories. This was best indicated by their different approaches to administrative and political reform and to the basic issues of federation and integration.

The chief shortcomings of this work are that the author relies far too heavily on a historical narrative of formal relations and on documentary evidence (records of conferences, petitions, etc.). Although Mrs. Phadnis assembled an impressive array of documentary evidence, she neglected the informal and possibly more significant aspects of the relations between the states and British India. The author also has not given adequate attention to the internal politics of the states and to the "popular movements" in some areas. Except for references to Congress resolutions and speeches by few nationalist leaders, the author ignores the general question of nationalist attitudes. These shortcomings make it less valuable as a true background to the integration of the states.

After reading Mrs. Phadnis' work, one is still very puzzled regarding just how the states were brought into the Indian republic and stripped of their autonomy so easily. One gets the impression of sharp changes in policies and attitudes with little explanation of how the shifts occurred. In general the quality of the work suffers from the lack of an explicit conceptualization or central theme.

Despite these criticisms Mrs. Phadnis has provided a very useful reference work in a much neglected area of Indian political development.—HAMPTON T. DAVEY, *University of Missouri—St. Louis.*

Yugoslavia: The New Class in Crisis. By NENAD D. POPOVIC. (Syracuse: Syracuse University Press, 1968. Pp. 240. $7.00.)

This analysis by Popovic of the "new class" phenomenon in Yugoslav society elicits an understandably mixed response from the reader. The book, on the one hand, is filled with specific and rather useful insights about Yugoslav politics based upon the experience of a once highly-placed diplomat now taking refuge in the West; whereas, on the other hand, it represents a subdued and yet angry polemic that at times mis-states recent Yugoslav developments with arguments couched in the acceptable phrases of contemporary elite theory. Perhaps the single most annoying feature of Popovic's analysis is precisely this attempt to give his polemical treatment of Yugoslav politics an undeserved legitimacy by the usage of social science terminology. From a social scientist's perspective this effort represents an unfortunate abuse of what has become a productive and widely-accepted theory of political elites.

For all intents and purposes the book is divided into three easily recognizable parts: the first four chapters develop the theme of the "new class" as the revitalized form that privileged, dictatorial rule has been taking in Yugoslavia; the next two chapters deal with foreign policy and administrative structure and, it might be noted, are rather awkwardly placed in the middle of the book; and, the last four chapters focus upon developing "economic nationalism" in Yugoslavia, the nature of the new economic reforms, and the implication of these reforms for current Yugoslav politics. This last set of chapters seem the most pertinent and

respectable of the lot and in fact provide some useful insights and intricate analyses of recent internal political haggling and controversies that stem from the new Yugoslav reforms. The single overriding theme that Popovic tries to develop is that the Yugoslav reforms over the years, the presumed liberalization of the country, the trends toward internal relaxation, etc. simply represent the range of subtle instruments the "new class" has employed to consolidate its elite position of power and privilege. A secondary, less explicit but no less relevant, theme stresses the fact that the Communist party in Yugoslavia has been losing ground over the years to "new class" elements, a development which the author for some mysterious, but perhaps humanly understandable, reason quite clearly and sorely laments.

There are also a number of specific points that require somewhat more extensive and more focused comment. At the very outset of the volume Popovic writes about the "many-cornered civil struggle between Communists and Nationalists, as well as among the embittered Yugoslav nationalities," that took place in Yugoslavia (p. 1). The author, however, does not explain that the struggle involved the two distinct resistance forces of Tito's partisans with a broadly Yugoslav appeal and Mihailoviches Cetniks with a narrower Serbian base, which it seems would be a far more accurate way of describing the distinction that Popovic arbitrarily characterizes as "Communists" and "Nationalists." In short, Popovic apparently tries to exploit the lack of detailed knowledge by the Western public regarding World War II Yugoslav resistance forces by characterizing the one group as "Communists" (and, hence, conspiratorial, illegitimate, and perhaps "evil") and the other as "Nationalists" (and, hence, "Yugoslav," legitimate, and official). It would also seem appropriate to emphasize that Popovic knew full well that such an interpretation (given the terminology that he chose to use) would in fact be forthcoming from the public at large in this country. As a second point, Popovic repeatedly claims that the "new class deprived the Party of its main hold on power, leaving it, weakened and helpless, at the mercy of the ruling stratum" (p. 77). Here we have the expression of what re-occurs several times as the Popovic melancholy lament in relation to the collapse of Communist party strength in Yugoslavia. He does not, however, bother to tell us exactly why it is less desirable for the "new class" element to dominate Yugoslav politics than it was (or is?) for more conventional Communist party forces. Indeed, it is not at all clear why we should not applaud any such development as a result of the expected more materially (and, hence, less ideologically) based motivational structure that the "new class" phenomenon in fact implies. As a third point, Popovic forcefully argues that those Yugoslav innnovations that cluster under the decentralization motif are merely instrumentalities (i.e., they reflect no generic value or sincerity of intent by involved reformers) for the further consolidation of growing "new class" power. Accordingly, the wide range of Yugoslav innovations—that is, self-management (p. 116), rotation of public offices (p. 123), the new 1963 constitution with its multi-chamber parliament (p. 126), the fact that the political role of chairman of the people's committee has been favored over the Party secretariat (p. 130), and the recent thrust toward and debate over internal democratization of the Party (p. 133)—all of these become suspect by Popovic as transparent devices that the "new class" has designed merely to perpetuate its authority at the expense of the wider Yugoslav populace. At this juncture, quite understandably, a credibility gap begins to develop as regards the substantive commentary and indeed the very intent of the author, since it seems quite unlikely that any elite (Communist or otherwise) could act so brilliantly and be as thorough in their manipulation of institutional innovation as Popovic suggests that the Yugoslavs have been. The credibility gap widens into an unbridgeable abyss when the author makes his final major point as regards the manipulative control capability of the "new class" element. In this regard, Popovic argues that the "new class" has indeed been strategically clever (although, surely, morally perverse) in permitting the various nationality or ethnic groups in Yugoslavia to assume their share of the burden in top Yugoslav leadership circles (p. 195). In short, he seems to lament the fact that various ethnic groups in the country are able to enjoy a proportionate power-role in the "new class" structure of Yugoslav society, a fact which he seems to feel unhappily serves to undermine the more traditional lines of ethnic identification as a basis of political power. From another perspective, it can of course be argued that such a balance among nationalities in fact represents an eminently wise (and, indeed, unavoidable) strategy given the internal complexity of the Yugoslav environment. In short, all of the factors that Popovic views as opportunistic devices for the consolidation of "new class" control and exploitation might also be interpreted simply as wise strategies of elite maintenance, in addition to being genuinely positive benefits which serve to make the internal setting of Yugoslavia more acceptable and more palpable to its general populace.

Despite the instructive asides and the many insights of this once practicing Yugoslav diplomat, his clear sense of disenchantment and his own involvement preclude the objective treatment of his chosen subject that certainly would have been

welcomed. Although vividly reflective of the apparent personal animostity that Popovic as a once highly-placed Yugoslav official has, in addition to those specialized and intimate perspectives that go along with this, the book does in fact possess value and should be recommended to the reader if for no reason other than this highly personalized aspect. However, despite the use that Popovic makes of a terminological lexicon that has currency in the social sciences, the book can be justified neither as a respectable bit of social science analysis nor as a balanced and objective piece of contemporary political history. The virtues that the book in fact possesses must be limited very largely to the highly personalized reminiscenses of a once well-regarded Yugoslav official and diplomat. In general, as might be expected, the author was far too much caught up, personally as well as politically, in most of the events that he attempts to describe to allow for the value of his effort to extend beyond this very limited scope.—M. GEORGE ZANINOVICH, *University of Oregon.*

Party Rivalry and Political Change in Taishō Japan. BY PETER DUUS. (Cambridge: Harvard University Press, 1968. Pp. 317. $6.95.)

The Harvard University Press' East Asian Series seems to be well on the way toward a comprehensive, if piecemeal, history of prewar Japan, to which this volume by Peter Duus is a praiseworthy addition. Earlier works in the series have focused upon the establishment of the Meiji Constitution, the career of Hara Kei, and Japanese foreign affairs after World War I; Mr. Duus has devoted his attention to the development of the Japanese party system during the years 1900–1927 and has rendered it lucidly, readably, and interestingly.

Considering the general focus of this *Review,* one should note what Mr. Duus' book is *not:* it is not social science. There is no theoretical focus or concern, and the occasional references made to the social sciences are often needless (p. 17) or meretricious (pp. 238–39). The failure to take account of any of the work of LaPalombara and Weiner, Dahl, Zolberg, or Scalapino, *inter al.,* is most noticeable.

But, then, the book does not purport to be social science; it is history, and enjoyable history. It is well written and it illuminates a period of Japanese history heretofore woefully neglected. The author designates a few important themes and follows them chronologically (with both description and explanation) to a point at which conclusion is logically permissable, whereupon he offers some reflective comments which sum up the previous discussion and provide those familiar with subsequent Japanese history or with developmental politics in general with considerable food for thought. There are typological lapses—the apparent omission of "not" at one point (p. 161, ln. 11) reverses the meaning of a sentence; the rendering of "objection" as "objective" (p. 205, ln. 8) is also misleading—but they do not detract from the substance of the book.

Mr. Duus' three major themes are: (a) what was the process through which the political parties achieved control of the cabinet; (b) why did this process eventuate in a two-party system rather than some other; and (c) once in power, what use did the parties make or not make of this power?

The primary characteristic of the quest for cabinet control is, in Mr. Duus' view, accommodation to the preexisting structure of the Japanese political system: manipulation of and compromise with the various elements in a polity where power was widely diffused. Rather than attempt to reform the structure, the parties were content to play the established game until the *genro,* the Elder Statesmen who constituted the only integrating factor at the apex of the system, died off, thus removing the only group so prestigious as to be able to appoint non-party premiers acceptable to the party politicians who dominated the parliament.

The evolution of the two-party system in Taisho Japan is explained primarily in terms of Japanese political homogeneity and the parties' single-minded pursuit of power. Socioeconomically, both political leaders and the bulk of the electorate were internally quite homogeneous; there was, moreover, general consensus regarding overarching political orientations and national goals (compare this with the newer states of Africa and Asia). With a paucity of acute social cleavages and issue conflicts, political alignments tended to form as vehicles for the attainment of parliamentary majorities. The appearance of such a majority almost invariably prompted attempts to unify the opposition; that the opposition did not simply join the majority group is explained by the presence of a plurality of power-seeking faction leaders, the "outs" whom never relinquished their ambitions, and the "ins" whom never felt compelled to attract (with promised payoffs, no doubt) support beyond a simple parliamentary majority.

In power, the parties continued for the most part to play the game: compromise with the Privy Council, the House of Peers, the bureaucracy, and the military; and tactical, not principled, formation of coalitions and manipulation of issues. In attaining power the Taisho parties lost the reforming spirit which had characterized their predecessors; by the time they achieved cabinet control they had become simply one—and, institutionally, the least legitimate one—of a number of competing power centers. Reform appeared undesirable to some leaders and hopeless to others; ex-

cept for the brief attempts of Premier Kato, little effort was directed toward solution of the new economic and social problems which faced Japan, as an industrial state, after World War I.

The aspect of party power most significant for the future fortunes of the party system (here Mr. Duus intriguingly implies but justifiably terminates his analysis) was its non-use, either as an instrument for the reform of the system or in response to the challenges posed by a rapidly growing politically conscious sector of the populace largely barred from effective political participation. Consequently, the parties created neither institutional defenses for their position, nor an independent source of legitimacy, nor a public which, perceiving the parties as credible vehicles for reform, was committed to political parties in principle. The price of tactical effectiveness, as Mr. Duus notes, was strategic vulnerability; when rightist elements turned against the parties in the 1930's there were few defenses, institutional or ideological, upon which they could fall back.—JAMES W. WHITE, *Stanford University.*

Social Change in Soviet Russia. By ALEX INKELES. (Cambridge: Harvard University Press, 1968. Pp. 475. $12.50.)

Social Change in Soviet Russia by Alex Inkeles is a collection of essays written over two decades in time. The author has arranged them under several classifications, each section being introduced by a brief essay of recent origin. Professor Inkeles occasionally updates passages in some of the essays through the addition of footnotes or new paragraphs.

The only significant fault with this collection of generally superb empirical and analytical essays is in the misleading expectation created by the title, the preface, and the first chapters. The reader expects a more coherent and up-to-date examination of social change in Soviet Russia. Indeed, Inkeles raises some tantalizing questions that the reader eagerly anticipates finding sound and recent expositions on. Unhappily there are often only dated commentaries or only scant attention is given.

Inkeles identifies three main elements that are analytically distinguished and that constitute the basic process of social change in Soviet society. These are (1) ideology, especially the conceptual apparatus and the goals of the elite, (2) the social structure that was inherited by the revolutionary regime in 1917, and (3) new institutional forms and social forces set in motion by the leadership of the revolution. The interaction of these elements, Inkeles holds, has shaped the present structure of Soviet society. The author gives scant treatment to the element of ideology, especially to consideration of developments in Soviet Russia since 1953. In fact, it is the light treatment of the post-Stalin period that disappointed this reviewer most seriously.

Despite the foregoing expression of disappointment, the reader gives his warm approval to the groups of essays found in the book. In their original form they were and will undoubtedly remain major contributions to the study of the Soviet social system and especially to the examination of change wrought by the Soviet regime. The problem lies in the failure of the essays to be the natural companions of each other that the author presumes. "The Totalitarian Mystique" is especially out of place in a work dealing with change.

As when some of the essays were first published, a legitimate question can be raised concerning the possible bias of a sample of refugees from the Soviet Union. And, as then, the solution to this question can only be fully resolved with a similar study of resident Soviet citizens. Of course, such an undertaking has continued to be impossible. Otherwise, the research of the author is solidly empirical and where there are questions about the accuracy of a source, Inkeles is quick to inform the reader of his awareness of this question. As studies in the difficult task of conducting empirical investigations where the system is not often open to such investigations, Inkeles' works are models for all students of authoritarian regimes in general as well as of the Soviet system in particular.

To recognize the author as only an investigator of facts, however, would be to perpetrate a most serious error. The finding of facts is always just the beginning, for Inkeles. He is an analytical scholar of the first order and his powers of interpretation are recorded here through papers spanning twenty years in time. What is particularly impressive is his ability to change his interpretation as he finds additional evidence that counteracts previous observations and interpretations. And even more impressive than this is the accuracy and viability of many insights expounded in the late 1940's and early 1950's.

After re-reading the essay on the mystique of totalitarianism, the reviewer dipped his pen in ink to attack the author's adherence to a model that he deems to be inappropriate for most of the post-Stalin period. The succeeding essays dealt with other subjects, so the reviewer's initial reaction appeared to be justified. Then, in the last section, Inkeles returned to the subject of totalitarianism and concluded that it is no longer applicable as a model of the Soviet system because it is insensitive to the sources of social support for the regime, it fails to consider the experimental aspect of the regime and its programs, and it was historically limited in scope. Therein are the danger of selling Professor Inkeles short, and also of reviewing a book composed of such diverse essays writ-

ten over an extended period of time.—MICHAEL P. GEHLEN, *Purdue University.*

The Political Transformation of Tanganyika, 1920–1967. By HUGH W. STEPHENS. (New York: Frederick A. Praeger, 1968. Pp. 225. $12.50.)

To its British administrators, Tanganyika was always the backwater of East Africa, lacking the throbbing excitement or economic potential of Kenya, Uganda, and Zanzibar. Impoverished and sparsely inhabited, Tanganyika lagged behind her neighbors in the apparent pace of political change —at least until 1954, when the founding of TANU (Tanganyika Africa National Union) seemed to catch the colonial regime unaware. Only seven years were necessary to develop a high degree of politicization and to lead Tanganyika to independence.

The Political Transformation of Tanganyika 1920–1967 is not a constitutional history, nor is it an analysis of the organization and growth of TANU—though the impact of British rule and the spread of political awareness play key parts in the story. Rather than tread these worn paths again, Stephens strikes into territory few analysts of African politics have explored: The use of aggregate data for inferring patterns of political change.

Publications on African politics have usually neglected quantitative indicators, or have relegated statistical information to the realm of interesting but unimportant sidelights. The relative absence arose for an obvious reason: the data taken for granted in longer established states do not exist in most of tropical Africa. In undertaking to present a model for political change through the use of quantitative data, Stephens set himself an extraordinary task. The relative absence of appropriate data forced him considerably to modify the indicators of social mobilization suggested by Karl Deutsch in this Review in September, 1961. Stephens draws upon eleven processes of change: population growth, exposure to modernity (visits to out-patient clinics), literacy, mass media audience, modern political participation and political assimilation (both measured by TANU membership), linguistic assimilation, cultural assimilation (adoption of Christianity or Islam), a shift into the money economy, urbanization, and changes in the gross domestic income.

There are minor problems with the measures employed. For example, the mass media audience is expressed as a percentage of the population 15 years of age or older, which "represents 40% of the population"; however, the shift into the money economy is based upon the percentage of males fifteen years of age or older, which "is assumed to represent 25% of the total population." Surely approximate male-female parity exists in Tanganyika after 15 years of age. Modern political participation and political assimilation are both based upon TANU membership, a criterion that, as the author recognizes, cannot be projected prior to 1954 and which does not differentiate between nominal supporters, adherents, and party militants.

Stephens does not fulfill the hope expressed on page 117, where he defined the "main concern" of the study as "the effectiveness of the government in the post-colonial era." Only one of the six chapters—20 of the 125 pages of text—is devoted to events after 1961. In this all-too-short chapter, Stephens focuses upon the political repercussions of social mobilization, notably increased pressures for an expanded scope of government activity and increased political participation. This chapter lacks the detailed richness found in Henry Bienen's admirable study, *Tanzania: Party Transformation and Economic Development.*

Particularly since 1964, Tanganyikan leaders have attempted to expand the government's economic, social and political controls, even while its effectiveness declined in certain respects. As the government becomes more involved with overall regulation policy, it risks, as Stephens points out, becoming the focus for popular grievances and aspirations. The necessary shift into the money economy can be accelerated only through revitalizing agriculture, while diminishing the prospects for widespread discontent that could lead to upheaval.

The major aspect lacking from *The Political Transformation of Tanganyika,* as in all too many of the recent spate of pessimistic books dealing with sub-Saharan Africa, is a clear indication of the factors that enter into the capability of a particular political system. What are the capacities of a political system such as Tanganyika's? It is easy to write of civil service inefficiency and corruption, of declining foreign assistance and the failures of domestic development schemes, or of the political decay exemplified by mutinies and coups d'etat. Given the pace of social mobilization, is the collapse of the Tanganyikan government only a matter of time? Can the trends initiated during the colonial period lead to sophisticated efforts at social prediction? What are the political costs and benefits of altering domestic investment priorities among education, health services, agriculture, industry, communications development, or other necessities for the developing state? Surely measuring social mobilization should be complemented by quantifying the "outputs" of government policy.—CLAUDE E. WELCH, JR., *SUNY at Buffalo.*

The Soviet System and Modern Society. By GEORGE FISCHER. (New York: Atherton Press, 1968. Pp. 199. $7.95.)

Seldom has a book in this field appeared at a

more opportune time carrying such an interesting title. The study of politics and society in the socialist countries is emerging from a period of flux during which unsupported speculation has been increasingly replaced by hard data and theoretically more sophisticated work. This book, written on such a timely and important topic, has been awaited with enthusiasm and anticipation. Rarely, however, has a book so failed to live up to expectations or, indeed, even to speak clearly to what apparently are the author's own theoretical contentions. The reader is promised a treatment of questions regarding Soviet Party leaders' willingness to meet the wants of the people, the changing nature of skills required to guide the advancing Russian economy, and the broader question of how well a "monist" model of control can fit a modern society. The evidence and discussion deal mainly with determining whether there is increasing emphasis on politicians in "top" positions in the Soviet Union possessing both economic and political skills and training.

The data upon which the study and accompanying inferences about the future organization of Soviet society are based consists of a "saturation" sample of official Soviet biographical material on six types of officials designated as "top" executives. Quite understandably this includes Secretaries of the Central Committee, the heads of major national departments, and First Secretaries of the party organizations in fourteen of the fifteen republics of the USSR. These officials compose twenty percent of the sample. Another fifteen percent is made up of Second Secretaries of republics and First Secretaries of party organizations in republic capitols. It is considerably less understandable why the remaining two-thirds of the sample is composed of First Secretaries of province level organizations if political power if the main criterion for inclusion in this sample of emerging top leadership. There are many national level officials not included in the sample that possess a great deal more power than the heads of many of the more obscure provincial party organizations. Granted, there are difficulties in data collection, but the indiscriminate use of all available data poses special problems in this case since the author often points to rather small percentage shifts in the composition of the entire sample as evidence of long-range trends in leadership change in top posts in the Soviet Union.

Using judgements of career patterns as a basis for making distinctions, the sample is divided into four types of executives. The "dual executives" compose sixteen percent of the sample and are characterized by career patterns showing a great deal of technical as well as party work in the economy before receiving a high level post. "Technicians" make up one-third of the sample and have career patterns showing extensive technical work in the economy, but no party work before attaining high office. "Hybrid executives" have technical training but little work in the economy before moving to the top. Nearly half the sample falls into the fourth category, "officials" having limited technical training or experience in the economy before their rise to power.

Using a basic collection of seven variables gleaned from Russian biographical sources in combination with the four created categories of officials, the author generates an amazing number of percentage tables. By the time the possible combinations have been exhausted, sixty tables have found their way into the one-hundred-fifty pages of text. Not surprisingly, many of the tables have little value for the reader and some reveal such small percentage differences that they could have easily been eliminated. The evidence indicates that increasing numbers of occupants of top posts have both economic and political skills. The author presents little additional information about what this means in behavioral or attitudinal terms. Is an "official" really that different from someone falling in the "hybrid executive" category to make these distinctions theoretically worthwhile?

At no place in the book is the reader introduced to the human beings that are being studied. The "dual executive" remains a mysterious, shadowy abstraction, reduced to a set of attributes on paper which don't really tell the reader anything about these individuals as people or decision-makers. One is told that twenty-four percent of newcomers to top positions qualified as dual executives in 1962 while only thirteen percent of the newcomers were dual executives in 1958. The meaning of the small differences reflected in these types of statistics is questionable as is the use of this material in building theory in this area, especially given the subjective coding system that assigned officials to one of the four categories in the first place.

Even if it is conceded that the material presented supports the argument that there is a long-term trend toward dual executives in the Soviet system, this does not necessarily speak to the author's contention that the monist model of organization can fit the socialist society of the future. In all advanced societies, monist as well as pluralist, political leaders must increasingly obtain technical skills and experience if they are to govern these complex societies effectively. This development takes place quite separately from a movement toward or away from a pluralistic representation of group interests, however, as the skills possessed by political leaders have no necessary relationship to the organization of the system within which they function.

Credit should be given to the author for generating an interesting body of data on a very timely topic. It is unfortunate that he chose to communicate his results in boook form rather than in an article or two that would have undoubtedly been extremely well received. The narrow data base, the difficulty of interpreting the countless tables, and the many unsupported attempts at theoretical generalizations limit the value of the study in book form. It is, however, an interesting treatment of an important body of data that will be of some significance until someone treats this subject in much greater detail.—DENNIS C. PIRAGES, *University of Connecticut.*

Fifty Years of Communism in Russia. ED. BY MILFORD M. DRACHKOVITCH. (University Park: Pennsylvania Press, 1968. Pp. 316. $7.50.)

Aspects of Modern Communism. ED. BY RICHARD F. STARR. (Columbia: University of South Carolina Press, 1968. Pp. 416 $7.95.)

The papers edited by Professor Drachkovitch are from a 1967 conference sponsored by the Hoover Institution on War, Revolution and Peace to mark the fiftieth anniversary of the Bolshevik coup. The contributors view various aspects of Soviet life over the perspective of the last fifty years. The papers edited by Professor Staar are from a closed conference on communist-ruled states held in 1968 under the auspices of Emory University. The book includes an introductory chapter by Jerzy Hauptmann; three papers on the Soviet Union; three on Eastern Europe, which is treated as a single unit; three on Communist China; and single papers on the Mongolian People's Republic, North Korea, North Viet Nam and Cuba. Each of these papers is followed by the remarks of a discussant.

The Drachkovitch book is a collection of interpretive essays. The official theme of the conference at which they were delivered was "Fifty Years of Communism in Russia." However, the conference organizers, we are told, decided to focus on a particular sub-theme: namely, the relationship between the original promise or message of the revolution and the performance of the Soviet regime over the last five decades. Drachkovitch notes:

> In their respective fields, the essays in this volume analyze why and how these initial promises were not and could not have been kept. (p. 4)

Drachkovitch argues that this is a legitimate analytical approach. Perhaps it is, but it is also one that introduces a certain bias by emphasizing a particular kind of failure.

Happily, most of the contributors overcome the problem posed by the conference's sub-theme and produce fair balance sheets of significant Soviet successes and failures. The result is a series of fine papers. Among the best of these are Leonard Schapiro's paper on the Soviet polity; John N. Hazard's on Soviet law; Raymond Garthoff's on the Soviet military; Max Hayward's on Soviet literature; and Ivo J. Lederer's on Soviet foreign policy.

Nevertheless, the conference's sub-theme sometimes produces results that jar the reader. In his conclusion to his very good paper on Soviet foreign policy, Lederer inserts a paragraph in which he notes that the Bolsheviks have not been able to fulfill their revolutionary pledge to abolish diplomacy and inter-state relations. He then promptly and wisely drops this matter and proceeds to a discussion of more important failures *and* successes. On the other hand, I suspect that the sub-theme caused G. Warren Nutter little discomfort. Those familiar with his work will not be surprised to learn that he has no apparent difficulty in writing twenty four provocative pages on the Soviet economy without expressing one positive observation about it. The focus of the conference also may have prompted Bertram Wolfe to conclude his essay on Marxism and the revolution with a reassertion of his thesis, now nearly ten years old, that the Soviet regime is one of permanent illegitimacy. This thesis rests, as it always has, on the verbal trick of equating legitimacy with representative government.

Readers will find much with which to agree and disagree in this collection of interpretive essays. All the papers are worth reading, and many are exceptional. After having read the book, I had only two regrets. The first was the focus of the conference. The second was that "technical difficulties" prevented the inclusion of any of the conference's critical discussion of the papers.

The Staar book is not nearly as successful a book as Drachkovitch's. Like many collections of papers, it has no focus. Papers on different countries, even when they share a common general topic such as politics or economics, do not discuss comparable subject matter. A reader looking for a contribution to the comparative study of communist systems will find this book disappointing.

The papers themselves are uneven in quality. A few are analytical and provocative, and some of these are followed by equally provocative critiques. One such example is the exchange between W. W. Kulski and Yaroslav Bilinsky on the nature of post-Stalin political developments, although some may regard Bilinsky's remarks as having a Cold War flavor. Richard L. Walker contributes a searching discussion of the problem of maintaining revolutionary elan in Communist China. If they are read together, the paper by Desmond P. Wilson, Jr. and the critique by Henry

H. Keith provide a good summary of Cuban-Soviet relations.

Most of the papers, however, contain little analysis. Some, including a paper by Hermann Gross on East European economic integration and a paper by Chu-yuan Cheng on the Chinese economy, are valuable as good summaries of useful data. Others, such as Staar's own paper on East European polycentrism and Dae-Sook Suh's paper on the emergence of North Korea's political elite, are merely descriptive. William B. Ballis, according to Edward D. Sokol, is guilty of factual errors and omissions in his paper, which is a rather uninteresting historical and institutional description of the Mongolian People's Republic.

Furthermore, not even all of the interpretive efforts succeed. James McConnell displays considerable creativity in his efforts to relate changes in Soviet military strategy to changes in ideology. He also provides, unwittingly, proof of the danger of using ideology as an explanatory variable in studying Soviet policy making. As Kenneth R. Whiting points out in his critique, many of the changes in strategy that McConnell links to shifts in ideology can be explained better by political and economic factors.

Finally, there is the introductory chapter by Hauptmann. Throughout most of the chapter he does a good job of extracting common themes and problems from the conference papers. Unfortunately, in the last part of the chapter, he attempts to construct a typology of communist ruled states based on the "concepts" of "goal rationality" and "sophistication." The typology's inherent problems are clearly revealed by the Kulski-Bilinsky dispute, which immediately follows this chapter, which occurred apparently before its writing, and which revolves around the question of whether the Soviet political leadership has become significantly more rational and more sophisticated since Stalin's death.

Informed readers will find most of the essays in the Drachkovitch book worth reading. Students of communist systems will find some of the papers in the Starr collection rewarding. Nevertheless, I wish that the better papers in the Staar book had been published individually as journal articles, and the others reworked and further developed for later publication. Taken as a whole as they now stand, they do not make a very satisfactory book even with the inclusion of the discussion, some of which is very good.—JAMES H. OLIVER, *University of Maryland.*

The Electoral System of Finland. By KLAUS TÖRNUDD. (London: Hugh Evelyn, 1968. Pp. 181. 42 shillings.)

The Revolution of 1905 did not produce lasting institutional reforms in Russia. In the Grand Duchy of the Russian Emperor, however, Finnish participation in that revolution produced a system of representation which remains in effect, and which Törnudd correctly argues is likely to exist as long as there is an independent Finnish political system. In *that* revolutionary period, Finns took advantage of the weakness of the Emperor to create a profoundly equalitarian institutional basis for Finnish politics, just as they later, in 1917, took advantage of the absence of the Emperor to implement first parliamentary government and then national independence. Before the Revolution of 1905, the Finnish Four Estates (nobility, clergy, burghers, and farmers) were still essentially feudal in nature. The Finnish reform of 1906 at one sweep took Finnish politics into its present modern era, for it created a unicameral Parliament elected by universal suffrage on the basis of proportional representation. With the enormous expansion of the electorate that this reform involved, Finnish parties had to expand both their ideological perspectives and their organizational structures to appeal successfully to the new voters. The reform of 1906 thus created the modern party system of Finland as well as the modern electoral system.

Törnudd describes the creation and the operation of the new system of representation in competent manner. He presents a vast amount of material, much of it in statistical form, which should be used by every political scientist interested in the relationship between electoral systems and party systems. Douglas Rae's *The Political Consequences of Electoral Laws,* for instance, would have benefited substantially from drawing upon Törnudd's data. Törnudd's tone is clinical, and he is moderate in his judgments and fair in his wording. There is none of the passion here that is found in most of the writings on electoral systems, whether of the one extreme of Hermens or the other extreme of Lakeman. Indeed, one of the major weaknesses of Törnudd's book is that, for the most part, he does not use the Finnish evidence he has so carefully compiled to argue against the extremists of one or another school. This reviewer, perhaps unjustly, suspected that this reluctance flowed from a lack of familiarity with many of the "classic" writings on electoral systems.

This book is not comparative enough. There is too much on Finland in it, including all kinds of parliamentary history of dubious relevance to the topic at hand, and too little political science. In many places in the book, for instance, the material virtually cries out for a comparison with the hypotheses of Maurice Duverger, but there is only one passing reference to Duverger's work, and none to *Political Parties.* Duverger's analyses of party strength, party schisms, and the "basic ele-

ments" of party structure would all have helped Törnudd avoid some analytical confusions. Also badly needed are explicit detailed comparisons with French and German electoral systems—and not Weimar alone. The Finnish situation between the electoral reform and the achievement of parliamentary government a decade later, for instance, was similar to the Imperial German situation in which universal male suffrage existed long before parliamentary government. The act of governing was separate from the act of representation, with very important consequences for party development, not least being the disenchantment of significant portions of the working class with representative institutions.

There are some other weaknesses in Törnudd's book. The author's English is often rather awkward, and occasionally misleading. The index is unreliable. Finnish political scientists these days are not famous for their command of Finnish history, and Törnudd is no exception. Sometimes he is a little indolent, as when he simply reports that information on the number of cells in the Finnish Communist Party is unobtainable. Sometimes he jumps to unwarranted conclusions, as when he argues: "When a party puts up the same candidate simultaneously in several constituencies, this can obviously be taken as a sign of central intervention in the nomination." As Törnudd concedes, research into the nominating process is notably lacking in Finland, in spite of the abundance of voting behavior studies. It might just as well be the case in such multiple nominations that constituency party leaders want one or more prominent national party leaders as candidates in their constituencies, especially since such national party leaders might later feel a sense of special obligation to "their" constituency. Provincialism, in the most literal sense, is not unknown in Finnish politics, and Finnish multi-member constituencies have essentially the same geographical boundaries as the Finnish provinces.

Törnudd's title is misleading. He does not treat all the aspects of the Finnish electoral system, and it is here that his most important omission of Finnish material occurs. He discusses only parliamentary elections. Local governmental elections have had considerable impact on the development of the Finnish party system. Doubtless more important, however, is the omission of presidential elections. Finland does have parliamentary government, but it also has a very powerful President of the Republic. The Constitution of 1919 gives the latter very great power, and with one exception (Relander) every Finnish President has made full —sometimes overfull—use of his constitutional prerogatives. Finnish presidential elections do count, and any study of the political importance of the way votes are counted in Finland is deficient if it ignores them. In many important ways, party development has been influenced by the nature of the presidential election, just as parties have influenced the nature of the presidential election. The party configuration in presidential elections is not always, and perhaps not even often, the same as the party configuration in parliamentary elections. It might even be argued that Finland has a different party system for presidential elections than for parliamentary elections. This matter, which would be of considerable interest for students of American parties, Törnudd does not even mention. Since there are some solid studies—in Finnish—of Finnish presidential elections, and the official Finnish presidential election statistics are full of valuable data, this omission is all the more surprising. A second edition, perhaps — MARVIN RINTALA, *Boston College.*

The Nazis in the Balkans. A Case Study of Totalitarian Politics. By DIETRICH ORLOW. (Pittsburgh: University of Pittsburgh Press, 1968. Pp. 235. $7.50.)

The inadequate utilization of social science concepts and methods in the study of dictatorships has resulted in a conspicuous lack of integrative empirical theories about contemporary autocratic politics. This is particularly true of the literature on National Socialism which has been basically ideographic in method, historical and ideological in orientation and broadly factual in content. Personality and policy questions and concern with the dramatic and extraordinary aspects of the regime have long dominated Nazi studies, leaving the everyday functioning of the system largely in the realms of myth and speculation. We know little about the political process in the Third Reich: at this stage it would be difficult to provide informed answers to the crucial question: "How did the Nazi system work?"

There is, however, an increasingly noticeable concern about the political process among students of National Socialism. Recent studies have begun to supplant the grand generalizations about the nature of politics under the Nazis. Painstaking empirical investigations of the more mundane aspects of the regime have started to convey a more realistic picture of the political process. Professor Orlow's study of an obscure organization in Nazi Germany is illustrative of this new orientation.

The study of the SOEG (Southeastern Europe Society) successfully demonstrates that the monolithic facade of totalitarian rule concealed a multiplicity of goals, bureaucratic chaos, over-lapping empires, fief-warfare as well as personal feuds and rivalries. The SOEG, which was founded to achieve coordination and control of German policies in Southeastern Europe, was a total failure. The large number of competing agencies with con-

flicting goals and ambitious leaders, the absence of clear lines of authority and the lack of institutional hierarchy made the imposition of a consistent and rational policy in that area impossible. The personal nature of power relationships frustrated the institutional delimitation of authority and resulted in continuous power struggles and bureaucratic warfare. The only fixed point, the only common reference was Hitler who remained above the conflicts as the supreme and ultimate arbiter for whose favors all parties vied.

The study is a well-documented historical account of the establishment, growth and ultimate demise of a minor organization in the Third Reich. In analyzing the tactics and maneuverings of Nazi leaders, the conflicts of issues and of personalities, the study illustrates the confused maze of institutional and personal relationships that determined the nature of decision-making in the regime. However, the book suffers from two major shortcomings: as a historical account it may be criticised for its inadequate historical framework, while as a case study of totalitarian politics it lacks sufficient conceptualization.

After following the author's detailed analysis of the issues and personalities connected with the operation of the SOEG, the reader is left with less than a meaningful perspective of Nazi activities in Southeastern Europe between 1940 and 1945 (since the SOEG was concerned with all the countries in the area south of Poland, "Southeastern Europe" is a more accurate term than "the Balkans"). Granting the lack of consistent policies and frequent shifts of emphasis due as much to the rapidly changing military situation as to internal power relationships, there were still some constant themes (such as policies toward the Volksdeutsche, economic organization and trade) that could have been handled in broader and bolder strokes. In addition, a historical perspective of German designs in Southeastern Europe would have lent the study a much needed framework within which Nazi policies and SOEG plans could have been better evaluated and understood.

The lack of adequate conceptualization deprives the author of the opportunity to make a much needed theoretical contribution to the study of totalitarianism in general and Nazism in particular. In the absence of any analysis of the concept of totalitarianism, it remains unclear why and in what respects the SOEG experience would demonstrate "totalitarian politics." The failure to utilize empirical data for the construction of hypotheses necessarily limits the study to description. The author fails to suggest relationships among the several characteristics of Nazi politics which makes his account interesting, but not explanatory of the system as a whole. What is one to make, for instance, of his statement concerning the incompatibility of Nazi ideology with economic and political realities? Such a statement lacks extrinsic meaning and fails to have explanatory value unless its relationship with other system attributes is made explicit by menas of hypotheses. Does an inoperative ideology tend to enhance conflict between "bureaucrats" and party men? Does it contribute to the multiplicity of goals and bitter rivalries? Does it tend to prevent institutionalization and stabilization of relationships thereby fostering confusion and insecurity which leaves the dictator the only fixed point in the system? His observation that a number of "systems" existed simultaneously in the Third Reich is similarly intriguing but non-explanatory in the absence of theory. Does this suggest the existence of a series of concentric circles of personal influence which frustrates the establishment of bureaucratic hierarchy? Could it explain the importance of *Rückendeckung* or personal backing without which no lower level official could hope to succeed in the Hitler regime? What is the theoretical significance of the fact that the SOEG was established as a private organization to coordinate public policies? These and many other questions are suggested by the author's account, but unhappily, it is left to the reader and to future scholars to extract theory from the data.

In sum, the study illustrates the limits of empirical investigations in the absence of theory which could order, connect and explain observed system characteristics. A description of the parts cannot explain the workings of the whole. Personal rivalries, bureaucratic chaos, empire building and the overwhelming authority of the dictator do not add up to a functioning, interdependent system, nor can they explain the political process without the help of intervening concepts and theories. Professor Orlow has written a thorough empirical study of the SOEG and succeeded in identifying a number of important characteristics of Nazi politics. To what extent he as a historian should be criticised for the lack of conceptualization and theory building depends on one's conception of history as a discipline.—JOSEPH NYOMARKAY, *University of Southern California.*

Reform und Aussenpolitik: Die Aussenpolitik der SPD Berlin-Bonn. By ABRAHAM ASHKENASI. (Köln und Opladen: Westdeutscher Verlag, 1968. Pp. 220. 25.00 DM)

The SPD in the Bonn Republic: A Socialist Party Modernizes. By HAROLD KENT SCHELLENGER, JR. (The Hague: Martinus Nijhoff, 1968. Pp. 247. 31.85 Guilders.)

There is probably no other European political party that has been the subject of more scholarly study than the German Social Democrats. The party's fascinating history, the ideological orienta-

tion of most academics, and the SPD's own excellent archives have combined to make it an attractive topic for historians, sociologists, and political scientists. The two additions to the literature on the SPD under review are both concerned with the party's post-war modernization. The work by Schellenger focuses primarily upon the party's domestic policies and general ideological transformation. Ashkenasi's book is more concerned with the SPD's foreign policy and particularly with the differing foreign policy programs developed by the governing SPD in Berlin, led in the post-war period by Ernst Reuter and later Willy Brandt, and the Social Democrats in West Germany, who were in opposition throughout the period examined in the book.

Schellenger attempts to test Michels' dictum that "power is always conservative" with reference to the SPD's post-war leadership. If power breeds conservatism, how can the organization ever innovate as the SPD apparently did? His argument is that Michels' thesis does not preclude adaptation and innovation if the party's elites perceive that reform is necessary in order to save the organization from decline and eventual disintegration. The SPD's elites perceived that as a result of the changing "political, social, and economic environment" in post-war Germany and Europe, reform was needed if the party was to remain a political force. Schellenger thus claims to have extended Michels' thesis with the findings of the "modern political sociologists," who demonstrated (apparently to both Schellenger and the SPD leadership) that the old programs and style were irrelevant.

This explanation for the SPD's self-modernization is hardly new and, as a conceptual framework, extremely thin. Indeed after a brief theoretical introduction, Schellenger makes no further mention of his working hypotheses until the last few pages of the conclusion in which he makes the above-mentioned modification of Michels.

The bulk of the book is actually a traditional, straight-forward description of the SPD from 1945–1961. After a brief sketch of the history of German socialism, Schellenger devotes three chapters to the SPD's programmatic development, which culminated in the adoption of the Bad Godesberg *Programm* in 1959. Schellenger gives us an account of the policy debates held at every national *Parteitag* from 1946 to 1960. As part of this analysis and in an attempt to determine the ideological orientation of the various *Bezirk* organizations, he analyzed policy resolutions submitted to the SPD's national conferences from 1950–1960. But he makes no systematic presentation of his findings. Perhaps the earlier work of Richard Rose on English party activists, which also used constituency resolutions to the national conferences of the Conservative and Labour parties for data on the ideology of party activists, would have been helpful here.

Schellenger then describes the influence of the Berlin SPD on the national organization and the gradual influx of "new-types" of leaders into the SPD's national organization. These three chapters on the party's leadership are the best in the book. Schellenger gives us a lively account of the conflicts within the Berlin organization between the Old Guard led by Franz Neumann and the modernizers led by Reuter and Brandt. He correctly points out that the intra-party squabbles were not only over party policy, but also included strong personality differences and conflicts over the party's style and image. He then examines the West German organization and describes the growing influence and rise to national prominence of other reformers in the various *Länder*. He sees this process of modernization culminating in the new "package" of personalities and policies the SPD presented to the electorate in the 1961 election.

His concluding chapter discusses other European Socialist parties and their strategic and tactical adaptations to the challenge presented by the "end of ideology". Schellenger relies here almost exclusively on the earler writings of Lipset and uncritically accepts the "end of ideology" argument.

Ashkenasi's main focus is on the conflict over foreign policy programs that took place within the Berlin SPD and the national organization from 1946–1961. He attempts to demonstrate that the governmental responsibility held by the Berlin organization made it more amenable to the pro-Western foreign policies of the Adenauer coalitions than to the policies of the Schumacher-led national SPD. He argues that the Berlin SPD, by virtue of its position as a governing party, had by the early 1950's made the transformation into a modern catch-all party several years before the national organization realized that the Schumacher strategy would doom the SPD to permanent minority status.

This is a difficult thesis to systematically examine. If governing alone induces a more moderate, pragmatic approach among Socialist parties, as Ashkenasi contends, how can one explain the fact that the Hessian organization of the SPD, which has consistently been the most "leftist" of all *Bezirk* organizations, is also a SPD stronghold? Clearly other factors such as the personalities of different leaders, and regional variations in the social structure of the party's membership are also involved.

Ashkenasi's sole concentration on disputes over foreign policy is also quite artificial. As he must admit at various points throughout the book, it is difficult, if not impossible, to separate foreign from domestic policies when examining intra-party

conflicts. He mentions only as an afterthought that perhaps disagreements over policy were sometimes artificial and actually cloaked what were mainly clashes of personalities. With the same data one could also easily argue that policy was largely secondary to personality and power considerations.

Ashkenasi's study then is also poorly conceptualized. The material he gathered could have been effectively used to analyze such problems as the relevance of foreign policy as an issue area in Germany or the domestic sources of the SPD's foreign policy. Depth interviews with the major participants still living, would also have perhaps enabled him to ascertain whether foreign policy was actually an independent factor in the intra-party conflicts. Although both authors conducted interviews with party officials, they give no systematic presentation of their findings. The interviews were apparently of the "background" variety.

It is especially regretable that neither author examined the social bases of SPD support during its modernization period. No attempt was made to link policy and leadership changes with electoral outcomes. The data for such a venture is available. Since 1953 the SPD has periodically commissioned extensive studies of the German electorate. Even an examination of the past research reports of organizations such as the *Institut für angewandte Sozialwissenschaft* in Bad Godesberg, which does most of the SPD's polling work, might have produced information on the effects of policy and leadership changes upon the electorate. The utility of such surveys for secondary analysis needs investigation by students of comparative political sociology.

In summary, Schellenger's study essentially supplements Douglas Chalmers' earlier work on the post-war SPD. His detailed accounts of various party conferences and the personalities of SPD leaders will be useful additions to the English language literature on post-war German politics. Ashkenasi's study will be helpful as a convenient reference for those interested in the details of disputes within the SPD over foreign policy. But as contributions to the systematic comparative study of political parties, both works are less than satisfactory.—David P. Conradt, *University of Florida.*

Students and Politics in Developing Nations. Edited by Donald K. Emmerson. (New York: Frederick A. Praeger, 1968, Pp. 444, $9.50.)

Students in the developing nations of Asia, Africa, and Latin America constitute a dynamic and highly influential action group. Large-scale student protests have been instrumental in toppling such authoritarian regimes as Jiménez in Venezuela, Rhee in South Korea, Diem in South Vietnam and Sukarno in Indonesia. In recent years student strikes, protests and boycotts have indeed become one of the most active forces in promoting political and social change in developing nations.

Who are these student activists? What biographic variables influence their behavior? What are the motivating forces behind student political movements? How does the university and its milieu affect student activism? What are the relationships between student politics and social change, as well as the interactions between student politics and national politics? Donald K. Emmerson and fourteen contributing authors explore these questions in the context of the developing nations of Africa (Algeria, David B. Ottaway; The Congo, Jean-Claude Willame; Ghana, David J. Finlay, Roberta E. Koplin, and Charles A. Ballard, Jr.; South Africa, Martin Legassick and John Shingler), Asia (China, Bruce D. Larkin; Indonesia, Harsha W. Bachtiar; South Vietnam, David Marr;), and Latin America (Brazil, Robert O. Mayhr; Chile, Myron Glazer; Cuba, Jaime Suchlicke: Venezuela, William L. Hamilton.)

In his concluding chapter, which is an analysis of the findings of the above authors, Emmerson indicated that student activists in the developing nations are most likely to be made. They most often live away from their families and are usually less religiously oriented than their parents. He finds that both parental status and income are unreliable indicators of student behavior and political attitudes. The university radical who adopts a given political position simply because it is contrary to his father's beliefs is indeed the exception. Moreover, the principal factors that appear to contribute most to student radicalism in developing areas are the sudden release from family constraints, the disorienting effects of an urban environment, and the increased opportunity for interaction among peers.

The university mileau also contributes to the politicization of the student. In the developing nation, the university is most often located in a large city, usually the capital. By providing easy access to political information, large national audiences utilizing an extensive communications network, proximity to both government and opposition forces, and often extensive areas of poverty —the large city tends to facilitate the work of the student activist.

The quality of the university system is a crucial variable. According to Emmerson, where faculty is part-time and underpaid, where lectures are given to large anonymous crowds, "where 'education' means passing examinations and 'learning' means cramming for them, where the student is unsupervised, unstimulated, and finally unrewarded"—under these conditions student unrest is almost inevitable. However, when barriers to university en-

trance are raised, when curriculum as well as occupational incentives become more professionalized, when effective communication lines are established between faculty and student, when the quality of the faculty and university life is upgraded, student activism is likely to decline.

Most of the contributing authors find that protests follow a general pattern. These protests may extend over a few days or several years, but the pattern seems to be standard: an initial, perhaps almost insignificant incident, followed by initial protest, "repressive" counteraction, massive protest, attempts to organize for sustained activity, further demonstrations revolving around a larger set of issues, increased partisanship and factionalism, lower enthusiasm, isolation and eventual decline.

Student politics obviously differ from "adult politics." Emmerson infers that the main difference lies in the fact that the former are more often the art of the impossible and that student activists do not understand the rules of the adult political game. Yet is it that they do not understand these rules or that they do not *want* to understand them? Students emphasize style over program, committment over compromise. This fact, Emmerson points out, is both their weakness and their strength. Perhaps unconsciously, they endeavor to find and to take advantage of every available opportunity to trigger larger political and social forces into action. "But whatever success in opposing the injustice and oppression they so keenly feel," says Emmerson. "(they) can at least serve to break the silence, to indicate fresh directions for the polity. That, in itself is no mean achievement."

In constructing this book, Emmerson has selected a small but representative assortment of developing nations. However, even Emmerson admits that the chapter on South Africa—though this is not a criticism of the chapter itself—appears anomolous to the book as a whole; surely the nation that pioneered the first successful human heart transplant seems a bit out of place among an array of developing African, Asian and Latin American nations. Each contribution is well-written, readable and internally consistent. Though Emmerson makes no mention of a common conceptual framework for the structuring of each chapter, his book, unlike many other symposia, seems to possess such an attribute.

The task of the political scientist in this budding and explosive field, now referred to as *students and politics,* is to search for the overall causes of student discontent and activism. Emmerson et al. have begun the slow, arduous process of mapping the roots, causes and trends in student political activism. They have begun to analyse and manipulate the available data, and in doing so have indicated many inconsistencies and information gaps. The book is a beginning, a pioneering effort in the study of student activity and politics in developing nations. As such, it provides a penetrating insight into some of the forces that encompass and bewilder our turbulent age.—HARLAN J. STRAUSS, *University of Oregon.*

Paul Miliukov Political Memoirs, 1905–1917. EDITED BY ARTHUR MENDEL, (Ann Arbor, the University of Michigan Press, 1967. Pp. 508. $9.75.)

Rationalism and Nationalism in Russian Nineteenth-Century Political Thought. BY LEONARD SCHAPIRO. (New Haven: Yale University Press, 1967. Pp. 169. $5.00.)

The Making of a Workers' Resolution, Russian Social Democracy, 1891–1903. BY ALLAN K. WILDMAN. (Chicago: The University of Chicago Press, 1967, Pp. 271, $7.95.)

The Crisis of Russian Populism. BY RICHARD WORTMAN. (New York: Cambridge University Press, 1967, Pp. 211. $7.50.)

The varied reactions of Russian intellectuals to the plight and prospects of their country in the nineteenth century seem never to lose their interest, or so it would appear from the regular appearance of new books dealing with this important theme. The significance of the issues, even for our own time, and the vigor and originality of many of the writers, explain the continued interest. The radicals remain the favored subject, through, as Leonard Schapiro notes, it is impossible to justify this either in terms of the quality or originality of their ideas. Indeed if one were to use the chatchword of the contemporary left—"relevance"—as his measure, the radicals would lose hands down.

The Schapiro essays on *Rationalism and Nationalism in Russian Nineteenth-Century Thought* were lectures delivered at Yale University in the autumn of 1965. The conceptual notion which Schapiro used to divide Russian political thinkers is stated in the title: the rationalists, who emphasized the possibility of fully rational political decisions, and the uniformity, or universality, of the political development of nations; and the nationalists, who stressed the uniqueness and the continuity of the traditions and institutions which form the political mentality of a people. These contrasting attitudes are illustrated in the opening chapter by the familiar examples of Speransky and Karamzin, as well as some of their less well-known contemporaries. The dominant theme of Russian nineteenth century thought is presented as a dynamic and creative tension between these two traditions, and the most appealing intellectual figures are the moderates of either camp who blended elements of both traditions: rationalists

with some sense of history, particularly the historical development of key Russian political institutions; or nationalists whose respect and concern for national political and social institutions were informed by a sense of the ideas and institutions which offered reform notions adaptable to Russia. Hence the tribute to Pushkin, whose "mature political thought, so far as it can be discerned, seems to be that rare synthesis of both liberalism and conservatism and of both rationalism and nationalism." Similar considerations lead Schapiro to give Granovsky and Chicherin more favorable comment than Herzen, and to see creative and highly original thought in the writings of the latter-day Slavophile and Zemstvist, Dmitri Shipov.

Schapiro finds a left and right wing in both the nationalist and rationalist camps. But whatever the degree of radicalism, structurally the ideas of the groups in each camp betray a common lineage. Thus the Socialist Revolutionaires traced their descent to the Slavophiles and to Karamzin rather than to the traditions which inspired the Social Democrats. Within this interpretive structure Lenin was not a rationalist and a "European," in the pattern of Struve or Plekhanov, but a nationalist. Lenin followed the populist lead in rejecting a long-term, progressive role for capitalism in Russia—"Capitalism was something that was already there, in the village as well as in the town, and something already to be destroyed." The ideas of a one-stage revolution and of a uniquely Russian path of development are evidence that he belonged in the nationalist revolutionary tradition, the tradition of Paul Pestel and Peter Tkachev. Comparing him with the latter, Schapiro notes:

> . . . the real parallel between Lenin and Tkachev lies in their nationalism—the sense that each had of the realities of the Russian scene which had to be taken into account before there could be any hope of a successful revolution. Most important perhaps is the conviction the two men shared that the overriding reality in Russia was the autocracy; that the autocratic machine had to be captured and used; and that this could only be effected by a disciplined minority, and not by the mass.

The right wing of the nationalist current of political thought was represented by Lenin's contemporary Shipov, whose thought, according to Schapiro. "represented at the time one of the most enlightened attempts to reconcile autocracy and reform, of the kind which had repeatedly failed in the course of the nineteenth century."

Thus, as Schapiro sees it, all political argument in nineteenth century Russia was, in the end, about the same subject—the autocracy. For this was the only institution of significance until the *zemstva* took feeble root. Among the revolutionaries the effort was either to overthrow it, and to build anew, as with the populists, or to capture it and use it in the interests of another class, as Pestel, Tkachev, and Lenin wished to do. Shipov sought to broaden the foundations of government by means of the *zemstva,* while the liberals attempted to build a whole new set of public institutions capped by an elective parliament. The events of 1917 marked the failure of the liberals to make their parliament an effective organ of government and brought the Bolsheviks to power. But history played a trick on the Bolsheviks, for with the institution of autocracy they inherited also the main problem of their predecessors: "how to run a country effectively with the sole aid of an outmoded autocratic system, and at the same time aspire to modernization."

Richard Wortman writes about the main radical expression of Schapiro's category of "nationalist" political thought, populism. For him the chief deficiency of the available literature on the populist movement is that it does "not consider the problem of the basic nature of the populist ethos. We have learned much about the events, organizations, and participants of the movement, but little about the populists themselves." He then proceeds to provide a good deal of interesting detail about the personal and psychological experience of a number of populists—most notably, Aleksandr Engel'gardt, Nikolai Zlatovratskii, and Gleb Uspenskii. The essays on these three colorful figures are extraordinarily well-written and informative. Wortman is right in his insistence that one needs a sensitive perception of the personal and intellectual pilgrimage of individual populists—needs to observe Engel'gardt attempting to form intelligentsia agricultural communes, to sense in the words and experiences of Uspenskii the real meaning of intelligentsia insolation from "the people," and to savor the deep disillusionment of Zlatovratskii in his later years—to understand the human meaning of populism. Yet he has not neglected the task of systematic analysis of populist ideas, and the later portions of the book perform this function admirably. These cover the history of the populist movement through the 1800s when in the aftermath of the destruction of populism as an organized movement, an effort was made to revive populist doctrine. Wortman writes with particular perception of the appearance of Marxism in Russia, and of the rather confused and confusing eclecticism with which populist intellectuals sought to incorporate elements of Marxism in their own thought.

If there is any major criticism which one might direct at this fine book, it is that while its author is thoroughly sympathetic to the individual populists, he is generally somewhat biased against populism as a political doctrine. He writes of populist doctrine at the end of the 70's that it "dissolved into a mist of exalted concepts whose

meaning was difficult for them to perceive." Or later, writing of the 80's, he speaks of a doctrine that had "turned into a body of sacresanct principles meant to comfort rather than to arouse." The general impression conveyed is that most populist doctrines consisted of "exalted concepts" or "illusions." The positive elements of populist-thought —its perception of the unique elements of Russian reality, and its persistent and increasingly productive study of the chief ingredient of that reality, the peasantry, deserves more favorable attention.

The period of the history of Russian Social Democracy which Professor Wildman treats (1891–1903) has been given a good deal of attention by many authors. He justifies his monograph as one "which concentrates on the substructures of the movement, one that defines the participating social groups, their attitudes and motivations, their interaction and mutual responses, and the resultant inner tensions and conflicts." What this means in application is a history of the Social Democratic movement during these fateful thirteen years which centers on the theme of the relations of intellectuals and workers. Much attention is given to the organization of Marxist "circles" in the 1890's, to the reactions of the workers to this activity, to early efforts at training of workers, strike activity, etc. He finds the efforts of the SDs to organize the workers extremely effective, and challenges the interpretations of Pipes and Keep which "have tended to ignore or depreciate this development and treat the workers' movement and Social Democracy as two entirely unrelated phenomena." Wildman finds the SDs increasingly effective in their efforts to bring the workers into the movement during the 1890's, as evident, for example, in growing skill in the composition of literature for distribution among workers.

But at the very moment when the Marxist intellectuals were most successful in stimulating and organizing the workers, the inner logic of the movement itself was leading to a fateful separation of the two groups. Wildman dwells at length on the division of function between the workers and the intelligentsia in the SD movement. As organizations grew in size, the typical "center" came to be dominated by intellectuals, and the strain between the intellectuals of the center and the workers in the lower echelons of the organization increased. The climax of this process was the 1903 Brussells-London Congress. The triumph of "Iskraism" at that congress was the triumph of "the close fraternity of those revolutionary intellectuals who were fanatically convinced of their historical mission in the forthcoming decisive stage of the revolutionary movement." The tragedy of the congress was that it ignored and repudiated the extraordinary achievements of earlier SD intellectuals, and of vigorous and highly motivated workers in the SD movement, to build a strong working class basis for the movement:

> . . . it [the intelligentsia leadership of the SD movement] had lost the ties that had once firmly bound it to the working class . . . A certain amount of sensitivity and tact could undoubtedly have done much to ease the breach; but this would have required yet another reorientation in the psychology of Russia's wandering fraternity of social renegades. This was not to come until after the upheaval of 1905, and then only in the Menshevik camp.

For the period which he treats, therefore, Wildman stresses the similarities, rather than the dissimilarities, between future Bolsheviks and Mensheviks. For him the crucial question is the viewpoint of the intellectual leadership of the SDs toward its worker following, and by this measure the two were indistinguishable from one another, or indeed from the populists, with whom they shared a "primary commitment . . . to the mystique of revolution itself, to the vision of a faultless society purged of the anomalies of the existing order . . . ." Thus the implications of this commitment become, in the last analysis, more significant than the specific differences of political program upon which Schapiro's groupings are constructed.

The political memoirs of Paul Miliukov are surely one of the most important, as well as one of the most interesting, documents of Russian political history of the twentieth century. Professor Mendel is to be congratulated for his choice of this translation project, though not for the care with which it was executed. There are a number of obvious translation errors which should have been caught in the editing.

It was Miliukov's destiny to play a key role at two decisive turning points—the transition from the autocracy to the constitutional monarchy, in 1905-6, and the transition from the constitutional monarchy to the republic, in 1917. The book makes very useful reading in company with all the others, but perhaps especially the Schapiro volume. Miliukov would be classified as a political rationalist in Schapiro's categories, and he has often been criticized for an excessively rationalist approach to politics which his critics describe as making him too uncompromising in dealing with the tsarist government after the 1905 Revolution, or with the circumstances of 1917. Miliukov's defense of his actions on both occasions makes convincing reading. He argues that his political following would have collapsed had he cooperated with the government on Witte's terms, and repudiates Maklakov's charges that he was chiefly responsible for the failure of the crucial First Duma. On the other hand, he admits, with characteristic candor, that he underestimated the division of opinion in high governmental circles and acknowledges that retrospectively he would "ascribe a greater significance to the efforts of those

who wanted to preserve the Duma than I thought at the time . . ."

As a political rationalist, Miliukov had worked for many years to establish and strengthen parliamentary institutions in Russia. Schapiro portrays this enterprise as essentially futile:

> When the time of decision came, in 1917, the Duma proved powerless as an organ of government; and the liberals proved powerless to assert either its legitimacy or its effectiveness. In the end, therefore, the one viable and real institution—the autocracy—won the day, even though it won under entirely new management.

Looking at this quotation in the context of Miliukov's memoirs exposes certain weaknesses. The adjectives "viable" and "real" fit awkwardly; the collapse of the autocracy hardly seems an argument for its viability, and why it was more real (whatever that means) than a younger political organ which had been functioning quite vigorously for several years is certainly unclear. Surely the point is that the autocracy collapsed and that the liberals, and later the socialists, attempted to create an executive organ to replace it. That they failed in this enterprise appears, in Miliukov's memoirs, to be quite easily explainable in terms of the extraordinary circumstances of the time, a frightful paucity of effective political leaders, and a vigorous element of utopian revolutionaries whose political fanaticism was as important as the shortsightedness of the tsar in weakening the parliamentary system at its birth, and who, at least in the case of the Bolsheviks, were actively hostile to parliamentary government both in 1905 and in 1917.

Perhaps the chief difficulty with Professor Schapiro's conception is that it portrays autocracy as an institution with a life of its own. This writer, at least, interprets these events not as the persistence of the old autocracy, which in any event had ceased to be an autocracy with the introduction of the new constitution in 1905–6, but with the creation of a new Bolshevik-inspired autocracy which was built upon the wreckage of former institutions by Lenin's plan and had as little similarity to the governmental structure of Russia in 1914 as the governmental structure of Nazi Germany bore to that of the German Empire of the same year.—HERBERT J. ELLISON, *University of Washington.*

INTERNATIONAL POLITICS, LAW, AND ORGANIZATION

The UN Secretary-General and the Maintenance of Peace. By LEON GORDENKER. (New York: Columbia University Press, 1967. Pp. 380. $8.50.)

Trygve Lie, while musing on the nature of the office to which he had just been appointed in 1946, concluded that the Secretary-General of the United Nations was to be more General than Secretary; but where, he wondered, were his divisions? In large measure the history of the office from 1946 to the present may be viewed as a series of attempts to answer that challenge—attempts marked with varying degrees of success. Each of the three incumbents has sought both to legitimize the role of the Secretary-General as a participant in United Nations policy-making and execution and to expand the range of activities considered appropriate for his concern.

Leon Gordenker's work on the subject, after examining the historical background of the office, does an admirable job of surveying the developments in the United Nations relevant to each of these two themes. In doing so he has filled a large gap in the literature on the United Nations, for not since Stephen Schwebel's book on the same subject, published in 1952, has there been a major study devoted to this all important office. The work is well researched, smoothly written and characterized by thoughtful, measured judgments. As the title indicates, however, the book is confined to a discussion of those aspects of the Secretary-General's activity relating to the maintenance of peace (and within this category primary emphasis is placed on the four instances of United Nations forces deployment: Suez, the Congo, West Irian and Cyprus). The author specifically excludes consideration of the Secretary-General's activities in the economic and social realms.

Another omission, implicit but no less important, is Gordenker's incomplete treatment of those issues involving the use of force in which United Nations activity was minimal and ineffective. For example, much is made of the innovative brilliance of Dag Hammarskjold in elaborating UNTF at the time of the Suez Crisis of 1956. Yet there is no attempt to explore very thoroughly his relative inaction with respect to the Hungarian revolution which was occurring simultaneously. The author accepts the standard version that there was little or nothing that either the Secretary-General specifically or the United Nations generally could do, given the willingness of the Soviet Union to use its own troops in Hungary. Perhaps; but it remains true that during some portions of the period Nov. 1–4, 1956, when Nagy was in control of the government, that the borders of Hungary were open and that a United Nations presence might have been provided in that unhappy land. The point is not that Hammarskjold or the Security Council missed a great opportunity; the point is rather that Gordenker doesn't

explore why Hammarskjold displayed an almost polar difference in his willingness to act as between Suez and Hungary. The answer may well be the obvious; that the Secretary-General was reluctant to get involved in a Soviet Union–United States dispute. If so, this says a good deal about the kinds of issues on which the Secretary-General can make capital; and it says something about Dag Hammarskjold as the servant of his office rather than of peace.

This point illustrates my only important reservation to the study: it is covered with a patina of "the official history." While Professor Gordenker is far too sophisticated ever to write as a member of the "gee whiz" school of analysis which characterizes all too much of United Nations Association studies, it is clear that his heart lies with all three Secretaries-General, most especially Dag Hammarskjold. However judiciously stated, a central theme of the book is that whenever Secretary-General activity is expanded the world is better for it. There is nothing wrong with believing this; but the reader should be made aware that he is reading a work with normative overtones—description all right, but description with a purpose: a defense of the office, of its incumbents, and of its expansion. In sum, Professor Gordenker—as the author of this study—could just as well be a member of the Secretariat as a Princeton professor.

In somewhat the same vein, it is difficult to disagree with any of Professor Gordenker's judgments, because he is a past master of the "on the one hand this . . . on the other hand that" school of analysis. If at one point he seems to be saying the Secretary-General has no political base, at another he analyzes his changing constituencies. If personality differences affect the Secretary-General's ability to act effectively, Hammarskjold as well as Lie became the object of Soviet wrath—and, it can be argued, was saved from a similar fate only by his untimely death. These kinds of judgments can certainly be viewed as reasonable; they also leave the reader puzzled as to just what the author thinks are the most important considerations in analysing the office.

Despite these reservations, the work is critically important for any student of the United Nations. If implicit exhortation seems sometimes to smother cold analysis, this is perhaps as much the fault of the traditional way of looking at international organization as it is any particular biases of the author.—JAMES N. MURRAY, *University of Iowa.*

Systems, States, Diplomacy and Rules. By J. W. BURTON. (Cambridge: Cambridge University Press, 1968. Pp. 251. $7.00.)

The general systems theory and its derivatives have already left a lasting mark on the study of politics. They have given us a new set of terms. We owe them a new taxonomy. New hypotheses have been generated by the general systems theory. It has extended the number of verifiable propositions about political behavior.

J. W. Burton's *Systems, States, Diplomacy and Rules* is one of the latest, most complete, and most imaginative attempts by a distinguished scholar to apply the general systems theory to the study of international relations. Burton's study is an offspring of the research program of the Centre for the Analysis of Conflict at University College in London. In many ways it appears to be a theoretical companion volume to a promised empirical study of a conflict done at the Centre, and it is a great pity that the two cannot be reviewed together. Burton's new hypotheses and propositions would then, perhaps, stand up more convincingly to some elements of the traditional theories which he refutes. Even without this opportunity one has to admit that the success of Burton's effort on the frontier of systems analysis is remarkable. It would be a disservice, however, not to raise critical questions about many a proposition that he has made.

The introductory part of Burton's study presents an excellent discussion of the general systems theory. It echoes, of course, the work of systems theorists in many fields but the summary is presented here with great simplicity and elegance. The structure of Burton's own theory of international relations resembles a triangle. At the top angle rests his central proposition derived from the general systems theory: the conduct of states is preconditioned by the need of states either to adjust to changes in their environment or to alter the environment. For Burton the ideal type of world society would be "one which is fully permissive of change and never requires adjustment by a State or its systems other than that which is slow-moving and continuous" (p. 36). At one base angle of the triangle lies the concept of legitimacy in the behavior of states. It tries to account for the difference between reciprocal and co-ordinated behavior, which is systemic and functional, and power behavior, which is non-systemic and hence dysfunctional. The second base angle of Burton's structure is occupied by his theory of decision-making. Burton's new realism concentrates on those decisions that are within the capability of states. The ties among the three angles of this triangle are defined by the inner logic of Burton's theory. The needs of states can be satisfied only by legitimized behavior; decision-making "bridges the gap between legitimacy in the sense of representing the values and interests of the State, and legitimacy in the sense of acting in accordance with agreements and demands made on the State"

(p. 57); mistakes in the decision-making process, of whatever origin, lead to dysfunctional results.

What troubles me in this construct is Burton's refusal to conceive explicitly of states as systems of action. He claims with Charles McClelland that they are "better regarded as the resultant of the interacting behavior of systems," but if they have a role in world society—and Burton admits that it may be a role independent of the systems of which they consist—are they not systems, too? This is an issue which is important not only from the theoretical point of view. Burton has taken this stand obviously consciously, as the predictive and especially the prescriptive parts of his study demonstrate. But he is typically cautious and circumspect. On pp. 28–29 he would say: "States as entities do have their unique properties and needs. . . . Furthermore, one of the roles of authorities is to ensure that the State responds as a whole to environmental circumstances: responses to threats and to war are obvious examples of total response, and all systems and parts of systems are mobilized accordingly."

This is clearly the way systems behave. But this is not the way Burton wants states to behave. It is certainly theoretically productive to abandon the concept of homogeneous state behavior. Burton's accusation of the traditional approaches that they did not recognize this problem is patently unjustified. He opens himself, however, to a charge of having oversimplified the heterogeneity of role, function, and performance of individual states in the international system. Instead of analytical categories of distinction he uses those of a historical and geopolitical nature and loses his defenses against an undue intrusion of subjectivism.

When Burton deals with the practice of international relations his tendency is to exaggerate the influence that social theory has on social action. If one chooses, however, to emphasize the self-fulfilling-prophecy effect, one cannot leave out that of suicidal prediction: social theories can and do work both ways. To blame "conventional wisdom" for the failures of international practice and not to give it credit for some of the success is to present only half of the picture. And to promise that the general systems theory may emancipate diplomacy from the shackles of "conventional wisdom" and transform it from an art into a science has any meaning only if one accepts, first, Burton's argument that, inspite of what he said about conflict in Ch. 5, "reperceptions of intentions and attitudes, and the revelation of choices (i.e., conflict resolution) is possible by accurate communication, but not by bargaining and the usual processes of diplomacy" and, secondly, that while "apparent differences of interests between States present problems to be solved," "diplomacy is primarily an instrument of contest, not of problem-solving" (p. 204). But to accept those two premises would amount to seconding Burton's ultimate removal of politics from the relations among nations.

Yet, it would be unfair to end on this note. Burton's critique of past theories is radical because he is building a new paradigm. If he shifts ground, it is because the new ground is still uncertain. If he is circumspect, it is because he is pioneering. *Systems, States, Diplomacy and Rules* may make a few questionable claims but it is by far richer in stimulating hypotheses and propositions.—MOJMIR POVOLNY, *Lawrence University.*

Disarmament and Soviet Policy: 1964–1968. By THOMAS B. LARSON. (Englewood Cliffs, Prentice-Hall, 1969. Pp. 280. $6.95.)

This book is intended as a sequel to a study of Soviet attitudes towards arms control and disarmament in the Khrushchev era. (Alexander Dallin, *et al., The Soviet Union and Disarmament*) Its goal is to consider ". . . the changing character of decision making and of decision makers in the post-Khrushchev USSR, and examine trends in ideology, foreign policy, economic development, and military affairs bearing on disarmament prospects." (p. 3)

The book can be divided into two essays approximately equal in length: the first deals with trends in Soviet domestic and foreign policy; the second, with the specifics of Soviet disarmament proposals, and Soviet positions on American and other proposals. Additionally, the book contains an excellent bibliography, a chronology of events pertaining to disarmament negotiations, and an essay by Kenneth Lieberthal on Chinese disarmament policies and their impact upon the USSR.

The author contends that despite a lessening of interest in disarmament and the adoption of a "harder" line on the part of the post-Khrushchev leadership, those in power seemingly have not rejected the assumption that meaningful negotiations are possible with the United States, although they have avoided obvious displays of handholding. On balance his analysis leads him to a pessimistic conclusion as to the possibilities for future agreements.

For those concerned with the nuclear dilemma, this is of great concern since the past record is not a happy one either. Of the agreements signed to date, all have been on peripheral issues. None has affected the capacities of the states in terms of internal deployment, or development of weapons systems. The treaties concerning outer space and Antarctica affect areas of transient human habitation and uncertain military value. The partial test ban treaty and the non-proliferation treaty put no curbs on internal activities. Three of the four treaties prohibit activities which neither power has engaged in regardless of whether or not there was a

formal ban in existence. Conventional weapons are covered only in the Antarctic and outer space treaties. In short, none of the agreements has affected the strategic military programs of the two powers. These agreements have been valuable primarily for their psychological value in temporarily reducing tensions; but, on the obverse side, the negotiation of the partial agreements may have also muted enthusiasm for broader understandings.

Larson shows a marked preference for formal rather than informal or tacit agreements. He argues that informal agreements, under the press of changing and uncertain circumstances have an "easy in, easy out" quality that, although not absent from formal commitments, is present to a lesser degree in them. This preference is not without merit. Tacit agreements, being less explicit in terms of prohibitions and goals do tend to be less inhibiting. Statesmen are more constrained by formal agreements since repudiation forces a state to pay a price in terms of its future credibility. Larson does not deny that viable, tacit arrangements are possible, but feels that presently such agreements are precluded because of the extreme asymmetry in forces and objectives of the two powers.

He attacks as myth the assumption prevalent in both official reports and scholarly monographs, that the USSR wishes to freeze the military status quo. In fact, the Soviet Union has shown no interest in accepting a permanent strategic inferiority, even if such a stance seems to offer substantial economic benefits in terms of freeing resources for other more productive projects. As example, even though economic resources have been tight, the collective leadership has initiated several new and expensive military programs such as the ABM deployment. Thus, the hope of equality remains in Soviet thinking about the defense, but by means of technological breakthrough, rather than massive deployment.

In terms of interpretation, the book offers little that is new, comes to no startling conclusions, and has few ideological axes to grind. Where data is thin, unavailable, or ambiguous, Larson seldom fails to point up the deficiency. He tends to emphasize the factors that militate against progress on measures that would restrict the arms race between the two super powers in significant ways, rather than the interests that should be common and compelling reasons for agreement. What emerges is a carefully researched, well-written piece that should prove of interest to the expert as a summary, and to the layman as an introduction to the complexities of arms control and disarmament.—J. LARRY TAULBEE, *Emory University.*

Peace and the Strategy Conflict. By WILLIAM R. KINTNER. (New York: Frederick A. Praeger, 1967. Pp. 264. $6.95.)

The Weapons Culture. By RALPH E. LAPP. (New York: W. W. Norton & Co., 1968. Pp. 230. $4.95.)

Anyone interested in a balanced picture of present day conflicting strategies for American national security and their consequences is recommended to read both of these books in tandem. *Peace and the Strategy Conflict* by William R. Kintner is a persuasive example of the anti-communist marginalist school that one has come to expect of studies emerging from the Foreign Policy Research Institute at the University of Pennsylvania. Kintner sees continued peace possible only through the maintenance of not U.S. parity with, but substantial nuclear superiority over, the Soviet Union and China. He is concerned, as was the American Security Council of last year, that present policies are giving up our superiority and thus our security. Believing a meaningful detente with the Soviet Union is something of a snare and a delusion, he refuses to recognize any meaningful degree of polycentrism within the communist bloc in so far as it affects our security. Consequently, he thinks the test-ban treaty of 1963 was a mistake; he questions the non-proliferation treaty recently ratified by the U.S.; and he supports a substantial anti-ballistic missile system. While paying lip service to the concept of arms control, he fears its general acceptance would only lead to "euphoria" on the part of the American public and thus places his faith in continued efforts to increase the margin of nuclear superiority of the United States over the Soviet Union.

Ralph Lapp, addressing himself to many of the same issues in *The Weapons Culture,* takes his position at the opposite end of Robert A. Levine's marginalist spectrum—that of the anti-war middle marginalist. Accepting as he does war and not communism as the greatest danger to American security, it is no surprise that Lapp emerges contradicting Kintner at almost every point. He approves the test-ban treaty; supports non-proliferation efforts; and rejects any anti-ballistic missile defense as provocative, defensively ineffective, and contributory to an expanded arms race rather than increased security. Arms control is necessary and desirable in Lapp's opinion but has not been seriously pursued by either the United States or the Soviet Union. Conversely Kintner believes that such steps as have been taken tend only to weaken the United States. Arms control, a long range goal of Kintner, in his opinion is possible only when the Soviets turn from "institutional fabric military goals and conspirational conflict methods to moderation and respectability." This is not to be expected in the forseeable future.

These two books are excellent examples of how analysis of and recommendations for American national security diverge when written from different value bases even though the same facts are com-

mon to both writers.—CLIFFORD KETZEL, *University of Kansas.*

The Arab-Israeli Dilemma. By FRED J. KHOURI. (Syracuse: Syracuse University Press, 1968. Pp. 436. $10.00.)

From the first page of the Inroduction to the final page of the conclusions, Professor Khouri makes painfully evident his determination to write a scholarly, comprehensive and dispassionate account of the Arab-Israeli conflict. He relies heavily on UN documents and *The New York Times* dispatches for his documentation. Indeed, there is hardly an unfavorable reference to Israel which is not taken from a UN report or is not prefaced with the phrase "according to *The New York Times.*" Yet, alas, the early reception of this book suggests that the objectivity of an author bearing an Arab name and writing about the Arab-Israeli dispute will have little credibility.

That Professor Khouri's book would be received unfavorably by pro-Israeli reviewers is not surprising. Israeli spokesmen have frequently voiced their belief that UN reporting of the countless clashes of Israel and the Arabs has been unfair and this book is in large part a reflection of those reports. But the favorable reception the book has received in pro-Arab circles is revealing of the state of the Arab-Israeli debate in the United States. There is passion in this book but a passion for a peaceful settlement of the dispute through UN auspices. Those Arab and Israeli leaders who are viewed as intransigent and uncompromising are dealt with harshly. David Ben-Gurion, Moshe Dayan and Golda Meir share villain's honors with "extremist" Syrian and Palestinian leaders and frequently with Nasser. Moshe Sharett and Levi Eshkol are pictured favorably along with King Hossein and King Feisal. In other words, the heros of both Israeli and Arab nationalism are condemned. In viewpoint Professor Khouri is very close to Uri Avneri whose *Israel Without Zionists* must count among the least popular books in the Zionist world. That, in contrast, Khouri should be favorably received by pro-Arab groups and his book placed on their recommended reading lists simply reflects the fact that of the countless books printed on this subject by American authors, only a handful treats the aspirations of the Arab people with any sympathy as does this book.

There is little doubt that the reader searching for bias in this book, as in any other on this subject, will find it. But whatever bias may be found is minor. Professor Khouri strives for and to a remarkable extent achieves balance. This book is in no sense a polemic. Yet it is heavily judgmental. Men and events are weighed against the type of behavior which could have produced a peaceful solution and judged when found wanting. In a different book this judgmental quality could be both interesting and valuable as in Christopher Sykes' *Crossroads to Israel.* Here it is somewhat defeating of the real value of the book: a faithful chronicle of events of the Arab-Israeli conflict especially as concerns the role of the United Nations. At the very least *The Arab Israeli Dilemma* stands as a valuable reference work for the student of the Middle East. But the effort to supplement careful description with analysis is subverted by the tendency to judge.

Given his objectives, Professor Khouri needed to construct a picture of the milieu of the Arab-Israeli conflict. Obvious analytic objectives would be an examination of Arab and Israeli nationalisms, a differentiation of Arab and Israeli mass and elite attitudes, and an exploring of Soviet and American Cold War behavior as it related to the Middle East. There is little point in judging Arab or Israeli elite behavior if the area of freedom of action has not been explored. Professor Khouri deals only obliquely with Arab and Israeli nationalisms. His repeated references to King Hossein as being "moderate" implies an attitudinal continuum, a highly debatable point and one never explored. Arab public attitudes are commonly treated as monoliths with the "anti-Israeli Arab masses" assertion a frequent one. Soviet motivations are asserted, never analytically developed, and the assertions are usually in the unreconstructed Cold War mold. This is particularly unfortunate with regard to the pro-Israeli Soviet period in 1947–1950. The assumption here as elsewhere that this was a sophisticated effort to achieve instability in the Middle East badly needs reexamining.

The tendency to judge is a common one in the literature of this period of Middle East history. In this case it detracts from what is otherwise a careful and comprehensive scholarly work.—
RICHARD W. COTTAM, *University of Pittsburgh.*

Mathematical Models of Arms Control and Disarmament: Applications of Mathematical Structures in Politics. By THOMAS L. SAATY (New York: John Wiley & Sons, Inc. 1968. Pp. 190. $11.95)

Thomas Saaty's *Mathematical Models of Arms Control and Disarmament* is a short but highly substantive book which focuses on real world problems relevant to arms control and disarmament from a mathematical perspective. The author aims to show that, contrary to the opinion of many students in political science, arms control and disarmament problems, although highly complex and seemingly insoluble scientifically, are potentially amenable to scientific analysis and ultimate solution through the employment of analyti-

cally tractable mathematical models: particularly game and decision models.

To realize his aim the author proceeds in a fashion which is characteristic of operations research. He begins by dimensionalizing arms control problems into three phases: (1) the selection of arms control objectives and their consistency; (2) the adoption of arms control policies and their stability—the most significant topic covered by Saaty; and (3) the choice of effective arms control actions to carry out policies and to fulfill objectives. Correspondingly he orders *Mathematical Models in Arms Control and Disarmament* into four self-contained units, of which the first three are presentations of game and decision theory models for the analysis of the respective three dimensions of arms control. The fourth unit is a current overview of the literature of international conflict and arms control research.

Structurally the individual units are autonomous, each having three components: (1) an operational definition of the particular phase of arms control under consideration which is translatable into a formal mathematical analysis; (2) a review of mathematical techniques relevant to the phase of arms control problems being analyzed; and (3) selected illustrative applications of the models and their derivations to real world cases.

More specifically, Part One is a single chapter unit devoted to the analysis of nations' arms control objectives and the measurement of their consistency. A graph theory model utilizing the "logic relations" technique of Joxe, (1964) which measures the consistency of foreign policy objectives is restated succinctly and clearly by Saaty and applied in an illustrative fashion to test the consistency of the West German foreign policy objective of re-unification.

Part Two is a multi-chapter unit devoted to a quantitative analysis of deterrence, arms control and arms race situations and the measurement of nations' foreign policy stability therein. The first chapter begins with the formulation of a dynamic equilibrium criterion for the measurement of policy stability. Mathematical models for the analysis of policies in deterrence, arms reduction and arms race situations are then proposed by Saaty. For stable deterrence situations Saaty proposes a probabilistic model in which stability or equilibrium is a function of the ratios of probability of gains with war to gains from successful deterrence for two or more competitors. For arms reduction Saaty proposes a graph theoretical model which formalizes the present arms level and the process of reducing arms from this initial level to a new negotiated level. For the analysis of policies in arms races the Richardson model is restated with its elaboration by Caspary to include economic constraints. The chapter is concluded with an illustrative application of Intriligator's dynamic model of a missile war (1967).

The second chapter in Part Two focuses on the means for determining optimum strategies for coordinating policies between two, or more, nations leading from conflict situations to arms control situations. Within this framework Saaty presents one of the more penetrating and extensive reviews of the game theory literature. The review, although comprehensive, suffers from a major structural defect in that the author's attempt to compress and consolidate the game theory literature into a relatively short and coherent restatement implicitly assumes a high level of familiarity with the mathematics of game theory; its comprehension will be difficult for the non-mathematically oriented political science audience. The second chapter of Part Two is concluded by the illustrative application of the meta-game model to the problems of nonproliferation and Vietnam respectively.

The third and final chapter of Part Two focuses on the negotiation process by which agreements on policies are arrived at between two conflicting parties who negotiate rather than resort to war to resolve their conflictual interactions. Saaty represents the process of negotiation as a cooperative game for which a Nash type approach is posited as the means for conflicting powers to avoid threat escalation and consequently to arrive at mutually acceptable agreements.

Two types of incomplete information games (Auman Maschler 1966) are then posited by Saaty as models for the analysis of the negotiation process in which the two or more negotiating parties have incomplete information as in arms control and disarmament situations. The first game type is an infinite stage or recursive zero-sum game in which the opponent is known but the payoff matrix is unknown. The second game type is a one-shot game, which need not be zero-sum, in which the payoff matrix corresponding to each opponent is known but the opponent is unknown.

Part Three, the last of the three substantive units, is a single chapter unit devoted to analysis of the choice of effective actions to carry out arms control policies and objectives. In this regard Saaty operationally defines the criterion for effective arms control actions as the assurance that the provisions of an arms control treaty are being adhered to. He then restates a number of quantitative models that appear to provide this assurance. A Bayesian decision theory model and other game theory models are presented which enable the estimation of the probability of causes of the violation of arms agreement, given apparent effects of the violation. The unit is concluded with a brief

comment on the of traditional sampling techniques for ensuring conformity to arms agreements.

Part Four, the least formal in the volume, is also a single unit chapter devoted to an overview of the recent arms control and international conflict literature. The review is sketchy in nature and lacks internal structure. Fortunately, Saaty supplements Part Four with a comprehensive reference list which follows the final unit and which is excellently cross-referenced in the text.

Mathematical Models in Arms Control and Disarmament is an important work because it provides a succinct, substantive and articulate argument for harnessing the power of applied mathematical models—of the non-linear type—for the analysis of complex political problems. In this regard Saaty succeeds in showing that there is a great deal to be learned by political scientists from the application of mathematical game and decision techniques to international conflict situations in both the scientific and policy dimensions. In doing this, however, he does not shirk his responsibility of revealing the present inadequacies of game theory and other abstract mathematical techniques for explaining the ever complex reality of the social science universe. He also indicates those areas in which he thinks there is a need for refinement of mathematical techniques to make them more effective tools for the enhancement of our analytical capabilities.

The Saaty volume, however, suffers from two weaknesses. First, almost all of Saaty's models involve the use of non-linear equations and inequalities. Although such models may provide more realistic descriptors of social and political systems than the more idealized linear models currently in use, the techniques for solution of non-linear equations are extremely complex and are in large measure beyond our present computing machinery capacity.

Second, Saaty presents a fairly large number of mathematical approaches to the study of the various dimensions of conflict. However, he fails to plot out, even if only in a preliminary fashion, the steps by which to begin arranging the techniques in order to enable the formation of a general theory of conflict and correspondingly of integration in the future. It would seem that in this regard, chapter four should have been devoted to pointing the way social scientists should be going in this area rather than a mere impressionistic review of the literature.

On balance, the present reviewer feels that Thomas Saaty's *Mathematical Models for Arms Control and Disarmament* makes a significant contribution in the area of international relations to the fledgling but quickly developing body of mathematical social science literature. It attempts to bridge the gap between political science and applied mathematics in a realistic way and indicates how applied mathematics may be used by social and political scientists as it is used by the engineer today, as an effective analytical tool. Moreover, it paves the way for similar subsequent review studies of applicable mathematical model in other areas of political science.—SAMUEL J. BERNSTEIN, *The University of Rochester.*

Peacekeeping: International Challenge and Canadian Response. By ALASTAIR TAYLOR, DAVID COX, AND J. L. GRANATSTEIN. (Toronto: Canadian Institute of International Affairs, 1968. Pp. 211. $2.75, paper.)

This book by three Canadian scholars examines Canada's commitment to international peacekeeping. In the first of four sections, Alastair Taylor discusses the "international context" of peacekeeping and observes that peacekeeping has been closely tied to colonial-related, predominately intra-state, conflicts. He believes that such conflicts are almost past and that future peacekeepers will be concerned with inter-state convicts among small, relatively unstable, regimes. He speculates that small states in conflict might be reluctant to cooperate with peacekeepers where their own, no longer anti-colonial, interests are involved and, under such circumstances, great powers might find it advantageous to utilize UN Charter peace preserving methods through the Security Council.

Taylor also suggests that protagonists might prefer regional peacekeeping arrangements to a wider international (UN) peaceforce. He fears that regional orientation would do more harm than good, although it cannot be discounted entirely because of previously demonstrated "susceptibilities, prejudices, and preferences." While this is true, there are considerations which extend well beyond susceptibilities. For example, how can small power regional military requirements be reconciled with the international perspective peculiar to UN peacekeeping? Taylor touches on these matters all too briefly. Canada's role in this is not clear either; it may try to mediate regional versus international perspectives, or it may simply be left out if UN peacekeeping gives way to regionally dominated solutions. What Taylor calls a second, post-colonial-related, phase of peacekeeping might really be an end to it altogether if the great powers are willing to assert their collective authority through the Security Council. Given current political and ideological divisions among them, this is not likely. If small power intransigence and regionalism prevail, however, then the prospects for peacekeeping are bleak indeed.

In two following sections, David Cox discusses

political and military aspects of Canadian peacekeeping and he reports on discussions held at the Queen's University-CIIA Conference on Peacekeeping held in Ottawa in 1967. In his first article, Cox examines Canada's problems and frustrations in maintaining a credible detachment from US policy while remaining committed to Western interests. Canada has achieved considerable success in this as indicated by its wide acceptance as a peacekeeper. However, peacekeeping creates a dilemna vis a vis NATO and NORAD and a solution was sought in the 1964 White Paper on Defense. In it, Canada's alliance obligations were reaffirmed while greater flexibility and mobility were provided for the armed forces. This should permit an easier response to peacekeeping calls from the UN wihout undermining present alliance commitments.

Cox also points out the importance of US political and financial support for peacekeeping and rightly indicates that peacekeeping is especially valuable for mitigating conflict in the non-Communist world. In fact, after the demise of UNEF, it appears that there are few prospects for peacekeeping in any other context.

The Peacekeeping Conference report yielded several good ideas; an especially interesting one, specifically directed at Canada, was the admonition that Canada looks like a great power to many developing peoples. Canadians were advised to avoid the appearance of "great power chauvinism" in their peacekeeping posture. This idea is probably remote from most Canadian minds.

The last section, by J. L. Granatstein, is an overview of Canadian peacekeeping experience. The reader who is not acquainted with this history will be impressed by the continuity of Canada's commitment since 1946. Covering so much ground in so brief a section leaves the political analysis a bit thin; nevertheless, basic differences in the approaches of the Liberal and Conservative governments are noted. Granatstein observes that public pressure forced Conservative Prime Minister, John Diefenbaker, to send Canadian peacekeepers to the Congo in 1960. While Conservative hesitation was a fact, the effect of public opinion is less well documented. There was relatively little public or Parliamentary debate at the time and if the Liberal opposition placed great pressure on the government, it did so in private. Granatstein also examines Egypt's objection to the use of Canada's British-sounding "Queen's Own" battalion for UNEF duty in 1956. This upset Canadians who had been so active in the formation of UNEF. Granatstein discounts the popular theory that Nasser's offended sensibilities motivated his rejection of Canadian troops; rather, he says that Nasser interpreted Canada's November 1 abstention in the General Assembly (done in light of its peaceforce proposal which followed) as equivocation based upon ties with Egypt's attackers, Britain and France. In essence, Nasser questioned Canada's neutrality and integrity with regard to UNEF. Eventually, he accepted Canadian administrative troops instead of infantry and Canadians convinced themselves that the matter was primarily technical.

Granatstein holds hope for the continuance and usefulness of peacekeeping and discounts recent criticisms of the policy. However limited the prospects, he believes that peacekeeping can calm some troubled waters and implies that in a nuclear world any contribution to calm is better than none at all. Peacekeeping is within the means of Canada to perform well—a similar effort added to US armed might would not be noticed. It is seen as a workable policy for one important smaller power which is trying to alter its external environment in order to create a mode favorable international climate. As such, this process has implications which extend well beyond Canada.—DONALD E. MILSTEN, *University of Maryland, Baltimore County.*

Middle East Politics: The Military Dimension. BY J. C. HUREWITZ. (New York: Frederick A. Praeger, 1969. Pp. 550. paper, $3.95.)

Professor Hurewitz' study is "an inquiry into the causes and consequences of military intervention in the postwar politics" of eighteen separate states which constitute the region of the Middle East and North Africa. (The author prefers to denote this entire region as the Middle East.) He views the army's intervention in politics as the function of many interplaying factors such as historical, international and domestic change.

In his introduction the author defines his method of studying the military role. In rejecting the use of a general theory, he argues that the existence of diversified political systems in a wide region like the Middle East can not be easily managed by a universal theory. It is for this reason that the present reviewer doubted, even before reading the book, not the validity of a general theory circumscribing the military role in politics, but whether the study of the military role in eighteen countries, discussed country by country, could be managed effectively in one volume and, needless to say, by one scholar.

The way that Professor Hurewitz discussed the army role in each country produced little of significance for the study of comparative politics. Unfortunately, these chapters were for the most part descriptions of political events in the last two decades. True comparison between countries was relatively lost because the style of discussion varied from one country to another. The author was often found discussing the historical development of a

political system with little success in linking such development to the present role of its army. Treatment of other countries such as Iraq and Syria—the only two countries discussed in one chapter—was mainly confined to the discussion of the causes of the political instability of their regimes.

In one of his concluding chapters, Hurewitz argues that there is little relationship between military modernization and social change and, at least in the Middle East, army rulers are not social revolutionaries, (Chapter 23). Support of such a hypotheses cannot be established simply by the comparison of the performance of military regimes vs. civilian regimes. Correlations between the army and modernization can only be established when we view the army as a social institution interrelated with other institutions in the society; this relation did not receive more than "lip service" from the author. Further, too often in our study of change we confuse military leadership and military establishment. Military leaders who control the government must be good conspirators, but are not necessarily the best political leaders that the army can provide. The retardation of modernization under the military regimes in Iraq and Syria is attributed in part to poor quality of leadership rather than to the desire of the military rulers to keep their communal value above the national value—the theme of the author's argument (p. 428). If this is true then why do we see the army's officers often losing their own communal support?

The truth of the matter is that army organization creates cohesion among officers from various communities who work to protect their own organizational interests; but not necessarily completely suppressing their communal values. It is also correct to argue that army officers in order to keep control over governmental structure need the support of different communities, and for this purpose they must enlarge the basis of political recruitment to other groups. If the Syrian and Iraqi regimes are dominated by a certain communal class, this is because the rulers came into a governing situation which was not of their own creation. Once the military regime establishes a strong grip on power it tends to move away from its own narrow constituency. Thus, it is significant to analyze patterns of change in military regimes over a defined period of time. The regimes of the Arif brothers (1964–68) was heavily dominated by Sunni Arabs in the first year, but it gradually incorporated a substantial Shiit element in government positions.

When all this is said, one may infer that rigorous investigation and more socio-economic data are needed to define the military relationship to politics. The "dimension" of the army role is not judged alone by the increasing number of the armed forces or the augmentation in defense appropriations; both of which the author emphasised. Other indices of army organization such as increasing specialization and differentiation of roles and autonomy, because of both their social and political implications, seem to be far from the author's thinking. Role-expansion of the army, however, depends among other things on the development of other institutions such as bureaucracy, party system, and on the degree of institutionalization of other social functions. These interrelated relations were central to Hurewitz' thinking when he wrote his two chapters on the Sudan and Tunisia, but he did not search for them in his studies of other countries. In Tunisia the author did not fail to recognize the role of the Neo-Destour Party in containing the army from encroachment on the civilian government, and in the case of the Sudan, the "high standards of the civilian bureacracy" (p. 163), to which is attributed the avoidance of military radical policies. Unfortunately, the Sudanese bureaucracy's role was not fully explained.

With the exception of these remarks, the author has written a very informative and erudite account of the military politics in the Middle East, which deserves attention from those who have interests in military authoritarianism. The author is to be commended for his search for data and information which this reviewer knows is very difficult to find, on a region which has become addicted to violence.—ABDUL H. RAOOF, *SUNY at Buffalo.*

Alliances and American Foreign Policy. By ROBERT E. OSGOOD. (Baltimore: The Johns Hopkins Press, 1968. Pp. 171. $6.50.)

With the advent of polycentrism, the traditional subject of alliances is receiving new attention. Catalzying this interest has been the invocation of alliance commitments to justify large scale military operations in Southeast Asia. However, this rationale has been so forcefully challenged by domestic dissenters that the government must reappraise its alliance policies as part of an overall reconsideration of United States foreign policy.

Robert E. Osgood, Director of the Washington Center of Foreign Policy Research offers his book on alliances (which "originated in a group study conducted by the Center for the United States government") as a clarification of the subject as well as to provide suggestions as to their future role in American foreign policy.

Historically, alliances were the primary instruments by which states adjusted to the fluctuations of balance-of-power politics. In this task, alliances performed four functions: "accretion of external power, internal security, restraint of allies, and international order." Each state would emphasize those functions which were most relevant to its own psychological and military situation. But the

priority previously accorded alliances has diminished in the postwar period as other forms of commitments, such as foreign aid, unilateral declarations, and base agreements, have provided greater flexibility in performing these functions.

Osgood explains both the declining importance of alliances and their continued use by developing the concept of preponderance (or empire). He assumes that because of its dominant military power, conjoined with the goal of a stable international order, the United States will continue to intervene abroad but in a manner more circumspect than, for example, in Vietnam. The current functions of several alliances, when viewed in the perspective of preponderance, indicate that American policies differ from region to region depending on the types of conflict prevalent. Thus NATO (and the Warsaw Treaty Organization as well) gives priority to the functions of intra-allied control rather than to deterrence. The latter function is performed by the existing two-state balance of power, leaving the superpowers free to deal with their allies' increasing pressures for independent policies. This situation is described as "political polycentrism within a framework of military duopoly."

In other parts of the world, resistance to communist control still is of primary concern to the United States. But because of the lack of consensus on security interests, the growth of indigenous nationalism and the increasing range of Soviet weaponry, reliance has been placed on commitments more informal and flexible than alliances. The role of alliance organizations has been to provide American access to particular governments and the opportunity to influence them. Osgood concludes that a kind of balance of power has been attained between the United States and the Soviet Union, but that China's unclear intentions may require further extension of American commitments.

A major portion of the book concerns the future of alliances. On the premise that the "changing structure of power and pattern of conflict will have a decisive effect upon the nature and role of alliances in the future." Osgood considers several alternative structures of power. Selecting one, not unlike the current situation, he speculates on the roles which alliances will likely play in American foreign policy. Technological developments (overseas transport capabilities, nuclear proliferation, and antiballistic missiles) are assessed for their impact upon this projected structure.

Three difficulties lessen the value of Osgood's work, all traceable in some degree to the restricted scope of his study. The first is reluctance to question the assumptions upon which America's foreign policy rests. Should the United States press for a stable international order? Need it enter alliances in order to ensure influence over its allies?

A second difficulty relates to the author's style of combining the formalisms of alliances with impressionistic observations of contemporary events. The conclusions drawn from this combination tend to be more platitudinous than, as Osgood calls them, speculative. For example, Osgood reminds us that, "The United States will have to make up for its reduced opportunity for direct leadership by opportunistic maneuvering in an increasingly . . . fluid field of power and interests." (p. 14) And, "The obligations and responsibilities entailed in various commitments may become more limited and flexible." (p. 159) Again, "ABM's seem likely to strain relations between the United States and its allies, but American ABM policy can mitigate the strain." (p. 154)

The third difficulty is common to all who try to generalize from recent events. The book was written before the Soviet occupation of Czechoslovakia, before the bombing of North Vietnam was halted and before Arab-Israeli military activities has resumed. There is little doubt but that these events would require the author to modify several of his evaluations.—JEROME J. HANUS, *The American University.*

Cohesion and Conflict in International Communism. By PETER MAYER. (The Hague: Martinus Nijhoff, 1968. Pp. 256. 34.20 guilders.)

In contrast to the Western European alliance system, regarding which a large body of literature exists, the Soviet alliance system has been somewhat neglected in recent years. No doubt this is partly the result of research problems familiar to all observers of communist affairs: the paucity of hard data; the presence of a perplexing ideological factor; the traditional separation of Soviet, East European, and Chinese fields of study. It may also reflect the lingering assumption that the uneven distribution of power in the communist camp makes the term "alliance" a misnomer. There is, of course, a massive inequality of power in the Western alliance also, but there, at least, several member states exercise independent control over their own nuclear weapons, a fact which generates a need for consensus beyond what is evident in the Soviet alliance.

The difficulties of the task invite careful attention to any work which proposes to resolve them. Professor Mayer attempts not only to explain the sources of unity and disunity in relations among communist states, as the title suggests, but sets for himself a number of additional tasks: to relate Marxist-Leninist theory to the problems of intra-bloc relationships; to explain the role of ideology in limiting policy choices available to current Soviet leaders; to clarify some contemporary political concepts, such as "power," and apply them to

communist relations; and to examine the problem of creating international decision-making agencies capable of reestablishing unity in the communist world. It is an ambitious task, and one which, in a number of respects, exceeds what has been accomplished in this book.

In his attempt to establish the causes and sources of disunity in the camp, Mayer identifies several related factors. One is an organizational factor, involving basic questions of control both within the CPSU and in the party's relations with other groups. The Bolshevik-Menshevik split in 1903 over the nature of the party provided the setting in which "the various factors which currently defy communist unity attempts first emerged." (127) Mayer argues that the major contemporary obstacle to unity in the camp lies in this organizational principle—that the movement must be monolithic and that the interests of the Soviet Union must be preeminent.

Ideology provides a second cause of disunity, but it is not clear just how important a factor it is. Mayer views communist ideology as inherently disunifying because of the contradictions existing among its basic doctrines, in particular the contradiction between "socialism in one country" and "proletarian internationalism." Yet most of the weight of his explanation falls on a third source of disunity: nationalism. In Eastern and Central Europe, recent years have seen "the abandoning of Marxism-Leninism by the party-states, who substituted policies based on national interest for the previously pertaining internationalism of the communist movement." (144) In the case of Chinese-Soviet relations, the additional factor of culture is involved, but Mayer makes contradictory statements concerning its importance. At one point he states that ". . . the differences between U.S.S.R. and Chinese communism derive largely from the cultural heritages which have helped shape the two nations." (56) Elsewhere he rejects the cultural analysis presented in Klaus Mehnert's *Peking and Moscow* as unacceptable. (108n)

Mayer states his preference for the organizational, or in his terms, "institutional," explanation, yet the analysis provided in the bulk of the book shifts back and forth among the various causes of disunity, generally failing to make a clear case for any one of them. Nor does the mere identification of several causal factors constitute an explanation. We are left then with multiple sources of disunity, and little guidance in weaving them into a convincing, multi-faceted explanation of communist inter-state relations.

The expository sections of the book are heavily documented and contain a great deal of useful information and some perceptive insights into communist bloc affairs. They include also, however, a fair number of intriguing, but unsubstantiated, conclusions about communist relations concerning which one could have wished for a bit more evidence. He states, for example, that Hungary now has "far greater independence" from the USSR than has Poland (179); that "Stalinist oriented foreign policy precepts . . . resulted in a series of unmitigated failures after the communist coup in Czechoslovakia" (226); and that Soviet controls over Eastern Europe were "dismantled" after 1953, while "the Soviet Union retreated step by step to popular indigenous demands for greater political, economic, and cultural autonomy. . . ." (227)

It is disappointing in a book on this subject to find the question of the sources of Soviet power over other communist states treated only in a scattered and indirect way. Since 1956, Mayer argues, Soviet power over the bloc countries has been "political" rather than "military," but these concepts are not clearly defined, and are included in the book almost parenthetically (pp. 124–5) in the midst of a discussion of the Sino-Soviet dispute. In any case the recent Soviet invasion of Czechoslovakia has not done much to fortify a purely political explanation of Soviet-Eastern European relations.

The author has succeeded in gathering together, in the text and in countless footnotes, much of the relevant information concerning recent relations within the communist camp. But the task of organizing and evaluating the data and extracting a convincing analysis of these relations remains to be done.—D. Richard Little, *Northern Illinois University.*

ANNOUNCEMENTS

The sixty-sixth Annual Meeting of the Association will be held September 8–12, 1970, at the Biltmore Hotel, Los Angeles, California.

ERRATA

In F. M. Barnard's article on "Culture and Political Development: Herder's Suggestive Insights," published in the issue of June, 1969, line 18, p. 10 should read: "But it is equally fair to presume. . . ."

The bibliographical statement on p. 545 of the June, 1969 issue incorrectly identified the publisher of Charlotte Beradt's *The Third Reich of Dreams* as the University of Chicago Press. The book was published by Quadrangle Books, Inc., of Chicago.

ARTICLES ACCEPTED FOR FUTURE PUBLICATION*

December, 1969

James W. Clarke, Florida State University, "Environment, Process and Policy: A Reconsideration"

Philip E. Converse and Warren E. Miller, University of Michigan, "The 1968 Elections"

Jeff Fishel, American University, "Party, Ideology, and the Congressional Challenger"

John G. Gunnell, State University of New York, Albany, Arthur S. Goldberg, University of Rochester, and A. James Gregor, University of California, Berkeley, Symposium on "Explanation, Deduction, and Social Scientific Inquiry"

Alex Inkeles, Harvard University, "Participant Citizenship in Six Developing Countries"

Edward J. Mitchell, Institute for Advanced Study, "Some Econometrics of the Huk Rebellion"

John E. Mueller, University of Rochester, "Voting on the Propositions: Ballot Patterns and Historical Trends in California"

James C. Scott, University of Wisconsin, "Corruption, Machine Politics, and Social Change"

Michael J. Shapiro, University of California, Berkeley, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives"

W. Phillips Shively, Yale University, "Ecological Inference: The Use of Aggregate Data to Study Individuals"

Sheldon S. Wolin, University of California, Berkeley, "Political Theory as a Vocation"

March, 1970

Joel D. Aberbach and Jack L. Walker, University of Michigan, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan"

Barry Ames, Stanford University, "Bases of Support for Mexico's Dominant Party"

Michael Haas, University of Hawaii, "International Subsystems: Stability and Polarity"

Howard D. Hamilton, Bowling Green State University, "Direct Legislation: Some Implications of Open Housing Referenda"

John E. Mueller, University of Rochester, "Presidential Popularity from Truman to Johnson"

Paul F. Power, University of Cincinnati, "On the Grounds for Civil Disobedience within American Democratic Thought"

Kenneth Prewitt, University of Chicago, "Political Ambition and Democratic Accountability"

William H. Riker, University of Rochester, "Rational Behavior in Politics"

Patrick Riley, Harvard University, "A Possible Explanation of Rousseau's General Will"

Stanley Rothman, Smith College, "Barrington Moore and the Dialectics of Revolution"

* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

Martin Shubik, Yale University, "Voting, or a Price System in a Competitive Market Structure"

Duff Spafford, University of Saskatchewan, "The Electoral System of Canada"

June, 1970

Steven J. Brams and Michael O'Leary, Syracuse University, "A Mathematical Model of Voting Bodies"

Steven R. Brown and John D. Ellithorp, Kent State University, "Emotional Experiences in Political Groups: The Case of the McCarthy Phenomenon"

Walter Dean Burnham and John D. Sprague, Washington University, St. Louis, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania, 1960–1968"

William R. Caspary, Washington University, St. Louis, "The 'Mood Theory': A Study of Public Opinion and Foreign Policy"

Aage R. Clausen and Richard B. Cheney, University of Wisconsin, "A Comparative Analysis of Senate and House Voting on Economic and Welfare Policy, 1953–1964"

Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, Carnegie-Mellon University, "An Expository Development of a Mathematical Model of Policy Formation in a Democratic Society"

Ada W. Finifter, Michigan State University, "Dimensions of Political Alienation"

Bryan R. Fry and Richard Winters, Stanford University, "The Politics of Redistribution"

M. Kent Jennings, University of Michigan, and L. Harmon Zeigler, University of Oregon, "The Salience of American State Politics"

Paul E. Peterson, University of Chicago, "Forms of Representation: Participation of the Poor in the Community Action Program"

David E. Powell and Paul Shoup, University of Virginia, "The Emergence of Political Science in Communist Countries"

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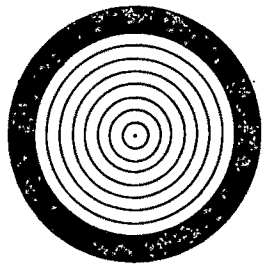
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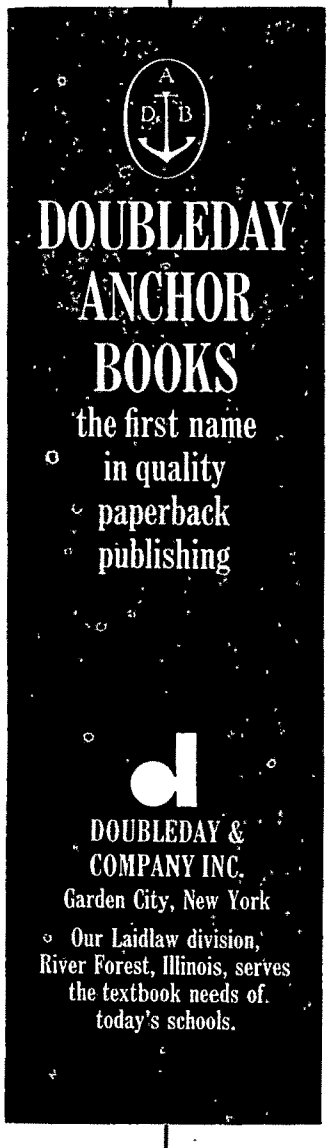
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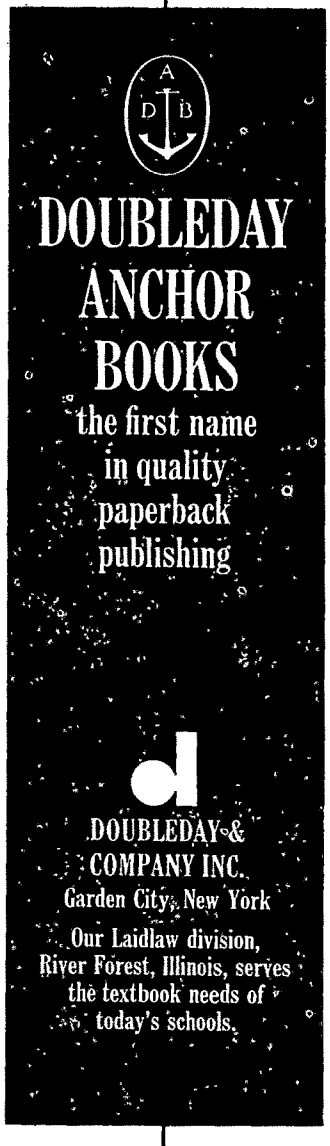
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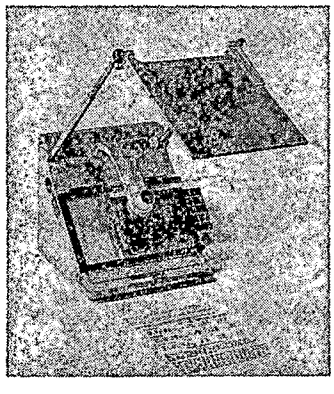
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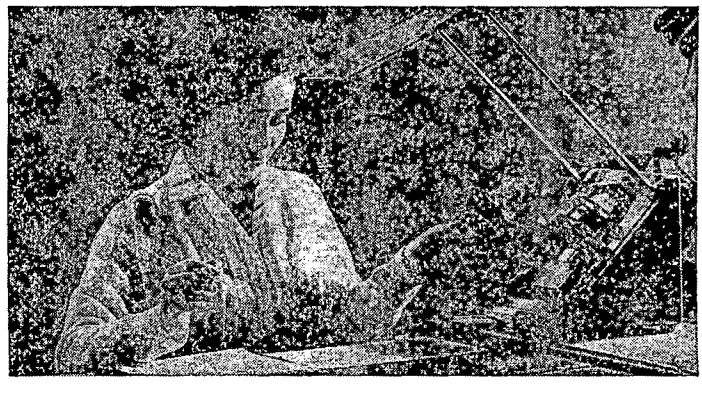
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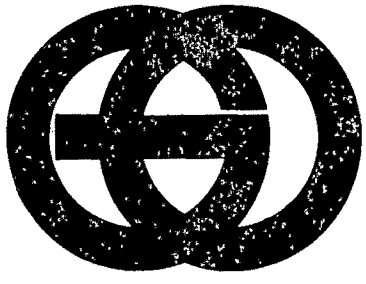
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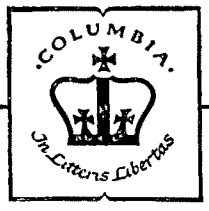
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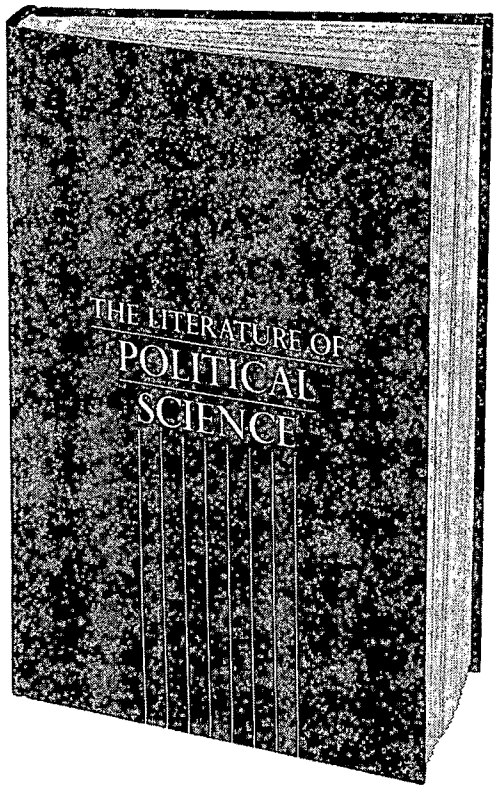
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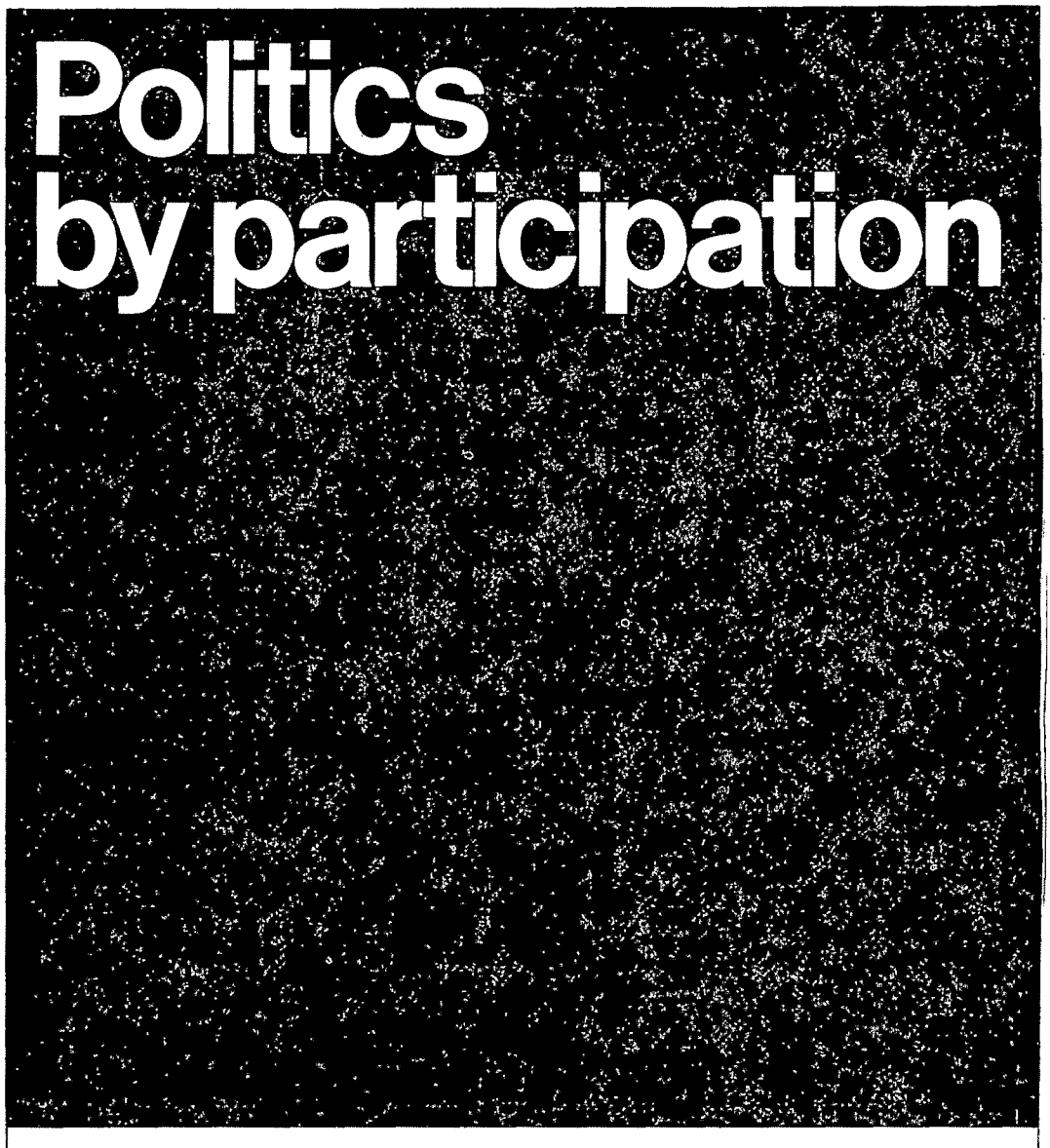
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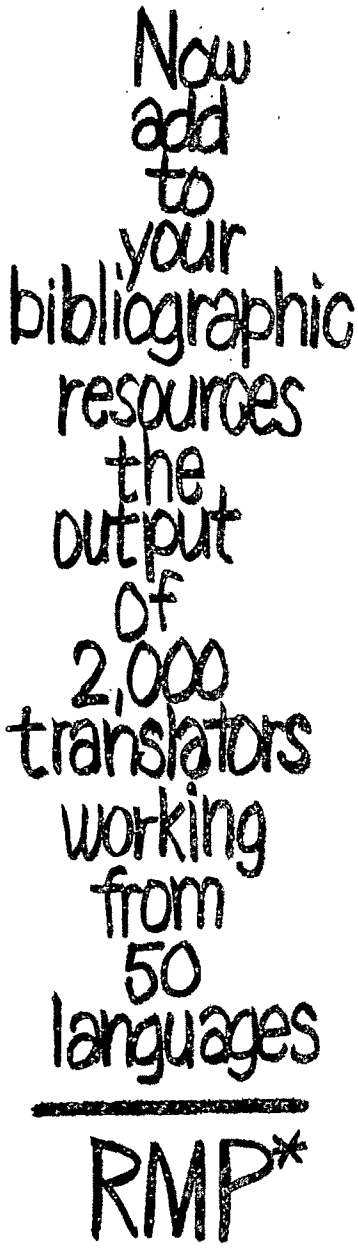
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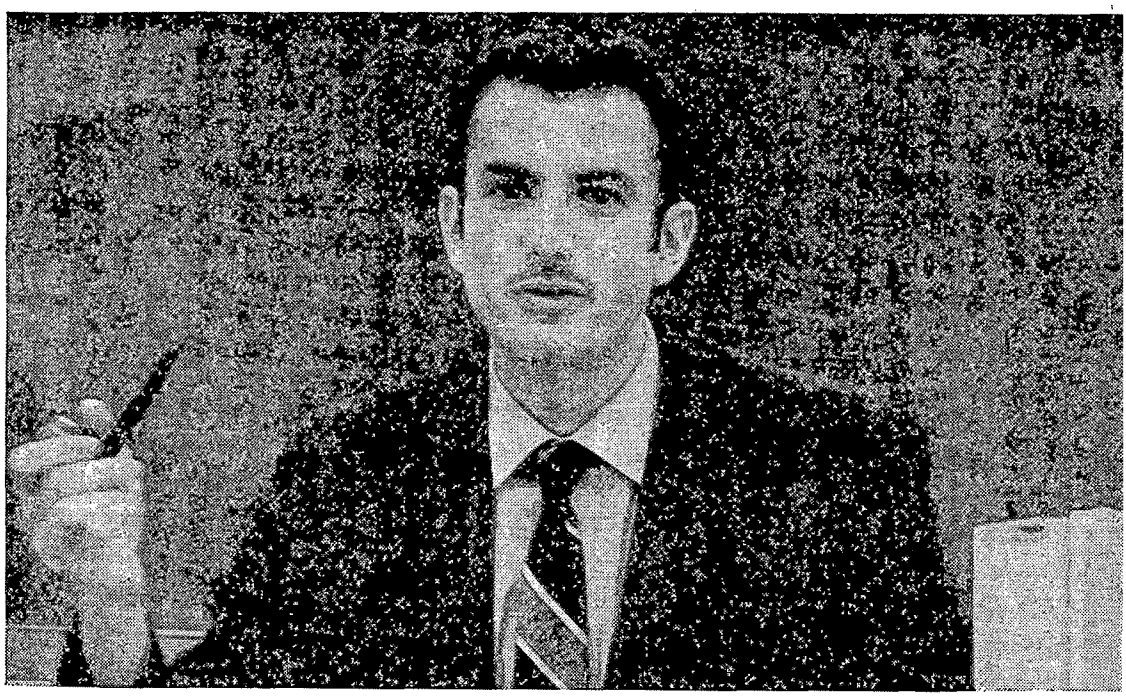
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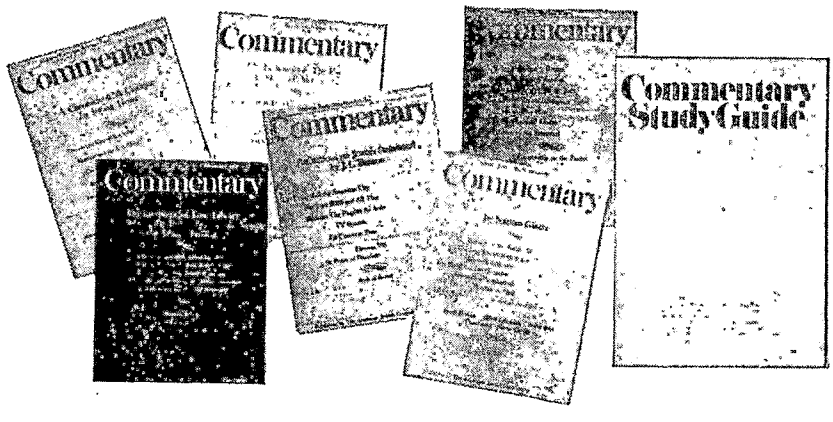
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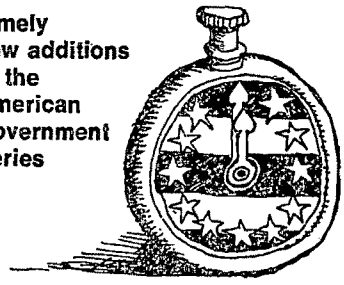
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The American Political Science Review

VOL. LXIII DECEMBER, 1969 NO. 4

THE NEW REVOLUTION IN POLITICAL SCIENCE*

DAVID EASTON
University of Chicago

A new revolution is under way in American political science. The last revolution—behavioralism—has scarcely been completed before it has been overtaken by the increasing social and political crises of our time. The weight of these crises is being felt within our discipline in the form of a new conflict in the throes of which we now find ourselves. This new and latest challenge is directed against a developing behavioral orthodoxy. This challenge I shall call the post-behavioral revolution.

The initial impulse of this revolution is just being felt. Its battle cries are *relevance* and *action*. Its objects of criticism are the disciplines, the professions, and the universities. It is still too young to be described definitively. Yet we cannot treat it as a passing phenomenon, as a kind of accident of history that will somehow fade away and leave us very much as we were before. Rather it appears to be a specific and important episode in the history of our discipline, if not in all of the social sciences. It behooves us to examine this revolution closely for its possible place in the continuing evolution of political science. Does it represent a threat to the discipline, one that will divert us from our long history in the search for reliable understanding of politics? Or is it just one more change that will enhance our capacity to find such knowledge?

I. NATURE OF THE POST-BEHAVIORAL REVOLUTION

The essence of the post-behavioral revolution is not hard to identify. It consists of a deep dissatisfaction with political research and teaching, especially of the kind that is striving to convert the study of politics into a more rigorously scientific discipline modelled on the methodology of the natural sciences. Although the post-behavioral revolution may have all the appearances of just another reaction to behavioralism, it is in fact notably different. Hitherto resistance to the incorporation of scientific method has come in the form of an appeal to the past—to classical political science, such as natural law, or to the more loosely conceived non-methodology of traditional research. Behavioralism was viewed as a threat to the status quo; classicism and traditionalism were responses calculated to preserve some part of what had been, by denying the very possibility of a science of politics.

The post-behavioral revolution is, however, future oriented. It does not especially seek to return to some golden age of political research or to conserve or even to destroy a particular methodological approach. It does not require an adherent to deny the possibility of discovering testable generalizations about human behavior. It seeks rather to propel political science in new directions. In much the same way, behavioralism in the 50's, by adopting a new technology, sought to add to rather than to deny our heritage. This new development is then a genuine revolution, not a reaction, a becoming, not a preservation, a reform, not a counter-reformation.

Post-behavioralism is both a movement, that is, an aggregate of people, and an intellectual tendency. As a movement it has many of the diffuse, unstable, even prickly qualities that the behavioral revolution itself once had in its own youth. It would be a serious mistake, indeed, a grave injustice, to confuse this broad, inchoate movement with any organized group either inside or outside the profession. Nor ought we to attribute any special political color to post-behavioralists in the aggregate. They range widely, from conservatism to the active left. Nor has this movement any particular

* Presidential Address delivered to the 65th Annual Meeting of the American Political Science Association, September 2–6, 1969. New York City.

methodological commitments. It embraces rigorous scientists as well as dedicated classicists. Neither does it appeal to any one age group alone. Its adherents include all the generations, from young graduate students to older members of the profession. This whole improbable diversity—political, methodological, and generational—is bound together by one sentiment alone, a deep discontent with the direction of contemporary political research.

Even though today the organized cleavages within our profession are writing most of the dramatic scenarios, in the end these cleavages may prove to be the least interesting part of what is happening. What will undoubtedly have far deeper meaning for us is the broader intellectual tendency that provides the environment within which current divisions have taken shape. It is on the purely intellectual components of post-behavioralism, therefore, that I shall focus.

New as post-behavioralism is, the tenets of its faith have already emerged clearly enough to be identifiable. They form what could be called a Credo of Relevance[1] I would describe the tenets of this post-behavioral credo as follows:

1. Substance must precede technique. If one *must* be sacrificed for the other—and this need not always be so—it is more important to be relevant and meaningful for contemporary urgent social problems than to be sophisticated in the tools of investigation. For the aphorism of science that it is better to be wrong than vague, post-behavioralism would substitute a new dictum, that it is better to be vague than non-relevantly precise.

2. Behavioral science conceals an ideology of empirical conservatism. To confine oneself exclusively to the description and analysis of facts is to hamper the understanding of these same facts in their broadest context. As a result empirical political science must lend its support to the maintenance of the very factual conditions it explores. It unwittingly purveys an ideology of social conservatism tempered by modest incremental change.

3. Behavioral research must lose touch with reality. The heart of behavioral inquiry is abstraction and analysis and this serves to conceal the brute realities of politics. The task of post-behavioralism is to break the barriers of silence that behavioral language necessarily has created and to help political science reach out to the real needs of mankind in a time of crisis.

4. Research about and constructive development of values are inextinguishable parts of the study of politics. Science cannot be and never has been evaluatively neutral despite protestations to the contrary. Hence to understand the limits of our knowledge we need to be aware of the value premises on which it stands and the alternatives for which this knowledge could be used.

5. Members of a learned discipline bear the responsibilities of all intellectuals. The intellectuals' historical role has been and must be to protect the humane values of civilization. This is their unique task and obligation. Without this they become mere technicians, mechanics for tinkering with society. They thereby abandon the special privileges they have come to claim for themselves in academia, such as freedom of inquiry and a quasi-extraterritorial protection from the onslaughts of society.

6. To know is to bear the responsibility for acting and to act is to engage in reshaping society. The intellectual as scientist bears the special obligation to put his knowledge to work. Contemplative science was a product of the nineteenth century when a broader moral agreement was shared. Action science of necessity reflects the contemporary conflict in society over ideals and this must permeate and color the whole research enterprise itself.

7. If the intellectual has the obligation to implement his knowledge, those organizations composed of intellectuals—the professional associations—and the universities themselves, cannot stand apart from the struggles of the day. Politicization of the professions is inescapable as well as desirable.

No one post-behavioralist would share all these views. I have presented only a distillation of the maximal image. It represents perhaps a Weberian ideal type of the challenges to behavioralism. As such the credo brings out most of the salient features of the post-behavioral revolution as it appears to be taking shape today.

II. SHIFTING IMAGES OF SCIENCE

What has this developing new image of political science to offer us? In the United States behavioralism has without doubt represented the dominant approach in the last decade. Will post-behavioralism destroy the undeniable gains of the behavioral revolution or is post-behavioralism only a valuable addition that can and should be incorporated into our practices?

One thing is clear. In a rapidly changing world surely political science alone cannot claim to have completed its development. Only

[1] Compare with the Credo of Behavioralism as described in D. Easton, *A Framework for Political Analysis* (Englewood Cliffs, N. J.: Prentice-Hall, 1965), p. 7.

on the assumption that behavioral political science has said the last word about what makes for adequate research and an appropriate discipline can we automatically read out of court any proposals for change.

The history of the various theoretical sciences, like physics and chemistry, reveals that every discipline rests on certain fundamental assumptions. It is a captive of what has been described as a research paradigm.[2] Over the years political science has been no less prone to develop models of what constitutes a good discipline or adequate research, and these models have undergone marked transformations.

The behavioral model of this century has been but the last in a long chain. It has shifted the balance of concern from prescription, ethical inquiry, and action to description, explanation, and verification. Behavioralism has justified this shift on the grounds that without the accumulation of reliable knowledge, the means for the achievement of goals would be so uncertain as to convert action into a futile game. The growing success of the scientific enterprise in political science cannot be denied.

New conditions of the modern world, however, force us to reconsider our image of what we want to be. Scientific progress is slow, and however more reliable our limited knowledge about politics has become in the last fifty years, social crises of unforeseen proportions are upon us. Fear of the nuclear bomb, mounting internal cleavages in the United States in which civil war and authoritarian rule have become frightening possibilities, an undeclared war in Vietnam that violates the moral conscience of the world, these are continuing conditions entirely unpredicted by political science, behavioral or otherwise. The search for an answer as to how we as political scientists have proved so disappointingly ineffectual in anticipating the world of the 1960's has contributed significantly to the birth of the post-behavioral revolution.

In this perspective the legitimacy of raising doubts about the adequacy or relevance of political science in the contemporary world of crises cannot be questioned. We can join the post-behavioral movement at least in asking: Must we be committed eternally to an unchanging image of the discipline, behavioral or otherwise? Is it not incumbent on us to take account of changing conditions and to be ready and willing to reconsider old images and modify them to the extent deemed necessary? Must political science continue to do what it has been doing over the last few decades, in the hope that some "normal" period will one day return in which time will be on the side of those who seek to develop a more reliable understanding of political processes?

The negative answer that many individuals from all generations of political scientists are giving is clear. One of the probable underlying reasons for this answer we can readily understand. Mankind today is working under the pressure of time. Time is no longer on our side. This in itself is a frightening new event in world affairs. An apocalyptic weapon, an equally devastating population explosion, dangerous pollution of the environment, and, in the United States, severe internal dissension of racial and economic origin, all move in the same direction. They move toward increasing social conflict and deepening fears and anxieties about the future, not of a generation or of a nation, but of the human race itself. Confronting this cataclysmic possibility is a knowledge of the enormous wealth and technical resources currently available in a few favored regions of the world, the spectacular rate of increase in man's material inventiveness and technology, and the rich potential just on the horizon for understanding social and political processes. The agony of the present social crisis is this contrast between our desperate condition and our visible promise, if we but had the time.

In the face of a human situation such as this, the post-behavioral movement in political science (and in the other social sciences simultaneously) is presenting us with a new image of our discipline and the obligations of our profession. It pleads for more relevant research. It pleads for an orientation to the world that will encourage political scientists, even in their professional capacity, to prescribe and to act so as to improve political life according to humane criteria.

We can respond by refusing to budge, much as the classicists and traditionalists once did in the face of the onslaught of the behavioralists. Or we can recognize the need for change and explore the best ways of reconstructing our conception of our discipline and of the related professional institutions of which we are part. It is the second course that I propose we consider.

III. THE IDEAL COMMITMENTS OF POLITICAL SCIENCE

A decision to contemplate revising the image of our discipline and profession places the political scientist in a strange and difficult predicament. Fierce pressures are building up for solutions to immediate problems. Yet the na-

[2] T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

ture of basic research is to shift the focus away from current concerns and to delay the application of knowledge until we are more secure about its reliability.

This dilemma of contemporary political science is perhaps best revealed in the ideal commitments of behavioralism. For example, according to the behavioral image of science, those very epistemological characteristics of political research to which the post-behavioralists so strongly object would seem to be unavoidable, indeed, highly desirable. Post-behavioralism deplores what it views as technical excesses in research. Yet no one could possibly deny that technical adequacy is vital. Without it the whole evolution of empirical science in all fields of knowledge in the last two thousand years would have been in vain. Despite some post-behavioral objections to scientific abstractness and remoteness from the world of common sense, by its nature science must deal with abstractions. No science could by itself cope with the whole reality as it is interpreted by the politician. Only by analysis, by chopping the world up into manageable units of inquiry, by precision achieved through measurement wherever possible, can political science meet the continuing need of a complex, post-industrial society for more reliable knowledge. Even to appeal to science to discard abstract theory and models as the test of relevance for research and to put in their place the social urgency of problems, is to ask it to sacrifice those criteria which have proved most successful in developing reliable understanding.

Furthermore, it appears that the use of the methods of behavioral science favors the very kind of sociological position for the political scientist to which post-behavioralism so strenuously objects. These methods help to protect the professional scientist from the pressures of society for quick answers to urgent if complicated problems. The history of the natural sciences shows us how slowly basic research moves. The overshadowing new ideas in the natural sciences—Newtonian mechanics, Darwinian evolution, Einstein's relativity, or modern cybernetics—come infrequently, on a time scale of centuries. But during the intervals between new ideas, great or small, science seeks to work out their implications with a passion for details, even if research seems to lead away from the practical, obvious problems of the day. These seemingly remote, often minute details, about scales, indices, specialized techniques for collecting and analyzing data and the like, these details are the building blocks of the edifice in which more reliable understanding occurs.

What is true about the slow pace of basic research in the natural sciences and about its remoteness we can expect to apply with equal force to the social sciences. Indeed in social research we even have difficulty in agreeing on the great discoveries, so undeveloped are our criteria of adequacy. In addition, even if the political scientist begins with an immediate social problem, as he so often does, in the process of investigation he will be likely to restate the problem in more researchable terms. This reconceptualization usually leads him back to the very kind of fundamentals that appear irrelevant to initial practical concerns.

The ideology of pure or basic research and its success in the better developed sciences in providing a reliable base of knowledge have seemed to justify this research strategy, slow and painstaking as it is. In helping to protect scholarship from the daily pressures of society for quick and ready answers, this ideology has freed science to pursue truth in the best way it knows how.

This same concern for generalized, verifiable understanding has forced social scientists to discriminate with extreme care about what we can and cannot do with our premises and tools. We can describe, explain, and understand but we cannot prescribe ethical goals. The value question is thus set aside, not because we consider it inconsequential, but only because we see it as unresponsive to the tools useful in analyzing and explaining the empirical world.

These then are some of the normal ideal commitments of science: technical proficiency in the search for reliable knowledge, the pursuit of basic understanding with its necessary divorce from practical concerns, and the exclusion of value specification as beyond the competence of science. It is these ideals that behavioral research in political science has sought to import into the discipline.

IV. NEW STRATEGIES FOR SCIENCE

Today these traditional ideals of science are confronted with a set of social conditions which have no historical precedent. This extraordinary circumstance has created the predicament in which behavioral research now finds itself. It derives from the fact that we are confronted with a new and shortened time scale in the course of human events, one in which the future may need to be discounted more heavily than ever before. For many, nuclear war or civil strife, with authoritarianism as a credible outcome, are clear and present dangers, to be counted in decades at the most. For many, without immediate and concentrated attention to the urgent issues of the present day, we may

have no future worth contemplating, however uncertain our findings or inadequate our tools. How then can behavioral research, with its acknowledged glacial pace and apparent remoteness, hope to meet the demands now being placed upon our discipline?

For some among post-behavioralists, the fear of physical and political self-destruction has led to the abandonment of science altogether. For them science is simply incapable of measuring up to contemporary needs. Others, who have always considered science to be inherently defective, now feel justified in their convictions. But for those post-behavioralists who continue to place their hopes in modern behavioral science, the current crisis poses the issue about the wisdom of continuing our commitment to a "normal" strategy of scientific research. These kinds of post-behavioralists have been driven to conclude that we have no alternative but to make our research more relevant. For them we can do so only by devoting all our professional energies to research, prescription and action with regard to the immediate issues of the day. In short, we are asked to revise our self-image by postponing the demands of slow-moving basic research and by acting in our professional capacity so as to put whatever knowledge we have to immediate use.

For all of us this plea poses some critical questions. Even in the face of the social crises of our time, do we really need to subordinate the long-run objectives of the scientific enterprise to the undeniably urgent problems of the day? Is there any other way in which we can cope with this transparent need for practical relevance? And if so, can we hope to retain for political science those conditions of theoretical autonomy, precision, and relative insulation so vital if we are to continue to be able to add to our capital stock of basic understanding?

I would argue that we do not need to abandon the historical objectives of basic science. There is a strategy that will enable us to respond to the abnormal urgency of the present crises and yet preserve these traditions. By adopting this course, post-behavioralism need not be considered a threat to behavioral research but only an extension of it necessary for coping with the unusual problems of the present epoch.

To appreciate the strategy implied, we must remember one thing. Even if it *is* arguable that the time scale in terms of which we must think has been greatly shortened, mere projection cannot fully persuade us that the future needs to be counted in decades, not centuries. What little solace we may get from it, we know that our intuitions have been wrong in the past. We may still have centuries rather than only decades ahead of us.

This realistic possibility suggests that we ought to pursue an optimizing strategy in which there is some apportionment of resources for the long run as against the short run, just in case we are not in fact all dead. The cost of devoting our efforts exclusively to short-run crises is far too high. It might easily assure that if we do in fact survive the present crises, the failure to continue to add to our capital accumulation of basic social knowledge will see us tragically unprepared for even greater crises in the more distant future. We will then have lost every chance to prevent the self-annihilation of mankind or the collapse of those political institutions we cherish.

Is there any sensible way in which we can provide for some satisfactory use of our resources without distracting excessively from the attention and altered research orientations that the major issues of the country and the world require? It is to this question that those of us who still have some hope that we may survive the certain and greater crises of the near future ought to be devoting some of our energies. Various courses of action are possible and we need to consider them as they apply to the discipline as well as to the profession.

V. THE DISCIPLINE

Basic vs. applied research

For the discipline, the post-behavioral revolution suggests the appropriateness of revising our ideal image at least as it has been incorporated into behavioralism. It is vital to continue to recognize the part that basic research ought to play. But in the allocation of financial and human resources we must also consciously recognize that a shift in emphasis must occur at once to take into account the critical times in which we live.

In terms of any ideal distribution of our efforts, basic research ought to command a disproportionate share. Although socially useful results from such research are usually a long time in coming, they are in the end more dependable. But under the inescapable pressure of current crises the emphasis needs to be reversed. A far larger part of our resources must be devoted to immediate short-run concerns. We need to accept the validity of addressing ourselves directly to the problems of the day to obtain quick, short-run answers with the tools and generalizations currently available, however inadequate they may be. We can no

longer take the ideal scientific stance of behavioralism that because of the limitations of our understanding, application is premature and must await future basic research.[3]

In truth this proposal represents less of a shift in our practices than a change in our ideological posture. The behavioral revolution has never been fully understood or absorbed into the discipline; we are still grappling with its meaning. Any casual inspection of ongoing research would reveal that, regardless of any ideal apportionment, at no time has pure research really consumed more than a very small fraction of the discipline's resources. We have been only too ready to advise federal, state and local agencies on immediate issues and political parties and candidates about their campaigns. It is just that with the behavioral revolution the ideals of the discipline as incorporated in research ideology were beginning to change. This new image legitimated that kind of basic research, the pay-off of which might not be immediately apparent, but the future promise of which was thought to be considerable. Today we need to temper our behavioral image of the discipline so that in these critical times we no longer see it as commanding us to devote most of our efforts to the discovery of demonstrable basic truths about politics. We will need to obtain more of our satisfactions from seeking immediate answers to immediate problems.

This kind of shift in disciplinary focus will call urgently for the systematic examination of the tasks involved in transforming our limited knowledge today into a form far more consumable for purposes of political action. Certain difficulties stand in the way of applying our knowledge. In the first place, contemporary social problems far outrun the capacity of political science alone or in concert with the other social sciences to solve them. Our basic knowledge is itself limited. What little we have is not necessarily directly applicable to practical issues.

In the second place, like medieval medicine, we may still be at the stage in which we are letting blood in the hope of curing the patient. Because of our low capacity for sorting out the complex causal connections between our advice and its social consequences, we have little assurance that we may not be doing more harm than good. Some efforts are currently under way to correct this situation. In the broadening quest for social indicators we are inventing techniques for isolating the outcomes of policy outputs[4] and for comparing these consequences with the presumed policy goals.[5] Thereby we shall have a measure of the effects of our intervention in the social processes. But the success of these efforts lies some distance in the future.

In the third place, political science alone is unable to propose solutions to social problems; these normally involve matters that call upon the specialized knowledge and skills of other social scientists. Yet seldom do policy makers seek the collective advice of comprehensive teams of social scientists.

These and many other difficulties have stood in the way of the application of our knowledge to specific situations. They have contributed to the low academic esteem of applied science, in comparison at least with basic research. Past efforts at application have experienced too little success to attract the best minds of the day.[6] In temporarily modifying the immediate priorities of the discipline, we will need to devise ways for elevating the self-conscious development of applied knowledge, inappropriately called social engineering, to the respectability that behavioralism has succeeded in acquiring for basic research.

To assign all of our research resources to the present, however, as some post-behavioralists seem to be suggesting, would be to discount the future far too heavily. We need to keep alive and active the legitimate long-range interests of all science. Social problem-solving is not totally inconsistent with this objective. The line between pure and applied research is often very fine. Those of us who choose to adopt the long-run point of view, optimistically expecting the survival of mankind, will find much from which to profit in the research undertaken by those concerned with applied problems. Yet this cannot relieve us of the need to continue to devote specific attention to basic problems in the discipline—to the reconceptualization of our significant variables, to the continuing search for adequate units of politi-

[3] See D. Easton, *The Political System* (New York: Knopf, 1953), pp. 78 ff.

[4] For the difference between outcomes and outputs see D. Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965), p. 351.

[5] For the literature on social indicators see R. A. Bauer (ed.), *Social Indicators* (Cambridge: M.I.T. Press, 1966); "Social Goals and Indicators for American Society," *Annals of the American Academy of Political and Social Sciences*, vols. 371 (May, 1967) and 373 (September, 1967).

[6] See H. W. Riecken, "Social Science and Contemporary Social Problems," 23 *Items* (1969), 1–6.

cal analysis, to the exploration of alternative theories and models about the operation of various types of systems, and to our basic methodological assumptions and technical requirements. Admittedly these persisting concerns often lead us far from the practical issues of the day. Yet without attending to these basic problems we cannot hope to add to our store of reliable knowledge, and thereby to prepare ourselves for equally critical political crises in the more distant future.

Value premises and research interests

In addition to suggesting this temporary reallocation of our resources as between basic and applied research, we need to become increasingly aware of the fact that basic research is not without its own substantive deficiencies. This is the message underlying the constant post-behavioral complaint that our research is not relevant. It is argued that excessive preoccupation with techniques and with factual description has distracted us from the significant questions about the operation of the American democratic system in particular. We have learned a great deal about this system but all within a value framework that accepts the ongoing practices as essentially satisfactory and at most subject only to the need for incremental improvements. As a discipline we have proved incapable of escaping a commitment to our own political system. This research myopia, the post-behavioralists argue, has discouraged us from posing the right questions for discovering the basic forces that shape the making and execution of authoritative decisions.

Here the post-behavioralists are alerting us, once again, to what has been repeatedly revealed over the years, by Marx, Weber, and Mannheim, among others, namely, that all research, whether pure or applied, of necessity rests on certain value assumptions. Yet the myth that research can be value-free or neutral dies hard. We have continued to develop our discipline as though the subjects we select for research, the variables we choose to investigate, the data we collect, and the interpretations we generate, have all some extraordinary pristine purity, unsullied by the kinds of value premises to which we subscribe, consciously or otherwise. We do not consistently ask the question, central to the sociology of knowledge: To what extent are our errors, omissions, and interpretations better explained by reference to our normative presuppositions than to ignorance, technical inadequacy, lack of insight, absence of appropriate data, and the like? Behavioralists have indeed failed to insist, with the same fervor we have applied to our technological innovations, that our operating values be brought forward for self-conscious examination and that their impact on research be assessed.

Today the hazards of neglecting our normative presuppositions are all too apparent. There can be little doubt that political science as an enterprise has failed to anticipate the crises that are upon us. One index of this is perhaps that in the decade from 1958 to 1968, this REVIEW published only 3 articles on the urban crises; 4 on racial conflicts; 1 on poverty; 2 on civil disobedience; and 2 on violence in the United States.[7]

In some considerable measure we have also worn collective blinders that have prevented us from recognizing other major problems facing our discipline. For example, how can we account for the failure of the current pluralist interpretations of democracy to identify, understand, and anticipate the kinds of domestic needs and wants that began to express themselves as political demands during the 1960's? How can we account for our neglect of the way in which the distribution of power within the system prevents measures from being taken in sufficient degree and time to escape the resort to violence in the expression of demands, a condition that threatens to bring about the deepest crisis of political authority that the United States has ever suffered? How can we account for the difficulty that political science as a discipline has in avoiding a commitment to the basic assumptions of national policy, both at home and abroad, so that in the end, collectively we have appeared more as apologists of succeeding governmental interpretations of American interests than as objective analysts of national policy and its consequences? Finally, in even so recent a major research area as political socialization, how can we account for the natural, effortless way in which inquiry has sought to reveal the contributions of preadult political learning to the stability of systems, virtually ignoring the equally significant function of socialization in bringing about political change?[8]

There is no single explanation for the narrow vision of our discipline. We can, however, at least go so far as to offer this hypothesis: Whatever the reasons, the failure to broaden the

[7] This undoubtedly reflects only the few articles on this subject submitted for publication rather than any editorial predisposition.

[8] See D. Easton and J. Dennis, *Children in the Political System: Origins of Political Legitimacy* (New York: McGraw-Hill, 1969), chapter 2.

vision of our basic research may be due in good part to a continuing hesitation to question our normative premises and to examine the extent to which these premises determine the selection of problems and their ultimate interpretations.

Creative speculation

How are we to make those serious efforts necessary to break out of the bonds imposed on basic research itself by ongoing value frameworks? How are we to create those conditions that will help us to ask fundamental questions about the operation of political systems, that will lead us to pose those "outrageous hypotheses" about which Robert Lynd once chided us?[9] A new awakening to the part that our value commitments and other social influences play in limiting the range of our basic research may partly correct the errors of our ways. But this moral self-scrutiny may not be enough. If we are to transcend our own cultural and methodological biases, such self-awareness can carry us only part of the way. We may need to take stronger measures and find additional help by returning to an older tradition in political research but in a thoroughly modern way.

Many years ago, in *The Political System*, I argued for the urgent need to reconsider our approach to value theory at the same time as we began the equally critical task of constructing empirical theory.[10] The latter task is now under way in our discipline. The first one, creative construction of political alternatives, has yet to begin.

To enrich their own understanding and to give broader meaning to their own social reality, the great political theorists of the past found it useful to construct new and often radically different conceptions of future possible kinds of political relationships. By formulating such broad, speculative alternatives to the here and now we too can begin to understand better the deficiencies of our own political systems and to explore adequate avenues of change that are so desperately needed. This, I would argue, must now be considered part of the task and responsibility of science if it is to retain its relevance for the contemporary world. Those philosophies that seek to revive classical natural law and that reject the possibility of a science of man have thereby forfeited their opportunity and put in question their fitness to undertake this creative task of theory. We require boldly speculative theorizing that is prepared to build upon rather than to reject the findings of contemporary behavioral science itself and that is prepared to contemplate the implications of these findings for political life, in the light of alternative, articulate value frameworks.

The significance for political science of this kind of creative speculation cannot be overestimated. For those who seek to understand how political systems operate, such speculation provides alternative perspectives from which to determine the salience of the problems they choose for research and analysis. If we take seriously the conclusions of the sociologists of knowledge, then our scientific output is very much shaped by the ethical perspectives we hold. In that event, by failing to encourage within the discipline creative speculation about political alternatives in the largest sense, we cannot help but imprison ourselves within the limitations of the ongoing value framework. As that framework begins to lose its relevance for the problems of society, its system-maintenance commitments must blind us to the urgent questions emerging even for the immediate future.

And this is precisely what has happened to political science. Both our philosophers and our scientists have failed to reconstruct our value frameworks in any relevant sense and to test them by creatively contemplating new kinds of political systems that might better meet the needs of a post-industrial, cybernetic society. A new set of ethical perspectives woven around this theme might sensitize us to a whole range of new kinds of basic political problems worth investigating. It might also point up the significance of inquiry into these problems with new or radically modified types of relevant empirical theories. Thereby we could perhaps be freed from that occupational myopia brought about by excessive attention to the facts as they are. We would perhaps be less prone to stumble into the pitfall of "empirical conservatism,"[11] or commitment to system-maintenance perspectives, of which political science has with justice been accused[12] by post-behavioralists and others.

In these several ways, then, does our discipline need reordering. Basic research needs to

[9] R. S. Lynd, *Knowledge for What?* (Princeton: Princeton University Press, 1939).

[10] D. Easton, *The Political System*, chapters 9 and 10.

[11] H. Marcuse, *One-Dimensional Man* (Boston: Beacon Press, 1964), chapter 4.

[12] See C. A. McCoy and J. Playford (eds.), *Apolitical Politics* (New York: Crowell, 1967) and D. Easton, *The Political System*, chapters 2 and 11.

be maintained as an investment for the future. But even *its* priorities need to be rearranged in the light of a better understanding of its own value assumptions. Applied, action-oriented research requires more systematic attention than ever before. We need greater awareness of the limits that our value premises have imposed on our research; and on the solid foundation of knowledge constructed by behavioral research, alternative possible rearrangements of our political relationships need to be seriously contemplated.

VI. THE PROFESSION AND THE USE OF KNOWLEDGE

Not only our discipline, however, but our profession needs restructuring to bring it into harmony with the changing conceptions of social science. Our discipline refers to our intellectual enterprise; our profession, to the trained and expert scholars who participate in the discipline. Post-behavioralism suggests that behavioral commitments create not only a discipline but a profession that shows a declining relevance to the political world around it.

The behavioral image of the profession

Two basic reasons account for this decline, it is in effect argued. First, professionalization of the discipline in behavioral terms has nourished an image of political science in which knowledge and action have been carefully separated and compartmentalized.[13] As scientists possessed of special skills, we see ourselves as purveyors of something called professional expertise. Our task as experts is to offer advice about means only, not about the purposes to which our knowledge might be put. As the well-worn adage puts it, we are on tap, not on top.

In fact, as post-behavioralism correctly asserts, the expert has never lived by this rule. In the discipline, as we have already noted, behavioral inquiry has not been able to attain any real measure of ethical neutrality. This has had serious consequences for basic research. In the profession too, the critics point out, ethical neutrality is no less spurious. In the application of his knowledge the political scientist explicitly or unwittingly accepts the value premises of those he serves. His posture of neutrality has the added consequence of undermining his will or capacity to challenge the broader purposes to which his knowledge is put.

A second reason accounts for the decline of professional relevance. Here post-behavioralism breaks sharply with the prevailing professional paradigm about the moral relationship between research and action. In the behavioral interpretation, the possession of knowledge imposes no special obligation on the political scientist to put his knowledge to use in the service of society. He remains free to choose whether or not he ought to step outside his scientific role for this purpose. This laissez faire attitude towards political engagement has been an accepted moral premise of the profession. It has permitted if not encouraged withdrawal from political strife. Knowledge is divorced from action.

For post-behavioralism, however, the line between pure research and service begins to fade. Knowledge brings an awareness of alternatives and their consequences. This opportunity for rational choice imposes special obligations on the knower. The political scientist as a professional is the knower *par excellence*. It is therefore immoral for him not to act on his knowledge. In holding that to know is to bear a responsibility for acting, post-behavioralism joins a venerable tradition inherited from such diverse sources as Greek classical philosophy, Karl Marx, John Dewey, and modern existentialism.

Criteria for the use of knowledge

The implications of this post-behavioral shift in the image of the professional's role in society are considerable. If the political scientist is to evaluate the uses to which his knowledge is being put and if he is himself to bring his knowledge to bear on social issues, what criteria are to guide his choices? Here post-behavioralism returns to the humanist conception of the intellectual as the guardian of those civilized, humane values known to most men. It is incumbent on the professional to see to it that all society, not just a privileged part, benefits from his expertise. His obligations are met only if he takes into account the broadest spectrum of interests in society.

Many post-behavioralists scrutinize the activities of scholars in recent years and conclude that the talents of political scientists have been put in the service largely of the elites in society —in government, business, the military and voluntary organizations. The professional is seen as having little communication and contact with those who characteristically benefit least from the fruits of modern industrial society—the racial and economic minorities, the unrepresented publics at home, and the colonial masses abroad. These are the groups least able to command the resources of expertise for which political science stands. The social re-

[13] See especially T. Roszak (ed.), *The Dissenting Academy* (New York: Random House, 1968), Introduction.

sponsibility of the political science expert is to rectify the imbalance.

In this post-behavioral view, the application of expert knowledge in the service of social reform becomes competitive with the pursuit of knowledge for its own sake. Reform becomes inseparable from knowledge.

Clearly there is in birth a new image of the professional, one in which science is not necessarily denied its place but in which the scientist is no longer free to divorce the life of the mind from the life of social action. Weber's differentiation between the vocation of the scientist and that of the politician no longer wholly suffices.

This new image leads to the politicization of the profession. If the individual professional is called upon to utilize his knowledge on behalf of society, those collectivities of experts that we call the professional associations are themselves equally culpable if in their corporate capacity they fail to challenge the purposes to which their expertise may be put or if they fail to act when their knowledge warns them of danger. Herein lie the moral and intellectual roots of the constant pressure of the professional associations to take positions on public issues about which their competence may give them special knowledge.

The politicization of the profession

This post-behavioral tendency to politicize the professional associations has met with great resistance. Objection arises less from principled argument than from the practical fear that our professional associations will no longer be able to fulfill their normal scientific purposes. Let us grant the plausibility of this practical consideration. Even so, do we need to reject entirely the new moral image being developed by post-behavioralism?

One fact is clear. The crisis of our times spares no group, not even the social sciences. The pressures to utilize all of our resources in critically evaluating goals as well as in providing effective means are too great to be denied. For increasing numbers of us it is no longer practical or morally tolerable to stand on the political sidelines when our expertise alerts us to disaster.

In accepting this new (but ancient) obligation of the intellectual, however, we need to recognize that the professional political scientist may engage in three distinguishable kinds of activity. These are teaching and research on the one hand and practical politics on the other. Somewhere between these the political scientist acts as a consultant and an adviser. Each of these kinds of activity—as a scholar, politician, and consultant—shapes and influences the other. Is it feasible to construct a single organization that will serve the collective purposes of the profession for facilitating all three of these kinds of activities? It seems highly unlikely. Can we provide some sensible division of labor among different organizations that will permit the fullest expression for all those activities into which these critical times are pressing the professional political scientist? This seems possible.

We can conceive of some professional organizations being devoted largely to that kind of action that helps to add to our store of basic knowledge and that eases communication among ourselves and among succeeding generations of political scientists. These we already have in our professional associations. They are designed to aid both teaching and research. We can, however, also conceive of other types of professional organizations that would be concerned with structuring the application of our expertise to ongoing critical social problems. This kind of organization we do not yet have in political science, or, for that matter, in the social sciences as a whole.

But here if we consider the matter only as political scientists we create insurmountable difficulties for ourselves. Social problems do not come neatly packaged as economic, psychological, political and the like. Our crises arise out of troubles that involve all aspects of human behavior. Our professional associations are oriented toward the disciplines, and these are analytic fields. Of necessity they piece up reality into specialties that have meaning largely for the pursuit of fundamental understanding. For purposes of setting goals and determining means for solving social problems, however, we need to draw the disciplines together again into a single organization, one that can mobilize the resources of all the social sciences and bring them to a focus on specific issues.

To this end it is time that we accept our special responsibility as students of politics. We must take the initiative by calling for the establishment of a Federation of Social Scientists, a proposal that has already been advanced by one of our colleagues.[14] The tasks of such a Federation would be to identify the major issues of the day, clarify objectives, evaluate action taken by others, study and propose alternative solutions, and press these vigorously in the political sphere.

Without collectively politicizing ourselves in

[14] David Singer of the Mental Health Research Institute, University of Michigan, Ann Arbor, Michigan, in personal correspondence.

this way, by the very act of standing by while the problems of the world continue to increase in numbers and intensity, we thereby uncritically acquiesce in prevailing policies. We in fact adopt a political position. By acting collectively in our professional capacities through a Federation of Social Scientists, we will have an opportunity to justify our policies intellectually and morally. Thereby we may begin to satisfy our growing sense of political responsibility in an age of crisis. At the same time we shall be able to preserve our historic institutions, the professional associations, for the continuing pursuit of fundamental knowledge.

Such a Federation would fail in its responsibilities, however, if it became merely an echo of national goals, an instrument of official policy, or a bland critic of things as they are. If Mannheim is correct in describing the intellectual as the least rooted of all social groups, the professional social scientist ought to view himself as committed to the broadest of humane values. These need to be the touchstone that he brings to bear on social isuses. Yet many barriers block the way. Of these identification with the goals and interests of one's nation is prominent. Political scientists have still to escape the crippling effects for scholarship of unwitting commitment to national goals and perspectives. Just as science as a set of disciplines has pretensions to being international in scope, so the social scientist himself needs to be denationalized. Some day, like the ideal international civil servant, the professional social scientist too may be permitted to achieve maximum freedom from national commitments by being obliged to carry an international passport and to conduct himself accordingly.

For the profession, therefore, the emerging post-behavioral phase is encouraging the development of a new norm of behavior. It sees policy engagement as a social responsibility of the intellectual whatever the institutional form through which this may be expressed. Some day it may also require the release of the social scientist from bondage to the unique needs and objectives of his own national political system.

It is clear that changing times require radical re-thinking of what we are and what we want to be both as a discipline and as a profession. Post-behavioralism is a pervasive intellectual tendency today that reveals a major effort to do just this. Its very pervasiveness prevents it from becoming the possession of any one group or of any one political ideology. It supports and extends behavioral methods and techniques by seeking to make their substantive implications more cogent for the problems of our times. Post-behavioralism stands, therefore, as the most recent contribution to our collective heritage. For that very reason, as an intellectual tendency it is not the threat and danger that some seem to fear. Rather, in the broad historical perspectives of our discipline, the post-behavioral revolution represents an opportunity for necessary change. We may choose to take advantage of it, reject it, or modify it. But to ignore it is impossible. It is a challenge to re-examine fearlessly the premises of our research and the purposes of our calling.

POLITICAL THEORY AS A VOCATION*

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The purpose of this paper is to sketch some of the implications, prospective and retrospective, of the primacy of method in the present study of politics and to do it by way of a contrast, which is deliberately heightened, but hopefully not caricatured, between the vocation of the "methodist"[1] and the vocation of the theorist. My discussion will be centered around the kinds of activity involved in the two vocations. During the course of the discussion various questions will be raised, primarily the following: What is the idea which underlies method and how does it compare with the older understanding of theory? What is involved in choosing one rather than the other as the way to political knowledge? What are the human or educational consequences of the choice, that is, what is demanded of the person who commits himself to one or the other? What is the typical stance towards the political world of the methodist and how does it compare to the theorist's?

The discussion which follows will seek, first, to locate the idea of method in the context of the "behavioral revolution," and, second, to examine the idea itself in terms of some historical and analytical considerations. Then, proceeding on the assumption that the idea of method, like all important intellectual choices, carries a price, the discussion will concentrate on some of the personal, educational, vocational, and political consequences of this particular choice. Finally, I shall attempt to relate the idea of the vocation of political theory to these same matters.

I. THE IDEA OF "METHOD" IN THE BEHAVIORAL REVOLUTION

In compiling its recent *Biographical Directory*, the American Political Science Association distributed a questionnaire which in its own way helped raise the present question, "What is the vocation of the political theorist?" Political Theorists were invited to identify themselves by choosing among "Political Theory and Philosophy (Empirical)," "Political Theory and Philosophy (Historical)," and "Political Theory and Philosophy (Normative)." Although the choices offered may signify vitality and diversity, they may also testify to considerable confusion about the nature of political theory. For their part, political theorists may think of it as an identity crisis induced by finding themselves officially assigned a classification which others have defined, a classification traceable to a set of assumptions about the nature of the theoretical life perhaps uncongenial to many theorists.

Beyond the matter of professional identity there are far more compelling reasons for raising the question of vocation. Whatever one's assessment of the "behavioral revolution," it clearly has succeeded in transforming political science. What is less clear is the precise nature of that revolution. Among leading spokesmen of the profession it has become stylish to interpret that revolution as a close facsimile of the sorts of scientific changes discussed by Thomas Kuhn in *The Structure of Scientific Revolutions*.[2] Accordingly, the behavioral revolution is described as the inauguration of a new theoretical paradigm. Such a view, I think, is mistaken. It blurs the significance of the change. A more accurate account is suggested by the following: "One of the most significant recent developments in the social sciences is the revolution in data gathering and data evalu-

* This is a revised version of a paper delivered September, 1968, before the Conference for the Study of Political Thought.

[1] "*Methodist.* One who is skilled in, or attaches great importance to, method; one who follows a (specified) method." *Oxford Universal Dictionary.*

Although most social scientists would contend that actual research rarely conforms to a step-by-step procedure, it remains the case that such procedure stands as a model for what they aim at. Thus, in a section of a text-book on research methods entitled "Major Steps in Research," the authors insert the qualification above but acknowledge that "published research strongly suggests the existence of a prescribed sequence of procedures, each step presupposing the completion of the preceding one." Claire Selltiz *et al., Research Methods in Social Relations*, rev. ed. (New York: Holt, Rinehart & Winston, 1963), pp. 8–9.

[2] I have discussed Kuhn's interpretation and its relevance to political science in "Paradigms and Political Theories," *Politics and Experience. Essays Presented to Michael Oakeshott*, ed. P. King and B. C. Parekh (Cambridge: Cambridge University Press, 1968), pp. 125–152.

ation. This revolution depends upon the developments in techniques by which data can be collected and analyzed. . . . "[3]

Assuming that this statement reflects a widespread sentiment which guides the actual practice of the profession, it provides a clue to the nature of the changes, what they are and what they are not, and what they signify for the vocations of political scientists and theorists. Despite claims to the contrary, political science has not undergone a revolution of the type described by Kuhn in which a new and dominant theory is installed. Although an abundance of new "theories" is available to the political scientist, it should be remembered that, by Kuhn's canon, the mere existence of new theories, or even the fact that some theories have attracted a following, are not conclusive evidence of a revolution. What counts is the enforcement by the scientific community of one theory to the exclusion of its rivals.

Although it is sometimes contended that "systems theory" constitutes the paradigmatic theory of the revolution, it is doubtful that this claim is tenable. Not only is there confusion about which of the several versions of the theory is the preferred one, or even whether any version is useful, but, above all, the popularity of systems theory followed rather than produced the behavioral revolution.

Whatever else it may be, a revolution without an initiating theory cannot qualify as a revolution by Kuhn's criterion. It may be, rather, a typically American revolution in which theories play a minor role. American political scientists, for the most part, have not only generally supported the traditional American diffidence toward theories, but they have elevated it to scientific status. The suspicion of theories is alleged to be a powerful contributor to the political stability of America and to its genius for pragmatic rather than ideological politics. In making this assertion I am not unmindful that there is hardly to be found a journal of political science in which some contemporary has not noted that "the mere accumulation of data without a guiding theory is, etc. . . . " Nor has it escaped my attention that a wide variety of theories exists for the political scientist to choose among. To call them political theories is, in the language of philosophy, to commit something like a category mistake. Systems theories, communication theories, and structural-functional theories are unpolitical theories shaped by the desire to explain certain forms of non-political phenomena. They offer no significant choice or critical analysis of the quality, direction, or fate of public life. Where they are not alien intrusions, they share the same uncritical—and therefore untheoretical—assumptions of the prevailing political ideology which justifies the present "authoritative allocation of values" in our society.

Nonetheless, to say that there has been no political theory which has inspired the revolution in political science is not to say either that there has been no revolution or that no intellectual patterns are being widely promoted throughout the discipline. There has in fact been a certain revolution in political science, one that reflects a tradition of politics which has prided itself on being pragmatic and concerned mainly with workable techniques. Like all technique-oriented activity, the behavioral movement presupposes that the fundamental purposes and arrangements served by its techniques have been settled and that, accordingly, it reenforces, tacitly or explicitly, those purposes and arrangements and operates according to a notion of alternatives tightly restricted by these same purposes and arrangements. The emphasis upon methods does not signify simply the acquisition of a "kit" of new "tools," but presupposes a viewpoint which has profound implications for the empirical world, the vocation and the education of political scientists, and the resources which nourish the theoretical imagination.

To contend that the idea of method is the central fact of the behavioral revolution is merely to repeat what the revolutionaries themselves have stated. "Most important, perhaps, the criteria by which one accepts or rejects statements about social life are of a special nature. The ultimate criterion is the method by which they are gathered."[4] If it should be the case that a widespread set of assumptions is commonly held among those committed to the primacy of method, it is of little consequence that the techniques are diverse and changing. What matters are the common assumptions and consequences which accompany the emphasis on technique. The extent of this transformation is such as to suggest that the study of politics is now dominated by the belief that the main objective—acquiring scientific knowledge about politics—depends upon the adoption and refinement of specific techniques and that to be qualified or certified as a political scientist is tantamount to possessing prescribed techniques. Concurrent with this de-

[3] G. A. Almond and S. Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), p. 43.

[4] *Ibid.*, p. 43.

velopment there has been an effort to imbue political scientists with what is understood to be the ethic of science: objectivity, detachment, fidelity to fact, and deference to intersubjective verification by a community of practitioners. These changes add up to a vocation, a *vita methodica,* which includes a specified set of skills, a mode of practice, and an informing ethic. This vocation, and the education which it requires, may mark the significance of the behavioral revolution.

At this point a protest might be made that too much is being read into the idea of method. Methods *per se* do not presuppose a philosophical view of things, but are neutral or instrumental, analogous to the technician in being indifferent to the purposes of their master. Such an argument is not only wrong but superficial. In the first place, the elevation of techniques has important curricular consequences. The requirement that students become proficient in an assortment of technical skills preempts a substantial portion of their time and energy. But more important, training in techniques has educational consequences for it affects the way in which the initiates will look upon the world and especially the political portion of it. "Methodism" is ultimately a proposal for shaping the mind. Social scientists have sensed this when they have noted that research methods are "tools" which "can become a way of looking at the world, of judging everyday experience."[5]

In the second place, the alleged neutrality of a methodist's training overlooks significant philosophical assumptions admittedly incorporated into the outlook of those who advocate scientific inquiry into politics. These assumptions are such as to reenforce an uncritical view of existing political structures and all that they imply. For the employment of method assumes, even requires, that the world be of one kind rather than another if techniques are to be effective. Method is not a thing for all worlds. It presupposes a certain answer to a Kantian type of question, What must the world be like for the methodist's knowledge to be possible? This presupposition is illustrated by a recent example which listed the major assumptions alleged to underly the "movement" of political behavior. The first item was: "*Regularities.* These are discoverable uniformities in political behavior. These can be expressed in generalizations or theories with explanatory and predictive value."[6] It follows that the methodist is in trouble when the world exhibits "deformities" or emergent irregularities. As the unhappy state of theories of "development" or "modernization" suggests, similar trouble appears when the world manifests "multiformities."[7]

This is but to say that there are inherent limits to the kinds of questions which the methodist deems appropriate. The kind of world hospitable to method invites a search for those regularities that reflect the main patterns of behavior which society is seeking to promote and maintain. Predictable behavior is what societies live by, hence their structures of coercion, of rewards and penalties, of subsidies and discouragements are shaped toward producing and maintaining certain regularities in behavior and attitudes. Further, every society is a structure bent in a particular and persistent way so that it constitutes not only an arrangement of power but also of powerlessness, of poverty as well as wealth, injustice and justice, suppression and encouragement.

It is symptomatic in this connection that political scientists have increasingly taken to describing themselves as "normal scientists."[8] The phrase is Kuhn's and he used it to designate a type of scientist whose vocation is not to create theories or even to criticize them but to accept the dominant theory approved by the scientific community and to put it to work. But if we ask, what is the dominant theoretical paradigm of our normal (political) scientists, the answer is that, in Kuhn's sense, there is none. Yet, surely, although there is no paradigm derived from what Kuhn calls "an extraordinary theory," such as Galileo or Newton produced, there must still be some guiding assumptions or framework which the methodist follows. The answer, I have suggested, is that there is such a framework of assumptions. It is the ideological paradigm reflective of the same political community which the normal scientists are investigating.[9] Thus when a researcher

[5] Selltiz, *et al., op. cit.,* pp. 6–7.

[6] D. Easton, *A Framework for Political Analysis* (Englewood Cliffs: Prentice-Hall, 1965), p. 7.

[7] As a recent work on political socialization (which is described as "a universal feature of political life . . .") admits: " . . . the reader is forewarned that the treatment is heavily biased in favor of a model appropriate to western democracies, particularly in the United States."

[8] See, for example, H. Eulau's language in I. de Sola Pool (ed.), *Contemporary Political Science* (New York: McGraw-Hill, 1967), pp. 58–59; and the more cautious remarks in A. Somit and J. Tanenhaus, *The Development of American Political Science* (Boston: Allyn & Bacon, 1967), pp. 174ff.

[9] This may appear contentious, but, in reality, it is only a restatement of what appears in G. A.

takes "the normal flow of events in American politics" as his starting point, it is not surprising to find him concluding that "the long-run stability of the system depends on the underlying division of party loyalties."[10]

These considerations become even more compelling if we concentrate for a moment upon the "systems" theorist. If society is conceived to be a system of decision-making, and if the recurrence of unjust decisions is commonly acknowledged, it follows that the system is, to some persistent degree, a structure of systematic injustice, otherwise the idea of a system is an inadequate account. The built-in embarrassments of a particular system have sometimes been recognized, as when it is asserted that a supposedly democratic system requires a certain measure of indifference or apathy, especially on the part of the poor and the uneducated. This reservation about systems which purport to be democratic, and hence participatory, is sometimes stated more bluntly when the system in question is non-Western:

> In the Congo, in Vietnam, in the Dominican Republic, it is clear that order depends on *somehow compelling* newly mobilized strata to return to a measure of passivity and defeatism from which they have recently been aroused by the process of modernization.[11]

For the most part, however, the systems theorist prefers to emphasize more formal regularities. Thus, for example, the political system is defined as a special form of "social interactions . . . that are predominantly oriented toward the authoritative allocation of values for a society."[12] What is most revealing about this definition is the location of the word "predominantly": it is placed so as to qualify the "interactions" and thereby to enable subsequent research to distinguish political from social interactions. If the same word had been used, instead, to qualify the "allocations," a substantially different view of a system would have emerged, one in which the allocations would be seen to favor some interactions rather than others. It is acknowledged in the work cited that the favored theory may "inadvertently" exclude "some elements of major importance,"[13] but not that a system may require deliberate and systematic exclusion of major elements. Rather, it is agreed that "a systems approach draws us away from a discussion of the way in which the political pie is cut up and how it happens to get cut up in one way rather than another." The remedy for this "*status quo* bias" is to fall back upon "partial theories" which deal with selected aspects of the *same* system, e.g., theories of "decision-making, coalition strategies, game theories, power, and group analysis."[14] What is conveniently overlooked by this recipe is that it merely reaffirms in different form, the same culinary assumptions about the common pie, for each partial theory claims to be a plausible account of the same whole.

That a discussion of method should naturally lead to considering some prominent theories current among political scientists is not surprising. Most contemporary theories are dependent upon the behavioral revolution, not only in the methodological sense that the theories in question look to behavioral techniques for confirmation or disconfirmation, but in the more important sense of sharing the same outlook regarding education, philosophical assumptions, and political ideology. The close linkage between contemporary ideas of theory and of methods justifies treating them as members of the same family, forming a community of common features which I have labelled "methodism." As the earlier pages have tried to suggest, the idea of method has come to mean far more than was implied by Bentham, for example, when he called it "the order of investigation."[15] It can be better understood as constituting an alternative to the *bios theoretikos*, and, as such, is one of the major achievements of the behavioral revolution. To grasp the nature of the *vita methodica* is not only important for its own sake, but should help in distinguishing it from the activity and vocation of theory.

II. HISTORY OF THE IDEA OF "METHOD"

One way to get at the idea of method is to recognize that it has a history reaching back to ancient Greek philosophy. Like *philosophia, methodus* was often used in association with the notion of a "way" (*aporie*) to truth.[16] Before

Almond, "Political Theory and Political Science," *American Political Science Review*, LX (1966), 873–875.

[10] A. Campbell, "Surge and Decline: A Study of Electoral Change," in A. Campbell *et al.*, *Elections and the Political Order* (New York: Wiley, 1966), p. 45.

[11] I. de Sola Pool, "The Public and the Polity," in Pool (ed.), *op. cit.*, p. 26 (emphasis added).

[12] Easton, *Framework*, p. 50.

[13] *Ibid.*, p. 48.

[14] D. Easton, *A Systems Analysis of Political Life* (New York: Wiley, 1965), p. 475.

[15] *Works*, ed. J. Bowring, 11 vols. (Edinburgh, 1843), Vol. II, p. 493.

[16] Heraclitus, frags. 203, 235; Parmenides,

long, *methodus* and *philosophia* began to diverge. Generally speaking, while *philosophia* and its sister, *theoria*, tended to stress the arduous difficulties awaiting those who sought truth, the devotees of *methodus* began to emphasize the economy of being methodical, that is, of faithfully following a prescribed sequence of mental steps, a "straight road" in Descartes' phrase.[17] The old metaphor of the "way" was subtly altered and became associated with the advantages of adhering to a beaten path rather than "blazing" a trail. A premonition of this change appeared in the Middle Ages when *methodus* tended to acquire the connotation of a "short-cut." It found popular expression in numerous attempts to compose *compendia* on various subjects.[18]

During the Middle Ages and well into the sixteenth century the idea of method remained encumbered by Aristotelian and scholastic logic. As a result, method was tightly bound by logical procedures whose main aim was to sift and order inherited knowledge and experience rather than to discover new things. Thus the two main procedures of scholastic logic were "invention" (*inventio*), or the methods by which contestable propositions could be analyzed pro and con, and "judgment" or "disposition" (*iudicium*), which comprised the methods of arranging words into propositions, then into syllogisms or inductions, and finally into whole discourses. The conservatory quality of method was illustrated in a sixteenth-century work, *The Rule of Reason*, written by Thomas Wilson and published in 1551. Declaring that "a reason [is] easier found than fashioned," he compared the logic of "invention" with the sort of traditional lore acquired by huntsmen, saying that "he that will take profite in this parte of logique, must be like a hunter, and learne by labour to knowe the boroughes. For these places [i.e., a marke whiche gieuth warnyng to our memory what we maie speake probablie] be nothyng els but couertes or boroughes, wherein if any one searche diligentlie, he maie fynd game at pleasure." In his definition of "method," Wilson clearly expressed the view of one who saw it primarily as an ordering and clarifying procedure, "the maner of handeling a single Question, and the readie waie howe to teache and sette forth any thyng plainlie, and in order, as it should be, in latine *Methodus*."[19]

Throughout the sixteenth century method continued to be thought of mainly in organizational terms. Petrus Ramus, the most influential writer of the period, reflected this tendency. "Method," according to his definition, "is of arrangement, by which among many things the first in respect to conspicuousness is put in the first place, the second in the second, the third in the third, and so on. This term refers to every discipline and every dispute. Yet it commonly is taken in the sense of a direction sign and of a shortening of the highway."[20] Despite the static nature of Ramus's conception, there was some anxiety about "the new devised aid." With his customary irony, Richard Hooker entered some reservations:

> Of marvellous quick despatch it is, and doth shew them that have it as much almost in three days, as if it dwell threescore years with them . . . Because the curiosity of man's wit doth many times with peril wade farther in the search of things than were convenient; the same is thereby restrained unto such generalities as every where offering themselves are apparent unto men of the weakest conceit that need be. So as following the rules and precepts thereof, we may define it to be, an Art which teacheth the way of speedy discourse, and restraineth the mind of man that it may not wax over-wise.[21]

Scarcely a generation later the restraints were rejected and Descartes introduced a new "way of speedy discourse" that promised to make men "the lords and possessors of nature."[22] The crucial step between Hooker and

frags. 342, 344–7. G. S. Kirk and J. E. Raven, *The Presocratic Philosophers* (Cambridge: Cambridge University Press, 1957). The idea reappears in Machiavelli, *Discorsi*, Bk. I, Preface; Tocqueville, *Oeuvres complètes*, ed. J.-P. Mayer (Paris: Gallimard, 1961–), Vol. I, p. 293.

[17] *Discourse on Method*, tr. J. Veitch, *The Method, Meditations, and Philosophy of Descartes* (New York: Tudor Publishing Co., n.d.), Pt. I, p. 149.

[18] See W. Ong, *Ramus. Method and the Decay of Dialogue* (Cambridge, Mass.: Harvard University Press, 1958), p. 53ff.; N. W. Gilbert, *Renaissance Concepts of Method* (New York: Columbia University Press, 1960), p. 3ff.

[19] All quotations are from W. S. Howell, *Logic and Rhetoric in England, 1500–1700* (New York: Russell and Russell, 1961), pp. 21, 23–24. The Ramist influence upon the American Puritans has been discussed by Perry Miller, *The New England Mind, The Seventeenth Century* (Boston: Beacon Press, 1939, 1961), p. 154ff. and Appendix A.

[20] Howell, *op. cit.*, p. 152.

[21] *Of the Laws of Ecclesiastical Polity*, 2 vols. (Oxford: Clarendon Press, 1885), I.vi.4.

[22] *Discourse on Method* (Veitch trans.), Pt. VI, p. 192.

Descartes had been taken by Bacon who developed a distinction between two kinds of *inventio*, one a technique for the discovery of things not previously known, the other for the rediscovery of something previously known but temporarily forgotten.[23] Rightly understood, method promised not only "the use of knowledge" but, above all, "the *progression* of knowledge."[24]

With the gradual development of the idea of method, its significance soon extended beyond the simple advantages of economy and efficiency of mental effort. In following a shortcut, the mind was literally "conducting" inquiry, that is, comporting itself in a special way, following a code of intellectual conduct which, while it might not automatically lead to new truths, would for the most part prevent the methodist from wandering into grievous errors. Thus method came to mean, among other things, a form of discipline designed to compensate for unfortunate proclivities of the mind. "I am indeed amazed," Descartes exclaimed, "when I consider how weak my mind is and how prone to error."[25]

Descartes was among the first to realize that the adoption of the methodical point of view was at least as important as the acquisition of specified techniques. To adopt a method was not equivalent to buying a new suit, to a transaction in which only the external appearance of the purchaser was altered. It was, instead, a profound personal choice, perhaps the closest functional equivalent to conversionary experience that the modern mind can achieve. At the very least, it was intended as a form of reeducation, as one of Descartes' works, *Regulae ad directionem ingenii*, implied. The educational force of the title has been partially lost in translation, *Rules for the Direction of the Mind. Ingenium* carries the meaning of "nature, character, temperament," rather than the more narrowly intellectualistic connotations of "mind." That work described the specific steps for conditioning and disciplining the *ingenium* of the novice, for "rendering [it] more apt in the discovery of yet other truths." The human tendency "to guess unmethodically, at random," not only produced error but mental flabbiness as well. "In so proceeding we are bound to weaken the mind's powers of insight" and, therefore, a strict program was required. "We ought to train ourselves first in those easier matters, but methodically. Thereby we shall accustom ourselves to proceed always by easy and familiar paths, and so, as easily as though we were at play, to penetrate ever more deeply into the truth of things."[26]

The celebrated Cartesian principle of doubt formed a vital part of the new regimen for the mind. Doubt was the means of preparing the mind for *regulae* by first depriving it of the major forms of resistance. Bacon, anticipating the difficulty, had noted that "a new method must be found for quiet entry into minds so choked and overgrown" that only an *expurgatio intellectus* would suffice.[27] Radical doubt was Descartes' version of the purge. Before the mind could proceed methodically, it must be turned upon itself, stripping off acquired habits, beliefs, and values until compelled to face the primordial truth of the *cogito* whose *sum* now stood divested of its cultural heritage in an ahistorical silence. " . . . Those who have learned the least of all that has hitherto been distinguished by the name of philosophy are the most fitted for the apprehension of truth."[28] What Bacon had exultingly proclaimed earlier, "I have purged and swept and levelled the floor of the mind,"[29] had now been programmed by Descartes.

Descartes attached certain self-denying ordinances to his program that are not without interest in the light of the recent evolution of political science. He singled out some subjects, God among them, as privileged and, therefore, protected from the destructive effects of doubt and methodical probing. He cautioned especially against bringing the new method to bear upon questions of morality and practical action. He himself had decided to accept existing moral values as a "provisional code" before submitting all else to doubt, "lest," as he explained, "I should remain irresolute in my actions . . ." More tellingly, since conflicting opinions often existed about what was right, he

[23] *Works of Francis Bacon*, ed. R. L. Ellis, J. Spedding and D. D. Heath, 7 vols. (London: 1887–92), Vol. VI, pp. 268–269.

[24] *Ibid.*, p. 289. "We know that the founders [of New England] studied Francis Bacon." Miller, *op. cit.*, p. 12.

[25] *Meditations*, II in *Descartes. Philosophical Writings*, tr. N. K. Smith (New York: Random House, 1956), p. 189.

[26] Rules X–XI (tr. N. K. Smith), pp. 43–44, 47.

[27] Cited in P. Rossi, *Francis Bacon. From Magic to Science*, tr. S. Rabinovitch (London: Routledge, 1968), p. 141.

[28] "Preface to the Principles of Philosophy" (tr. Veitch), p. 288.

[29] *Francis Bacon. Selected Writings*, ed. H. G. Dick (New York: Random House, 1955), pp. 435, 533. See also Descartes, *Discourse on Method* in N. K. Smith (ed.), *Descartes. Philosophical Writings*, Pt. IV, p. 118.

would regulate "[his] conduct in conformity with the most moderate opinions, those furthest removed from extremes . . ."[30] On political matters he was equally cautious, but more ambivalent. On the one hand, he expressed great admiration for those political societies which exhibited the rational symmetry legislated by a single intelligence; on the other, he abstained from drawing practical conclusions from this, saying only that most societies manage to work tolerably well over the long-run.[31]

Although these political remarks underscore Descartes' preference for rational method over inherited knowledge, they are mainly significant for revealing the reason for his support of the *status quo:* fear of disorder. He was convinced that upheaval invariably followed fundamental reform and that innovators should be warned away:

> Great public institutions, if once overthrown, are excessively difficult to re-establish, or even to maintain erect if once seriously shaken; and their fall cannot but be very violent.[32]

From a preference for the existing scheme of institutions and for "the most moderate" morals it was easy to pass to an identification of the two so that existing arrangements were taken to be the expression of what was reasonable and "furthest removed from extremes." Such a political world snugly fits the methodist's need, not only for the security it provides for his investigations, but also for the assured regularities it gives him to investigate.

What sort of political commitment is likely from a self which has been purged of inherited notions, pledged to the support of existing political and moral schemes, yet inhibited by the belief that they are "provisional"? A self of this type is likely to treat politics and morals in a way that avoids fundamental criticism as well as fundamental commitment. This lack of commitment is connected with the special form which the fear of fundamental change takes with the political methodist. He will boldly renounce any belief in a natural structure of political societies, and declare that "any set of variables selected for description and explanation may be considered a system of behavior. At the outset, whether it is a system given in nature or simply an arbitrary construct of the human mind, is operationally a pointless and needless dichotomy."[33]

Once doubt has abolished all privileged beginnings, there is no compelling reason why *this* rather than *that* should constitute the point of departure or the way of conceiving the problem, just as there is no logical or scientific reason for siding with the status quo. And yet the astonishing culmination of these arbitrary choices is not a truly sceptical temper but, as Descartes frankly admitted, rigidity and single-mindedness.

> My second maxim was to be as unwavering and as resolute in my actions as possible, and having once adopted opinions to adhere to them, however in themselves open to doubt, no less steadfastly than if they had been amply confirmed.

Descartes embellished the point by a contrast between the person who clings steadfastly to a chosen belief and the confused traveller who constantly changes directions. "Even though at the start it may have been chance alone which determined . . . [the] choice of direction," and even though what the resolute person takes to be "very certain and true" may be very doubtful, he is still likely to get *somewhere*, and, at the same time, he most certainly will be relieved from "all the repentings and feelings of remorse which are wont to disturb the consciences" of those who vacillate.[34]

How does the state of contemporary political science compare with the Cartesian philosophy of method? Despite occasional deference paid to "the tradition of political theory," there is a widely shared belief that that tradition was largely unscientific where it was not anti-scientific and that the defining characteristic of a scientific revolution is to break with the past.[35] This animus against tradition will be considered at greater length when we try to assess its significance for the study of politics.

[30] *Philosophical Writings* (ed. Smith), *Discourse on Method*, III, p. 111.

[31] *Ibid.*, II, pp. 103–104, 112.

[32] *Ibid.*, Pt. II, p. 103.

[33] Easton, *A Framework*, p. 30.

[34] *Discourse on Method*, tr. Smith, Pt. III, pp. 111–113.

[35] "It is the very essence of the theoretical enterprise that, if and when it seems appropriate, it should feel free to sever itself from the bonds of traditional ways of looking at political life." Easton, *A Framework*, p. viii.

There is no doubt that breaking with the past has been a feature of all great theoretical innovations, including those in the history of political theory. Yet the matter is not that simple, as witness Plato's respect for tradition, Aristotle's deference to his predecessors, Augustine's retrieval of major aspects of classicism, and Machiavelli's insistence on restoring certain forms of classical political knowledge. Hobbes was probably the first writer to advocate a break in the modern sense. Some aspects of his attempt will be discussed in my forthcoming essay, *Hobbes: Political Theory as Epic* (University of California Press).

Here we are concerned with Descartes' view of politics and especially with his counsels about political change. It is easier and safer, he declared, to reconstitute the foundations of knowledge than to attempt "the slightest reformation in public affairs."[36] An echo in contemporary political science is the following:

> A political system is an accident. It is an accumulation of habits, customs, prejudices, and principles that have survived a long process of trial and error and of ceaseless response to changing circumstance. If the system works well on the whole, it is a lucky accident—the luckiest, indeed, that can befall a society . . . To meddle with the structure and operation of a successful political system is therefore the greatest foolishness that men are capable of. Because the system is intricate beyond comprehension, the chance of improving it in the ways intended is slight, whereas the danger of disturbing its workings and setting off a succession of unwarranted effects that will extend throughout the whole society is great.[37]

It might be objected that many contemporary political scientists would disavow this formulation as extreme and would draw attention to their repeated efforts at reform. Without wishing to deprecate these efforts, the contention remains that most proposals for reform on the part of political scientists represent a narrow range of alternatives founded on the assumption that the system has no inherent defects, or if it has, that these are acceptable "costs." The result is to foreclose a genuinely theoretical discussion which would seriously question and reflect upon the qualities of the system as a whole. Accordingly, the political scientist tends to follow the Cartesian path of extolling the existing as "the most moderate" or "further removed from extremes," and then defending it as though it were "very certain and true." This has taken the by now familiar form of identifying the American political system with "normal politics" and then seeking to establish by empirical methods the factors which produce it. There then follows the general explanation that the system has functioned normally, i.e., in a stable way, because it has avoided immoderation, i.e., "extremism" or "intensity." America has been spared these evils, it is alleged, not because of the excellence of her institutions or her citizens, but because of such factors as: the absence of ideological conflicts and political passions, a healthy amount of voter apathy, a measure of voter ignorance, political parties whose genius is to abstain from presenting clearly defined alternatives, the influence of cross-pressures which fragment the citizen's loyalties and reduce his commitments to the consistency of Jello, a strategy of decision-making which favors "small or incremental change" because it is not disruptive,[38] and a system where the access to power succeeds in keeping at bay the poor, ignorant, deviant, and deprived.

III. CONSEQUENCES OF "METHODISM" IN CONTEMPORARY POLITICAL SCIENCE

It would be easy, especially at the present time when attacks upon liberal pluralism are increasing, to dismiss as an unfortunate lapse the way in which contemporary political science has come to such good terms with American politics. To accuse political science of an ideological bias is not to explain why it succumbed to the bias, or whether the nature of political science in America is or has always been such that identification with the going scheme of things is a recurrent temptation. Only a superficial view would hold that the condition of American political science can be remedied merely by substituting an opposing ideology. Perhaps the problem is far more deeply rooted in the past of American political science and American political society itself. If this should be the case, it would be mere *ante bellum* nostalgia to attempt to return to the state of political science before the behavioral revolution. If such an attempt were made, it is likely that both political science and political theory would be found similarly tainted.

To expose the common root of a problem as vast as this is patently beyond our present scope, but a suggestion as to its nature is perhaps possible. Two assertions by Tocqueville supply the starting-point. The first is: "Hardly anyone in the United States devotes himself to the essentially theoretical and abstract portion of human knowledge." The second is: "Among democratic nations . . . the woof of time is every instant broken and the track of generations effaced."[39] These may be characterized as a diffidence toward theory and history. Rather than attempt to trace the course of this diffidence, let us try to suggest how it is manifested in contemporary political science, but remembering that today's political science is remarkable for its Cartesian methodism and for its protestations about the importance of theory

[36] *Discourse on Method*, Veitch tr., Pt. II, p. 158.

[37] Edward C. Banfield, "In Defense of the American Party System," *Voting, Interest Groups, and Parties*, ed. B. Seasholes (Glenview, Ill.: Scott, Foresman, 1966), p. 130.

[38] D. Braybrooke and E. Lindblom, *A Strategy of Decision* (New York: Free Press, 1963), p. 73.

[39] *Democracy in America* (ed. P. Bradley), 2 vols. New York: Knopf, 1945), Vol. II, pp. 42, 99.

as a guide for empirical research. The possibility to be explored is whether the age-old problem of America, its suspicion of theory and of the human past, has not been worsened by the behavioral revolution, especially in the domain of education.

The first methodistic act for the Cartesian was to purge the self of the opinions acquired by upbringing, education, and common experience. The contemporary methodist performs the same act of divestment, except that he will use the language of social science in order to explain that he must, as far as possible, rid the mind of biases and preconceptions, such as those produced by class, status, occupation, family, religious upbringing, or political attachments. In so doing, he is performing a true ritual, the reenactment of the archetypal American experience of breaking with the past. Or, if this seems too esoteric, perhaps the purged methodist is merely a footnote to Tocqueville's remark that "America is one of the countries where the precepts of Descartes are least studied and are best applied."[40]

This anti-traditionalist bias, cultivated in the name of the elimination of bias, has manifested itself on numerous occasions during the past decades as the effort to diminish the significance of "traditional political theory," as it has revealingly been called. Some have wished to have it eliminated entirely from the education of political scientists, while others have been mainly concerned to substitute a more scientific version of theory, and still others have wished to rescue individual "propositions" from the corpus of the ancient literature and submit them to operational testing. Leaving aside the criticisms which are anti-theoretical in principle, the other responses are interesting because what they are objecting to is not "theory" but a tradition of theory. Stated differently, what is bothersome about the history of theory is that it displays the working out of an inherited form, which is what a tradition is all about. Political theory has been perhaps the only field of study in all of American political science to exhibit this peculiar feature. Moreover, since the vast bulk of the literature which composes the tradition is European, as well as ancient, it is not difficult to see why it should be an object of suspicion.

This same bias is also manifested against the traditional forms of knowledge to which the methodist falls heir when he chooses to become a student of politics. As an ancient field of study, political science has acquired considerable knowledge about laws, constitutions, institutions, and unwritten practices. This inherited knowledge evokes a typically Cartesian and American response:

> . . . traditional methods [of political science]—i.e., history writing, the description of institutions, and legal analysis—have been thoroughly exploited in the last two generations and now it seems to many (including myself) that they can produce only wisdom and neither science nor knowledge. And while wisdom is certainly useful in the affairs of men, such a result is a failure to live up to the promise in the name of political *science.*[41]

Although one might be troubled by the kind of human concern which would provoke a confrontation between "political wisdom" and "political *science,*" the antithesis has the merit of opening the question, What is political wisdom? Put in this vague form, the question is unanswerable, but it may be reformulated so as to be fruitful. The antithesis between political wisdom and political science basically concerns two different forms of knowledge. The scientific form represents the search for rigorous formulations which are logically consistent and empirically testable. As a form, it has the qualities of compactness, manipulability, and relative independence of context. Political wisdom is an unfortunate phrase, for, as the quotation suggested, the question is not *what* it is but *in what* does it inhere. History, knowledge of institutions, and legal analysis were mentioned. Without violating the spirit of the quotation, knowledge of past political theories might also be added. Taken as a whole, this composite type of knowledge presents a contrast with the scientific type. Its mode of activity is not so much the style of the search as of reflection. It is mindful of logic, but more so of the incoherence and contradictoriness of experience. And for the same reason, it is distrustful of rigor. Political life does not yield its significance to terse hypotheses, but is elusive and hence meaningful statements about it often have to be allusive and intimative. Context becomes supremely important, for actions and events occur in no other setting. Knowledge of this type tends, therefore, to be suggestive and illuminative rather than explicit and determinate. Borrowing from Polanyi, we shall call it "tacit political knowledge."[42]

The acquisition of tacit political knowledge is

[40] *Ibid.*, Vol. II, p. 3.

[41] W. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962), p. viii, emphasis in the original.

[42] M. Polanyi, *Personal Knowledge* (New York: Harper and Row, 1964), *passim.*

preeminently a matter of education of a particular kind and it is on this ground that the issue needs to be joined with the political methodist. The mentality which is impatient of the past and of traditional political theory is equally curt with the requirements of tacit political knowledge which is rooted in knowledge of the past and of the tradition of theory. The knowledge which the methodist seeks is fairly characterized in his own language as composing a "kit of tools" or a "bag of tricks." To acquire knowledge of techniques is no small matter, for they are often difficult and require considerable "retooling," which is to say that they imply a particular kind of program of instruction in specific methods.

Tacit political knowledge, on the other hand, accrues over time and never by means of a specified program in which particular subjects are chosen in order to produce specific results. Whatever may be the truth of the adage that he who travels lightest travels farthest, diverse, even ill-assorted baggage, is needed because the life of inquiry preeminently demands reflectiveness, that is, an indwelling or rumination in which the mind draws on the complex framework of sensibilities built up unpremeditatedly and calls upon the diverse resources of civilized knowledge. But if the life of inquiry is narrowly conceived as the methodical "pursuit" of knowledge, it is likely to become not a pursuit but an escape from the spare and shabby dwelling which Descartes literally and symbolically occupied when he composed his *Meditations*. Even those who would wish to address their minds to "data" are aware that data are constituted by abstractions, and that usually what has been culled from the phenomena are the subtle traces of past practices and meanings which form the connotative context of actions and events.

To recognize the connotative context of a subject matter is to know its supporting lore; and to know the supporting lore is to know how to make one's way about the subject-field. Such knowledge is not propositional, much less formulary. It stands for the knowledge which tells us what is appropriate to a subject and when a subject-matter is being violated or respected by a particular theory or hypothesis. Although appropriateness takes many forms, and we shall return to some of them, it is impossible to reduce its contents to a check-list of items. For example, can we say with exactness what is the precise knowledge which makes us uneasy with statements like the following?

> The interesting issues in normative political theory are in the end generally empirical ones. . . There does exist, however, *one interesting problem in political theory* which is strictly normative. That is the problem of evaluating mixes of desiderata. . . . It may be called the 'utility problem' or in still more modern terminology, the 'dynamic-programming problem.' . . . On this strictly normative problem of program packages more progress has been made in the past half century than in all the previous 2,000 years of political theory put together.[43]

Although these assertions may appear absurd, it is not easy to say why, except that some important political and theoretical questions are being rendered unrecognizable. Behind the assertions, however, lie some revealing attitudes towards knowledge. These bear upon the contrast between methodistic knowledge and the forms of theory congenial to it, and, on the other hand, the kind of knowledge characteristic of tacit political knowledge and the forms of theory built upon it. The methodistic assumption holds that the truth of statements yielded by scientific methods has certain features, such as rigor, precision, and quantifiability. The connection between the statements and their features is intimate so that one is encouraged to believe that when he is offered statements rigorous, precise, and quantifiable, he is in the presence of truth. On the other hand, an approach to the "facts" consisting of statements which palpably lack precision, quantifiability, or operational value is said to be false, vague, unreliable, or even "mystical." In actuality, the contrast is not between the true and the false, the reliable and the unreliable, but between truth which is economical, replicable, and easily packaged, and truth which is not. Methodistic truth can be all these things because it is relatively indifferent to context; theoretical truth cannot, because its foundation in tacit political knowledge shapes it towards what is politically appropriate rather than towards what is scientifically operational.

Questions concerning appropriateness, context, and respect for a subject do not concern effete matters, but very practical ones. They involve the resources, or the lack thereof, which we draw upon when the decision concerns matters for which there can be no certitude. What "belongs" to a given inquiry is one such matter, and how to decide between one theory and another, or between rival methods, are others. Yet the kind of knowledge necessary to these decisions, tacit political knowledge, is being jeopardized by the education increasingly being instituted among political scientists. To illustrate the problem, we might consider the implications for tacit political knowledge of a

[43] Pool, *op. cit.*, pp. 23–24 (emphasis added).

typical proposal for increasing the student's mastery of methods. Our example is a recent volume on survey research methods for undergraduates and graduates in political science. In the spirit of Descartes' *regulae*, the authors describe it as a "handbook" or "manual," "a check-list" or inventory of "do's and don'ts," whose aim is to encourage the "empirical emphasis" in political science. Not content with offering a manual of technical instruction, the authors claim advantages of an educational and vocational kind will be promoted if survey research is made part of the curriculum. Thus the instructor, impaled by the twin demands of teaching and research, is reassured that the two can be reconciled if students are put to work learning survey methods while conducting his research. Further, the method is extolled as a way of overcoming the shortcomings of "the lone scholar" whose skills are inadequate for dealing with the size and range of problems confronting empirical political science. The imperative, "resources must be increased," decrees that the lone scholar be replaced by "group activity and teamwork." In the same vein, it is claimed that "the educational advantages for students are impressive" and among the putative advantages are the acquisition of an *ingenium* with traits congenial to the new emphasis:

> . . . students gain the opportunity to learn more about themselves . . . Too few students get the experience of fighting to remain neutral while carefully probing attitudes hostile to their own. Such instruction in self-control is valuable for the headstrong and overprotected.

Despite the tenor and direction of this conception of education, it is insisted that the new generation of students will be able to do "what was not expected of the previous generation of college students—i.e., to discover new knowledge as well as to acquire old."[44]

But will they? As for acquiring "the old," the authors bemoan the fact that political science departments have been hampered by the "lack of knowledge of research skills" and that the conventional academic calendar does not afford sufficient time for students to learn "sampling, interviewing, coding, analysis, etc." Exactly how the student will "acquire the old" when the demands of the "new" are so great is not discussed. In this connection it is relevant to recall Kuhn's description of the way scientific education has been affected by this determination to consolidate scientific advances and insure cumulative knowledge. He characterized scientific education as "narrow and rigid . . . probably more so than any other except perhaps in orthodox theology." It is not well designed "to produce the man who will easily discover a fresh approach," but it is admirably suited for preparing "normal scientists" and for enabling the community to readapt itself when a fundamental change occurs in theoretical orientations. " . . .Individual rigidity is compatible with a community that can switch from paradigm to paradigm when the occasion demands."[45] To the best of my knowledge, political scientists who otherwise approvingly cite Kuhn have consistently declined to take up the implications of his analysis of scientific education.

Although the invention of methods, like the invention of theories, demands a high order of creativity and is entitled to the highest praise something important, perhaps ironical, occurs when that discovery is institutionalized in a training program. The requirements for those who are to use the theory or method are very different from the talent which discovered them, although, paradoxically, the technical skills may be the same. Descartes noted that a child might become as proficient as the genius in following the rules of arithmetic, but he never argued that the child could discover the rules. This is so, not simply because of the chance element in discovery, but because of the more baffling questions of the personal and intellectual qualities of the discoverer and of the cultural conditions of discovery.[46]

In this context the contemporary methodist's notion of training becomes significant. The idea of training presupposes several premeditated decisions: about the specific techniques needed and how they will be used; about what is peripheral or irrelevant to a particular form of training; and about the desired behavior of the trainee after he has been released from his apprenticeship. The idea of theorizing, on the other hand, while it presupposes skills, cannot specify briefly and simply the skills needed, their degree, or combinations. Kepler's followers could be contemptuous of their master's Platonism and astrology, as Newton's admirers were of his religious fascinations; but it would be risky to discount the influence of these extra-scientific considerations upon the formation of the respective theories.

[44] C. H. Backstron and G. D. Hursh, *Survey Research* (Evanston, Ill.: Northwestern University Press, 1963), ed. J. Robinson, pp. xi-xv, 4, 13.

[45] T. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 165.

[46] R. Taton, *Reason and Chance in Scientific Discovery*, tr. A. J. Pomerans (New York: Science Editions, 1962), p. 64ff.

The impoverishment of education by the demands of methodism poses a threat not only to so-called normative or traditional political theory, but to the scientific imagination as well. It threatens the meditative culture which nourishes all creativity. That culture is the source of the qualities crucial to theorizing: playfulness, concern, the juxtaposition of contraries, and astonishment at the variety and subtle interconnection of things. These same qualities are not confined to the creation of theories, but are at work when the mind is playing over the factual world as well. An impoverished mind, no matter how resolutely empirical in spirit, sees an impoverished world. Such a mind is not disabled from theorizing, but it is tempted into remote abstractions which, when applied to the factual world, end by torturing it. Think of what must be ignored in, or done to, the factual world before an assertion like the following can be made: "Theoretical models should be tested primarily by the accuracy of their prediction rather than the reality of their assumptions."[47] No doubt one might object by pointing out that all theorizing does some violence to the empirical world. To which one might reply, that while amputations are necessary, it is still better to have surgeons rather than butchers.

It is not enough, therefore, to repeat commonplaces, viz., that facts are senseless without theoretical concepts, or that the meaning which facts acquire from a theory is purchased at the price of shaping the facts by the theoretical perspective employed. It is not enough because so much depends upon the kind of theory being used and the personal and cultural resources of the user. Perhaps it is some debilitating legacy of Puritanism that causes us to admire "parsimony" in our theories when we should be concerned that the constitution of the factual world depends upon the richness of our theories which, in turn, depends upon the richness of the inquiring mind. This concern may well be what fundamentally unites the scientific theorist and the so-called traditional theorist.

When a scientist observes a fact, he "sees" it through concepts which are usually derived from a theory. Facts are, as one philosopher has neatly put it, "theory-laden." Kepler, for example, observed many of the same facts as his predecessors, but because he viewed them differently a new era of science was ushered in.[48] The same might be said of Machiavelli, as well as of every major theorist from Plato to Marx. Some theorists, as Tocqueville suggested, see differently, others see farther. All would probably have agreed with Tocqueville that, for the theorist, nothing is more difficult to appreciate than a fact,[49] and nothing, it might be added, is more necessary as a condition for theorizing than that facts not be univocal. If they were, creativity and imagination would play a small role and it would be appropriate to speak of theorizing as a banal activity, as "theory-construction." If facts were simply "there" to be collected, classified, and then matched with a theory (or with the observation-statements derived from it), the political scientist might well declare, "Whether [a] proposition is true or false depends on the degree to which the proposition and the real world correspond."[50] But although everyone is ready to acknowledge that facts depend upon some criteria of selection or of significance, what is less frequently acknowledged is that such criteria usually turn out to be fragments of some almost-forgotten "normative" or "traditional" theory.

Because facts are more multi-faceted than a rigid conception of empirical theory would allow, they are more likely to yield to the observer whose mental capacities enable him to appreciate a known fact in an unconventional way. As one philosopher has said, "Given the *same* world it might have been construed differently. We might have spoken of it, thought of it, perceived it differently. Perhaps facts are somehow moulded by the logical forms of the fact-stating language. Perhaps these provide a 'mould' in terms of which the world coagulates for us in definite ways."[51] Once again we are confronted by the warning that the richness of the factual world depends upon the richness of our theories: "The paradigm observer is not the man who sees and reports what all normal observers see and report, but the man who sees in familiar objects what no one else has seen before."[52] Thus the world must be supplemented before it can be understood and reflected upon.

Vision, as I have tried to emphasize, depends for its richness on the resources from which it can draw. These extra-scientific considerations may be identified more explicitly as the stock of ideas which an intellectually curious and broadly educated person accumulates and which come to govern his intuitions, feelings,

[47] A. Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), p. 21.

[48] N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1965), p. 5ff.

[49] The remarks from Tocqueville are to be found in *Oeuvres complètes*, Vol. I, pp. 12, 14, 222.

[50] R. Dahl, *Modern Political Analysis* (Englewood Cliffs, N. J.: Prentice-Hall, 1963), p. 8

[51] Hanson, *op. cit.*, p. 36.

[52] *Ibid.*, p. 30.

and perceptions. They constitute the sources of his creativity, yet rarely find explicit expression in formal theory. Lying beyond the boundaries circumscribed by method, technique, and the official definition of a discipline, they can be summarized as cultural resources and itemized as metaphysics, faith, historical sensibility, or, more broadly, as tacit knowledge. Because these matters bear a family resemblance to "bias," they become sacrificial victim to the quest for objectivity in the social sciences. If scientists have freely acknowledged the importance of many of these items,[53] how much more significant are these human creations for the form of knowledge, political science, which centers on the perplexities of collective life, on objects which are all too animate in expressing their needs, hopes, and fears.

Doubtless the objection will be raised that if a discipline is to be empirical its practitioners must be equipped to "handle" data in ways approximative of the sciences which have been more successful, and that to suggest otherwise is to consort with the heresy of saying that philosophical and moral knowledge may lead to a better empiricism. Yet we might consider the following.

Throughout the history of political theory a student will find a preoccupation with the phenomenon of "corruption." Today, however, we scarcely know how to talk about it,[54] except when it flourishes in non-Western societies. Yet it is a common and documented fact that "organized crime" exerts significant power and influence, controls enormous wealth, and exhibits many of the same features which ordinarily arouse the interest of political scientists, e.g., organization, authority, power, kinship ties, rules, and strong consensus. Despite the promising research possibilities, no textbook on American government provides a place for organized crime in "the system," no study of "polyarchy" or community-power has taken cognizance of it. It is not far-fetched to suggest that this empirical oversight is connected with the belief that moral knowledge is empirically irrelevant.

Or, to take another example, one can think of many fine empirical studies which have never been conducted because contemporary political science has substituted the bland, status quo-oriented concept of "political socialization" for the ancient idea of "political education." If, instead of blinkering the inquiring eye with a postulate that "conduct is *politicized* in the degree that it is determined by considerations of power indulgence or deprivation of the self by others,"[55] we took seriously an old-fashioned hypothesis, such as that advanced by J. S. Mill, that "the first element of good government . . . being the virtue and intelligence of the human beings composing the community, the most important point of excellence which any form of government can possess is to promote the virtue and intelligence of the people themselves,"[56] we might be better sensitized to the importance of genuinely empirical studies of truly fundamental political concern. For example, think of the empirical richness of an inquiry into the current structure of income taxes, especially in terms of the moral and political implications it holds for civic education. The structure of income taxes is a registry of the power and powerlessness of our social, economic, and ethnic groups; of the official way we rate the value of various social activities by the one standard generally accepted. It is also a system of incentives for behavior that define what is virtuous, unvirtuous, and morally indifferent; and, by tacitly encouraging behavior otherwise deemed blameworthy, encourages the gradual legitimation of that behavior, and thereby shapes what used to be called "the virtue of the citizenry." It would be difficult to imagine a richer field for behavioral inquiry, or one more likely to yield important knowledge about the quality of life in this republic. Yet it remains unharvested because our impoverished understanding of civic

[53] E.g., K. Popper, *The Logic of Scientific Discovery* (New York: Science Editions, 1961), pp. 19, 38.

[54] An exception would be S. Huntington, "Political Development and Political Decay," *World Politics*, Vol. 17 (April, 1965), 386–430. As an illustration of a contemporary way of dealing with the problems the reader is referred to A. A. Rogow and H. D. Lasswell, *Power, Corruption, and Rectitude* (Englewood Cliffs, N. J.: Prentice-Hall, 1963). This work criticizes Acton's epigram, points out how a like animus against power led to the separation of powers doctrine, how the latter frustrates the majority, and how the problem can be handled by organizational and bureaucratic sanctions.

[55] H. D. Lasswell and A. Kaplan, *Power and Society. A Framework for Political Inquiry* (New Haven: Yale University Press, 1950), p. 145.

[56] *Representative Government*, Ch. II (Everyman edition, p. 193). My point would not be affected if political socialization were defined in some other contemporary mode, e.g., learning "roles," or as "a readiness to tolerate outputs that are perceived to run contrary to one's wants and demands. . . . ": Easton, *A Systems Analysis of Political Science*, p. 272.

virtue and education has caused us to neglect the field.

Finally, one cannot help wondering whether political science, having jettisoned "metaphysical" and "normative" preoccupations about justice in favor of research into "judicial behavior" and the "judicial process," are not reaping the results: an inability to address a major phenomenon like the dangerous rash of *political* trials in America today and to reflect upon what these trials signify for the future of the authority and legitimacy of the state.

If the presence or absence of the moral and philosophical element affects the process by which theories constitute the empirical world, the choice among theories would seem to be a serious matter. But again, the contemporary mood trivializes what is involved in a theory's formulation and thereby obfuscates the importance of the choice among rival ways of constituting the world. The following quotation may be extreme but it does disclose the fantasies of the behavioral scientist about theories:

> In a report entitled *Communication Systems and Resources in the Behavioral Sciences*, the Committee on Information in the Behavioral Sciences outlines an ideal system that would in effect provide researchers with a computer analogue of the intelligent, all-informed colleague. Such a colleague would read widely, have total recall, synthesize new ideas, always be accessible, and be sensitive to each researcher's needs . . . The computer based system could respond to an individual's direct request for facts, data, and documentation; it could take the initiative and stimulate the researcher by suggesting new ideas, facts, or literature of interest; it could react intelligently to a scientist's work (analyze its logic, trace implications, suggest tests); and it could help disseminate ideas and provide feedback from the scientific community.[57]

If we can safely assume that choosing a theory or a method is not quite the same as choosing a helpful friend who, as Nietzsche taught, must be worthy of being your enemy, we might want to press the question further. When we choose a theory or a method, are we choosing something momentous, like a self, or something innocuous, like an "intellectual construct" or "conceptual scheme"? or something depersonalized, like "a series of logically consistent, interconnected, and empirically verifiable propositions," or like "a generalized statement of the interrelationships of a set of variables"?

Undoubtedly these characterizations tell us something about the formal features of a theory, but they are deceptive in their parsimony. If the question is slightly reformulated to read, what is the human significance of choosing a theory?, then it becomes evident that much more is involved. Choosing a theory is significant for two conflicting reasons: it initiates new ways of thinking, evaluating, intuiting, and feeling; and it demands a substantial sacrifice in the existing forms of these same human processes. The first point is obvious, the second less so. This is because, like the law of treason, history books tend to be written by the victors and hence the sacrifices which accompany the triumph of a new theory are apt to be overlooked or bathed in a kind of Jacobite nostalgia.

The history of political theory is instructive on this score, for many of the great innovative theorists were highly self-conscious about choosing among theoretical alternatives. They knew that the true drama of theorizing involved offering a theory which could not be accommodated within prevailing values and perceptions of the world. When Hobbes allowed that his readers would be "staggered" by his theory,[58] he was not merely stating the obvious fact that his views concerning religion, authority, rights, and human nature were incompatible with traditional religious and political notions, but the more profound point that unless his readers were prepared to revise or discard those notions, they would not be able to grasp the full meaning of the theory and the theory itself could not become an effective force in the world. The same general assumptions had been made by Plato in his challenge to traditional Greek values and to the democratic ethos of Athens, and by Augustine in his effort to demolish classical notions of history, politics, virtue, and religion. Among more recent writers, none has been as sensitive as Max Weber to the emotional and cultural losses attendant upon the commitment to scientific rationalism.

Where our contemporary way of talking has not obscured the drama and demands of theorizing, it has trivialized them. Theories are likened to appliances which are "plugged into" political life and, since it is the nature of appliances to be under sentence of built-in obsolescence, "theories are for burning," leaving only a brief funereal glow which lights the way to "more scientific theories and more efficient research procedures."[59] If adopting a theory were

[57] *Political Science.* Newsletter of the American Political Science Association, Vol. I, No. 1 (Winter, 1968), p. 25 (col. 1).

[58] *De Cive*, Pref. *ad finem.*

[59] D. Apter, *The Politics of Modernization* (Chicago: University of Chicago Press, 1965), p. x.

equivalent to "trying out an idea," testing an hypothesis, or selecting a technique, there would be little reason to object to treating it casually.

At the very least, a theory makes demands upon our time, attention, energy, and skills. More fundamentally, the adoption of a theory signifies a form of submission with serious consequences both for the adopter and for those who imitate him, as well as for the corner of the world which the theory seeks to change our mind about. A certain sensibility is needed, qualities of thinking and feeling which are not readily formulable but pertain to a capacity for discriminative judgment. Why is this so? To compress the answer severely, in political and social matters we tend to think in one of two ways: in trying to explain, understand, or appraise we may ask, what is it like?; or we may ask, what is appropriate? The first way invites us to think metaphorically, e.g., Hobbes' argument that a representative is like an agent, or the contemporary notion of a political society as a system of communications. Ever since Plato, theorists have recognized the fruitfulness of metaphorical thinking, but they have also come to realize that at certain crucial points a metaphor may become misleading, primarily because the metaphor has a thrust of its own which leads to grotesque implications for the object or events which it is supposed to illuminate. A recent example of this pitfall is provided by Professor Deutsch's *Nerves of Government*, which argues for the concept of a communications system as a useful and proper model for political theory. The argument rests on a combination of metaphors and the success of the argument depends upon a confusion of the two. The first metaphor consists in likening the nature of human thinking and purposive action to the operation of a communications system, e.g., the 'problem of value" is like a "switchboard problem," or "consciousness" is "analogous" to the process of feedback.[60] The second metaphor involves the reverse procedure: a communications system may be treated like a person. Human qualities, such as "spontaneity," "freedom of the will," and "creativity," can be "built into" a machine, and then it becomes possible to propose empirical propositions about society derived from the operations of the machine. But the whole argument depends upon, first, mechanizing human behavior and, second, humanizing mechanical processes. Once this is accomplished, grotesque results follow, e.g., internal rearrangements in a system, or in a person, which reduce goal-seeking effectiveness are described as "pathological" and resemble "what some moralists call 'sin'."[61]

A second way of judging asks, what is appropriate? Appropriateness of judgment cannot be encapsulated into a formula. This is because it depends upon varied forms of knowledge for which there is no natural limit. This dependence is rooted in the basic quest of political and social theory for theoretical knowledge about "wholes" made up of interrelated and interpenetrating provinces of human activity. Whether the primary theoretical task be one of explanation or critical appraisal, the theorist will want to locate "divisions" in the human world and embody them in theoretical form. For example, what aspects of that division which we call "religion" have a significant bearing on the activity called "economic"? Perforce, a political theory is, among many other things, a sum of judgments, shaped by the theorist's notion of what matters, and embodying a series of discriminations about where one province begins and another leaves off. The discriminations may have to do with what is private and what is public, or they may be about what will be endangered or encouraged if affairs move one way rather than another, or about what practices, occurrences, and conditions are likely to produce what states of affairs. The difficulty is the same regardless of whether the theoretical intention is to provide a descriptive explanation, a critical appraisal, or a prescriptive solution. By virtue of their location in a whole, one province shades off from and merges into others: where, for example, does the cure of souls end and the authority of the political order over religion begin? where do the effects of technical education merge into questions about ethics and character? where does the autonomy of administrative and judicial practices start and the "mysteries of state" stop? how much of the impetus for the Crusades is to be assigned to religious motives and how much to political or economic considerations?

If, as Plato suggested long ago, the task of theory is to locate "the real cleavages" in things and to "avoid chopping reality up into small parts" or drawing false boundaries,[62] then the sense of what is appropriate is critical. Given the theorist's preoccupation with wholes, the interconnectedness of human provinces, the values and expectations with which men have invested each of their provinces, and the ulti-

[60] *The Nerves of Government* (Glencoe, Ill.: Free Press, 1963), pp. 94, 98.

[61] *Ibid.*, pp. 91–92.

[62] *Politicus*, 262 b-c.

mate bewildering fact that man is single but his provinces are multiple, a theoretical judgment which, by definition, must discriminate can only be restrained from rendering inappropriate determinations if it is civilized by a meditative culture. To be civilized is not only the quality of being sensitive to the claims and characters of many provinces, but, according to an older definition, rendering what is proper to a civil community.

IV. THE VOCATION OF THE POLITICAL THEORIST

If the preceding analysis has any merit it will have suggested that the triumph of methodism constitutes a crisis in political education and that the main victim is the tacit political knowledge which is so vital to making judgments, not only judgments about the adequacy and value of theories and methods, but about the nature and perplexities of politics as well. Here lies the vocation of these who preserve our understanding of past theories, who sharpen our sense of the subtle, complex interplay between political experience and thought, and who preserve our memory of the agonizing efforts of intellect to restate the possibilities and threats posed by political dilemmas of the past. In teaching about past theories, the historically-minded theorist is engaged in the task of political initiation; that is, of introducing new generations of students to the complexities of politics and to the efforts of theorists to confront its predicaments; of developing the capacity for discriminating judgments discussed earlier; and of cultivating that sense of "significance" which, as Weber understood so well, is vital to scientific inquiry but cannot be furnished by scientific methods; and of exploring the ways in which new theoretical vistas are opened.

For those who are concerned with the history of political theories, the vocation has become a demanding one at the present time. How demanding it is can be seen by glancing at Kuhn's account of the manner in which scientific invention treats its own past.[63] In the formative period of their education students are required to master textbooks rather than to familiarize themselves with the creative writings of the great scientists of the past. The characteristic teaching of scientific textbooks, according to Kuhn, is to show how the great achievements of the past have prepared the way for the present stage of knowledge and theory. As a result, discontinuities are smoothed over, discarded, or unsuccessful theories are assumed to have been inferior, and the idea of methodical progress dominates the entire account.

[63] *Op. cit.*, pp. 162–168.

How easy it is to impoverish the past by making it appear like the present is suggested by the way in which social scientists have lapsed into the same idiom as Kuhn's scientific textbooks. "As Aristotle, the first great behavioral scientist, pointed out a long time ago . . ."[64] or, again, "the behavioral persuasion in politics represents an attempt, by modern modes of analysis, to fulfill the quest for political knowledge begun by the classical political theorists," although it is admitted that classical theory is "predominantly prescriptive rather than descriptive."[65] What seems to have been forgotten is that one reads past theories, not because they are familiar and therefore confirmative, but because they are strange and therefore provocative. If Aristotle is read as the first behavioralist, what he has to say is only of antiquarian interest and it would be far more profitable to read our contemporaries.

What we should expect from a reading of Aristotle is an increase in political understanding. What we should expect from the study of the history of political theories is an appreciation of the historical dimension of politics. The cultivation of political understanding means that one becomes sensitized to the enormous complexities and drama of saying that the political order is the most comprehensive association and ultimately responsible as no other grouping is for sustaining the physical, material, cultural and moral life of its members. Political understanding also teaches that the political order is articulated through its history; the past weighs on the present, shaping alternatives and pressing with a force of its own. At the present time the historical mode is largely ignored in favor of modes of understanding which are inherently incapable of building upon historical knowledge. One of the most striking features of game theory, communications models, and mechanical systems is that in each case the organizing notion is essentially history-less.

The threat to political understanding is not to be denied by arguing that we can substitute more precise functional equivalents for older language or that we can translate older notions into more empirical terms. From time im-

[64] B. Berelson and G. Steiner, *Human Behavior* (New York: Harcourt, Brace & World, 1964), p. 13.

[65] Eulau, *op. cit.*, p. 7. See also G. Almond and B. Powell, *Comparative Politics: A Developmental Approach* (Boston: Little, Brown, 1966), p. 214; H. Alker, *Mathematics and Politics* (New York: Macmillan, 1965), pp. 6–8.

memorial writers have talked of the "burdens" of ruling, the "anguish" of choosing, and the "guilt" of actors who must employ coercion. To assimilate these actions to the calculations of gamesters or to describe them as "decision-making" or "outputs" is to distort both sides of the analogy. If in game-playing, for example, anguish, burdens, and guilt were recurrent features, the whole connotative context surrounding the idea of a game would be lost and nobody would "play." The ancient writer Philostratus once remarked of painting that no one could understand the imitative techniques of the painter without prior knowledge of the objects being represented. But when the attempt is being made to convey knowledge, not by imitative techniques, but by abstract signs and symbols which stand for objects commonly understood, everything depends on whether one truly understands what the symbol means. Does he understand, for example, the kinds of discriminative judgments which have been suspended when the symbol of an "input" is made to stand equally for a civil rights protest, a deputation from the National Rifle Association, and a strike by the U.A.W.? Does he understand that what allows him to discriminate between these "inputs" is a tacit knowledge derived from sources other than systems theory? Again, will he be able to compensate for the fact that systems theory makes it possible to talk about an entire political society without ever mentioning the idea of justice, except in the distorting form of its contribution to "system maintenance"? Is he aware that if one can focus on the American political order as a *system*, he does not have to confront the unpleasant possibility of it as an *imperium* of unsurpassed power. If, in rebuttal, the political scientist claims that the sort of studies referred to above really do presuppose the knowledge which would make political sense out of formal methods, then it is necessary to reply that the contemporary political scientist threatens to chalk around himself a vicious circle: his methods of study presuppose a depth of political culture which his methods of education destroy.

But what of the vocation by which political theories are created rather than transmitted? Testimony that such a vocation has existed is to be found in the ancient notion of the *bios theoretikos* as well as in the actual achievements of the long line of writers extending from Plato to Marx. How shall we understand this tradition as containing an idea of vocation which is relevant both to the challenge raised by the prestige of science and to the contemporary state of political life?

V. NATURE AND ROLE OF EPIC POLITICAL THEORISTS

In what follows I shall develop the thesis that the traditional idea of political theory displays some features which resemble forms of scientific theory, but which, by virtue of their political bearing, are uniquely the properties of political theory. As a way of bringing out the distinctive nature of this vocation, I shall call it the vocation of "epic theorist," a characterization which probably seems pretentious or precious, but which has been selected in order to call attention both to the unusual "magnitudes" of this form of theorizing, and to its distinguishing purpose and style.

Perhaps the pretentiousness of the phrase may be lessened by briefly recalling a comparable conception of theory in Kuhn's work. He employs the phrase "extraordinary" science to describe the contributions of the great scientific innovators. Kuhn's main point is that these theories mark a break with previous ones; that is, they inaugurate a new way of looking at the world, which includes a new set of concepts, as well as new cognitive *and* normative standards. Taking this as a suggestion of how to think about great theories, the first feature shared by epic theorists has to do with magnitudes. By an act of thought, the theorist seeks to reassemble the whole political world. He aims to grasp present structures and interrelationships, and to re-present them in a new way. Like the extraordinary scientific theory, such efforts involve a new way of looking at the familiar world, a new way with its own cognitive and normative standards.[66]

The second aspect of epic theory can be brought out if we look upon a theory not only as a structure of formal features, but also as a *structure of intentions*. The structure of intentions refers to the controlling purposes of the theorist, the considerations which determine how the formal features of concept, fact, logic, and interconnection are to be deployed so as to heighten the effect of the whole. In using the word "purposes" I mean to acknowledge that the structures exhibit considerable variety, and yet I also mean to maintain that there has been a persistent feature in all of them, one which may perhaps seem naive to our age of unmasking where all emperors are naked. All of the major theories of the past were informed by "public concern," a quality which was not incidental to the activity, but fundamental to the

[66] Here it is only necessary to recall Plato's long discussion of cognition or Hobbes's effort to place political philosophy upon a new and more scientific basis.

very notion of being engaged in *political* theory. The cynical "realist," Machiavelli, professed, "I love my country more than my soul."[67] In his dialogue *Utopia* Thomas More wrote emphatically about the theorist's commitment: "If you cannot pluck up wrongheaded opinions by the root, if you cannot cure according to your heart's desire vices of long standing," he declares to the pure political philosopher, represented by Hythloday, "yet you must not on that account desert the commonwealth."[68] Hobbes, who was never one to romanticize men's motives, represented himself as "one whose just grief for the present calamities of his country" had driven him to theorize.[69] Similar sentiments abound in the writings of Plato, Augustine, Locke, Rousseau, Bentham, Tocqueville, and Marx, among others. This unanimity suggests that if a Plato or a Marx had said what the modern scientist says repeatedly, and some social scientists come perilously close to saying, namely, that they are not responsible for the political and social consequences of their inquiries, it would appear more foolish than blameworthy. Concern for *res publicae* and *res gestae* are as irreducible and natural to the vocation of theorist as a concern for health is to the physician. This quality of caring for public things contrasts sharply with the mental set which believes that "the formulation of the topic into a research problem is the first step in scientific inquiry and, as such, should be influenced primarily by the requirements of scientific procedure."[70]

Because history suggests that all political societies have both endured and employed violence, cruelty, injustice, and known the defeat of human aspirations, it is not surprising that the theorist's concern for *res publicae* and the commonweal has issued in theories which, for the most part, have been critical and, in the literal sense, radical. Why this is the case and the import of it for the contemporary vocation of theorist can be shown by referring once more to Kuhn. He has argued that scientific revolutions tend to occur when research begins to turn up persistent "anomalies," i.e., when phenomena are encountered that cannot be squared with the theory. To qualify as an anomaly the phenomena, should, in principle, be explicable by the theory; or, stated differently, the anomaly must be relevant to the kinds of problems for which the theory purports to furnish an explanation. It does not count as an anomaly if it raises a question which the theory cannot be said to recognize as important and hence be expected to answer.

The concept of anomaly suggests that a scientific crisis occurs because something is wrong "in" the theory. When nature does not conform to the scientist's expectations, he reacts by reexamining his techniques and theories. He assumes that the "mistake" lies with one or the other, not with nature. The bearing of this upon political science becomes clearer when we consider some frequent criticisms directed at traditional political theories by contemporary political scientists. It is charged that such theories are useless in explaining voting behavior, political apathy, the formation of politically relevant attitudes, and the precise degree of actual control exercised by the electorate. "If someone were to ask, 'How can I learn about what sorts of people participate most in politics, and why?', I would urge him to start with the most recent studies and work backwards. I seriously doubt whether he would get much help from Aristotle, Rousseau, or the *Federalist Papers*."[71] From such criticisms one would conclude that traditional political theories are valueless because they cannot explain why the political world is as it is. There is, in other words, something "wrong" in the theories. Whether this type of criticism will stand depends upon a prior understanding of the intentions of epical theorists: to what were their theories a response? As we have noted earlier, when it is believed that something is "wrong," scientists look for the error in the theory, not in the world. The same assumption is echoed by a contemporary behaviorist when he writes, "If there is a crisis, then, it is a crisis in the theory of representation and not in the institution of representation."[72] The assumption of the epic theorist has been of a different and contrasting kind. He has been preoccupied with a particular magnitude of problems created by actual events or states of affairs in the world rather than with problems related to deficiencies in theoretical knowledge. To be sure, problems-in-the-world and problems-in-a-theory are often interconnected, but the former has taken precedence among epic theorists and has been determinative of the latter. The shaping experience has been the recurrently problematic state of the political world, not the problematic state of theories about that world. What is

[67] Letter to Vettori, April 16, 1527.

[68] *Utopia*, Bk. I, tr. E. Surtz (New Haven: Yale University Press, 1964), p. 49.

[69] *De Cive*, Preface to the Reader, ed. S. P. Lamprecht (New York: Appleton, 1949), p. 18.

[70] Selltiz, *op. cit.*, p. 31.

[71] Dahl, *Political Analysis*, p. viii.

[72] Eulau in Pool (ed.), *Contemporary Political Science*, p. 55.

problematic emerges when political life is experienced either as a threat or as a promise. Most of the important theories were a response to crisis; they have reflected a conviction either that political action might destroy certain civilized values and practices, or that it might be the means for deliverance from evils, such as injustice or oppression. These polarities can be illustrated by the contrasting responses of Burke and Paine to the French Revolution, or of Tocqueville and Marx to the events of 1848. The point is not that theories come in pairs, or that the "same" events can be viewed very differently and equally persuasively; but rather that epic theories issue not from crises in techniques of inquiry, but from crises in the world.

In the language of theory, crisis denotes derangement. One form of derangement is the result of forces or conditions beyond control, e.g., the plague which hit Athens during its struggle against Sparta and, according to Thucydides, weakened the vital conventions governing Athenian political life. Other kinds of derangement are closer to what Aristotle called contingent matters, that is, matters about which men can meaningfully deliberate and choose. These kinds of derangements are the result of certain types of "errors" or "mistakes": errors in *arrangements*, in *decisions*, and in *beliefs*. Obviously the three types are often interrelated and combined: mistaken beliefs may produce faulty arrangements and foolish decisions; an unwise decision, e.g., one which over-extends the resources of a society, may encourage mistaken beliefs, such as the illusion of omnipotence. Despite their obviousness, these three types may help in clarifying the defining, specific problems of traditional political theory. It is too vague to leave it that theorists are stimulated by problems-in-the-world, and it is misleading to say simply that they are drawn to a class of problems about which something can and should be done. What is all-important is that a problem be a truly theoretical one. A problem such as that presented by the inefficiency of postal services or the ineffectuality of legislative committees may be traceable to errors in arrangement (such as faulty delegations of administrative authority), or to mistaken beliefs (such as that seniority is the most expedient principle for determining committee chairmanships), or to a combination of erroneous arrangements and mistaken beliefs. Without denying the practical importance of these problems, they are not theoretical but technical in nature: they concern the most expeditious means of achieving goals which are, for the most part, agreed on beforehand. Likewise, the question of what decision is proper under particular circumstances is a matter for practical reason or judgment, not for theory.

There is one setting, however, in which specific arrangements, decisions, and beliefs become theoretically interesting. That is when they are "systematically mistaken": when arrangements or decisions appear not as random consequences of a system which otherwise works tolerably well or as the result of the personal foibles of a particular office-holder but as the necessary result of a more extensive set of evils which can confidently be expected to continue producing similar results. Such a system would be systematically deranged. An illustration of what is being argued here is provided by Plato's criticism of Athenian democracy. The main thrust of his criticism was not directed against certain politices which he opposed, or even against the democracy's condemnation of Socrates.[73] Rather, the main thrust was towards arguing that the bad policies and actions were bound to occur in one form or another, because the entire polity was systematically ordered in a mistaken way. Another example is provided by Marx. His case against capitalism did not rest on the charges that it chained the workers to a subsistence level, produced wastefully, and unfairly enriched those who owned the instruments of production. It was aimed, instead, at exposing the logic of capitalism which made injustice, alienation, and exploitation inevitabilities rather than contingencies.

This concept of the *systematically* mistaken explains why most political theories contain radical critiques. Their authors have tried to get at the basic principles (in the sense of starting-points) which produce mistaken arrangements and wrong actions. This same impulse determines why a political theory takes the form of a symbolic picture of an ordered whole. That it is a whole is dictated by its function, which is to be complementary to, or a substitute for, the systematically disordered whole which the theory seeks to displace. The possibility that the factual world is the outcome of a systematically disordered whole produces still another major difference between the epic political theorist and the scientific theorist. Although each attempts to change men's views of the world, only the former attempts to change the world itself. Although the scientist surely may claim for his theories the daring, beauty, and imaginative-

[73] If the Seventh Letter is to be believed, Plato also condemned the government of the Thirty, which included some of his kinsmen, for their threats against Socrates (324 d-e).

ness that are claimed for other forms of endeavor, he will concede that at some point his theory must submit to confirmation by the world. T. H. Huxley spoke sadly of "beautiful" theories tragically murdered by "an ugly little fact." Plato, in contrast, had asked defiantly, "Is our theory any the worse, if we cannot prove it possible that a state so organized should be actually founded?" Epic theory, if it has not strictly attempted to use theory to murder the ugly facts of the world, has taken a very different view of them, refusing to yield to facts the role of arbiter. Facts could never prove the validity of a true theory, because facts, in the form of practices or actions, were "less close to truth than [is] thought."[74] Thus for Plato the political facts of Athenian democracy were perfectly consistent with the theory of democracy, but the theory itself was systematically mistaken in its organizing principles, that is, deranged.

When we turn our attention to political life in the modern states, its appearance seems more suitable to methodical inquiry and mechanical models or theories. Our political and social landscape is dominated by large structures whose premeditated design embodies many of the presuppositions and principles of methodism. They are deliberately fabricated, their processes are composed of defined "steps," and their work is accomplished by a division of specialized labor whose aggregate effect seems marvelously disproportionate to the modest talents which are combined. Not only do these organizations impart regularity and predictability to the major realms of our existence, thereby furnishing the conditions whereby methodical inquiry can pursue its goal of scientifically verifiable knowledge with reasonable hopes of success—for what could be more hopeful than to know that the political and social world is deliberately fashioned to produce regular and predictable behavior?—but also, since these organizations are uniquely the product of mind, rather than of mysterious historical forces, we are able to say with far greater confidence than Hobbes and Vico, who first announced the principle, "we can know it, because we made it."

Yet this is the state of affairs which the greatest modern philosopher of method, Max Weber, foresaw and despaired of, a world of bleak, forbidding, almost sterile reality, dominated by large and impersonal bureaucratic structures which nullified the strivings of those political heroes evoked in "Politics as a Vocation." "A polar night of icy darkness and hardness" was his description of the world to come.[75] In a fundamental sense, our world has become as perhaps no previous world has, the product of design, the product of theories about human structures deliberately created rather than historically articulated. But in another sense, the embodiment of theory in the world has resulted in a world impervious to theory. The giant, routinized structures defy fundamental alteration and, at the same time, display an unchallengeable legitimacy, for the rational, scientific, and technological principles on which they are based seem in perfect accord with an age committed to science, rationalism and technology. Above all, it is a world which appears to have rendered epic theory superfluous. Theory, as Hegel had foreseen, must take the form of "explanation." Truly it seems to be the age where Minerva's owl has taken flight.

It would seem, then, that the world affirms what the leaders of the behavioral revolution claim, the irrelevance of epic theory. The only trouble is that the world shows increasing signs of coming apart; our political systems are sputtering, our communication networks invaded by cacophony. American society has reached a point where its cities are uninhabitable, its youth disaffected, its races at war with each other, and its hope, its treasure, and the lives of its young men dribbled away in interminable foreign ventures. Our whole world threatens to become anomalous.

Yet amidst this chaos official political science exudes a complacency which almost beggars description. It is excusable that a decade ago a political scientist could contend that only a "fanatic" would want to "maximize" political equality and popular sovereignty at the expense of other values, such as leisure, privacy, consensus, stability, and status. But it is less excusable to find the following in a recent collection of papers delivered before the APSA and subsequently published under its imprimatur: "Our discipline is enjoying a new coherence, a pleasant sense of unity, and self-confident identity that fits its rapid growth and healthy mien."[76] Polanyi has remarked that "it is the normal practice of scientists to ignore evidence which appears incompatible with the accepted system of scientific knowledge, in the hope that it will prove false or irrelevant."[77] In this spirit

[74] *Republic* 473 (Conford tr.)

[75] H. Gerth and C. W. Mills, *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), p. 128.

[76] Pool (ed.), *Contemporary Political Science*, p. vii. The essays by Eckstine and Dahl are excepted.

[77] *Personal Knowledge*, p. 138.

American political scientists continue to devote great energy to explaining how various agencies ingeniously work at the political socialization of our citizens and future citizens while mobs burn parts of our cities, students defy campus rules and authorities, and a new generation questions the whole range of civic obligations. And while American political scientists have laboriously erected "incrementalism" into a dogma and extolled its merits as a style of decision-making that is "realistic," it is apparent to all that the society suffers from maladies—the decay of the cities, the increasing cultural and economic gap between our minorities and our majority, crisis in the educational system, destruction of our natural environment —which call for the most precedent-shattering and radical measures.

Amidst all this, a political scientist approvingly quotes the following from a social scientist: "To argue that an existing order is 'imperfect' in comparison with an alternative order of affairs that turns out upon careful inspection to be unobtainable may not be different from saying that the existing order is 'perfect'."[78]

This assertion poses squarely the issue between political theory on the one side and the alliance between the methodist and the empirical theorist on the other. The issue is not between theories which are normative and those which are not; nor is it between those political scientists who are theoretical and those who are not. Rather it is between those who would restrict the "reach" of theory by dwelling on facts which are selected by what are assumed to be the functional requisites of the existing paradigm, and those who believe that because facts are richer than theories, it is the task of the theoretical imagination to restate new possibilities. In terms of theory, the basic thrust of contemporary political science is not antitheoretical so much as it is deflationary of theory. This is most frequently expressed in the anxiety of the behaviorist who discovers that the philosophy of democracy places excessive demands on the "real world" and hence it is the task of political science to suggest a more realistic version of democratic theory. Thus the authors of *The Civic Culture* admit that it would be possible to "explain" the low degree of political involvement on the part of citizens by "the malfunctioning of democracy." But, they caution, this kind of explanation rests on a belief "that the realities of political life should be molded to fit one's theories of politics." A "somewhat easier and probably more useful task," they contend, is suggested by "the view that theories of politics should be drawn from the realities of political life." Because "the standards have been set unreasonably high," the theory should be changed.[79]

Is it possible that in this genial, Panglossian twilight Minerva's owl is beginning to falter as it speeds over a real world that is increasingly discordant and is beginning to voice demands and hopes that are "unreasonably high"? Perhaps it is possible, especially if we remember that according to Greek statuary, Minerva's pet was a screech-owl, for a screech is the noise both of warning and of pain.

[78] Cited by A. Wilkausky, *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964), p. 178.

[79] *Civic Culture*, p. 475.

CONTINUITY AND CHANGE IN AMERICAN POLITICS: PARTIES AND ISSUES IN THE 1968 ELECTION*

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Without much question, the third-party movement of George C. Wallace constituted the most unusual feature of the 1968 presidential election. While this movement failed by a substantial margin in its audacious attempt to throw the presidential contest into the House of Representatives, in any other terms it was a striking success. It represented the first noteworthy intrusion on a two-party election in twenty years. The Wallace ticket drew a larger proportion of the popular vote than any third presidential slate since 1924, and a greater proportion of electoral votes than any such movement for more than a century, back to the curiously divided election of 1860. Indeed, the spectre of an electoral college stalemate loomed sufficiently large that serious efforts at reform have since taken root.

At the same time, the Wallace candidacy was but one more dramatic addition to an unusually crowded rostrum of contenders, who throughout the spring season of primary elections were entering and leaving the lists under circumstances that ranged from the comic through the astonishing to the starkly tragic. Six months before the nominating conventions, Lyndon Johnson and Richard Nixon had been the expected 1968 protagonists, with some greater degree of uncertainty, as usual, within the ranks of the party out of power. The nominating process for the Republicans followed the most-probable script rather closely, with the only excitement being provided by the spectacle of Governors Romney and Rockefeller proceeding as through revolving doors in an ineffectual set of moves aimed at providing a Republican alternative to the Nixon candidacy. Where things were supposed to be most routine on the Democratic side, however, surprises were legion, including the early enthusiasm for Eugene McCarthy, President Johnson's shocking announcement that he would not run, the assassination of Robert Kennedy in the flush of his first electoral successes, and the dark turmoil in and around the Chicago nominating convention, with new figures like Senators George McGovern and Edward Kennedy coming into focus as challengers to the heir apparent, Vice President Hubert Humphrey.

No recent presidential election has had such a lengthy cast of central characters, nor one that was kept for so long in flux. And under such circumstances, there is an inevitable proliferation of "what ifs?" What if Lyndon Johnson had decided to run again? What if Robert Kennedy had not been shot? What if George Wallace had been dissuaded from running, or had remained simply a regional states-rights candidate? What if Eugene McCarthy had accepted party discipline and closed ranks with Humphrey at the Chicago convention? What if Hubert Humphrey had handled the interaction with Mayor Daley and the Chicago demonstrators differently?

Strictly speaking, of course, there is no sure answer to questions of this type. If the attempt on Kennedy's life had failed, for example, an enormous complex of parameters and event sequences would have been different over the course of the campaign. One can never be entirely confident about what would have happened without the opportunity to live that particular sequence out in all its complexity. Nonetheless, given sufficient information as to the state of mind of the electorate during the period in question, plausible reconstructions can be developed which do not even assume that all other things remained constant, but only that they remained *sufficiently* constant that other processes might stay within predictable bounds. And answers of this sort, if not sacrosanct, carry substantial satisfaction.

One of our purposes in this paper will be to address some of these questions, as illuminated by preliminary analyses from the sixth national presidential election survey, carried out by the Survey Research Center of the University of Michigan.[1] An effort to develop answers gives a

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[1] The 1968 national sample survey(N = 1559) was made possible by a grant from the Ford Foundation, whose support we gratefully acknowledge. A total of 1559 citizens of voting age were interviewed, most of them both before and after Election Day. The preliminary nature of this report is to be emphasized, since the data on which it is based had not been fully cleaned at the

vehicle for what is frankly descriptive coverage of the 1968 election as seen by the electorate. At the same time, we would hope not to miss along the way some of the more theoretical insights which the peculiar circumstances of the 1968 election help to reveal. In particular, we shall pay close attention to the Wallace campaign, and to the more generic lessons that may be drawn from this example of interplay between a pair of traditional parties, potent new issues, and a protest movement.

I. THE SETTING OF THE ELECTION

The simplest expectation for the 1968 election, and one held widely until March of that year, was that President Johnson would exercise his option to run for a second full term, and that with the advantages of incumbency and the support of the majority party in the land, he would stand a very good chance of winning, although with a margin visibly reduced from his landslide victory over Barry Goldwater in 1964.

We will probably never know what role public opinion may have actually played in his decision to retire. But there is ample evidence that the mood of the electorate had become increasingly surly toward his administration in the months preceding his announcement. When queried in September and October of 1968, barely 40% of the electorate thought that he had handled his job well, the rest adjudging the performance to have been fair to poor. A majority of Democratic and independent voters, asked if they would have favored President Johnson as the Democratic nominee had he decided to run, said they would not have. Affective ratings elicited just after the election for all the prominent political figures of the campaign showed Johnson trailing Robert Kennedy in average popularity by a wide margin, and lagging somewhat behind Humphrey and Muskie as well, among other Democrats (see Table 2). Given the normal head-start that a sitting president usually enjoys in such assays of opinion, Johnson completed his term amid a public bad humor matched only in recent elections by the cloud under which Harry Truman retired from the presidency in 1952. It is correspondingly dubious that Lyndon Johnson could have avoided the embarrassment of defeat had he set his sails for another term.

Indeed, the pattern of concerns exercising the voters and turnover in the players on the presidential stage combined to produce a shift in popular preferences between 1964 and 1968 which was truly massive. It is likely that the proportion of voters casting presidential ballots for the same party in these two successive elections was lower than at any time in recent American history. Among whites who voted in both elections, a full third switched their party. Almost one Goldwater voter out of every five turned either to Humphrey or to Wallace four years later (dividing almost 3 to 1 for Wallace over Humphrey); at the same time, three in every ten white Johnson voters switched to Nixon or Wallace, with Nixon the favorite by a 4-to-1 ratio. A full 40 percent of Nixon's votes came from citizens who had supported Lyndon Johnson in 1964! Much of this flood, of course, came from Republicans who were returning home after their desertions from Goldwater.

Nevertheless, Democrats and Independents who had voted for Johnson and then turned to Nixon four years later made up nearly half of *all* the remaining vote switches, more than matching the combined flow of Johnson and Goldwater voters who supported Wallace, and almost equalling the total Wallace vote. The Johnson-Nixon switchers easily outweighed the flow away from Goldwater to Humphrey and Wallace, and the Republican presidential vote rose from 39% to 43% in 1968 as a consequence. At the same time, the loss of more than a quarter of the total Johnson vote to Wallace and Nixon was scarcely offset by the trickle of votes from Goldwater to Humphrey, and the Democratic proportion of the vote across the land dropped a shattering 19 percentage points from more than 61 percent to less than 43 percent.

Such a massive drain from the Democratic ranks establishes a broader parallel with 1952, for in both cases an electorate professing to be of Democratic allegiance by a considerable majority, had arrived at a sufficient accumulation of grievances with a Democratic administration as to wish it out of office, thereby producing what we have labelled elsewhere a "deviating election."[2] Indeed, the frantic mo-

time of writing. When the study is released through the Inter-University Consortium for Political Research, interested analysts may discover small discrepancies from the statistics reported here. Readers should also remember that all sample statistics are subject to varying amounts of sampling error in relation to the number of cases on which they are based.

[2] A deviating election is one in which the party commanding the identifications of a majority of the electorate is nonetheless voted out of power temporarily. See A. Campbell, P. Converse, W. Miller, and D. Stokes, *The American Voter* (New York: John Wiley, 1960), Chapter 19.

tion of the electorate in its presidential votes between 1964 and 1968 may be ironically juxtaposed against the serene stability of party identifications in the country, for the overall proportions of self-proclaimed Democrats, Independents and Republicans have scarcely changed over the past twenty years, much less in the past four. Of course this juxtaposition calls into question the predictive value of party identification, relative to other kinds of determinants of the vote, and we shall undertake a more intensive discussion of this matter presently. For now, however, let us simply point out that while the inert distribution of party loyalties cannot by definition explain the complex flows of the presidential vote between 1964 and 1968, it was handsomely reflected in the 1968 congressional elections, as it has been in virtually all of the biennial congressional contests of the current era. Despite widespread dissatisfaction with Democratic performance, the Republican proportion of seats in the House rose only a minute 1 percent, from 43 in 1966 to 44 percent on the strength of the Nixon victory. Even at more local levels, the continuing dominance of Democratic partisanship across the nation is documented by the results of thousands of races for state legislative seats. Prior to the election, Democrats controlled 57.7 percent of all legislative seats. After the election, which saw contests for some or all seats in 43 states, Democratic control had dropped from 4,269 seats (or 57.7%) to 4,250 seats (57.5%).[3]

In view of such continued stability of partisanship, it is clear we must turn elsewhere to account for the remarkable changes in voting at the presidential level between 1964 and 1968. The classic assumption is, of course, that such change must spring from some flux in "short-term forces"—the impact of the most salient current issues, and the way in which these issues interlock with the leadership options, or the cast of potential presidential figures in the specific year of 1968. These terms obviously best define the setting of the 1968 election.

When asked on the eve of the presidential election to identify the most important problem facing the government in Washington, over 40% of the electorate cited the war in Vietnam. The salience of this issue provided another striking parallel with 1952. In both presidential elections, widespread public discouragement with the progress of a "bleeding war" in the Far East, seen as initiated by a Democratic administration, was a major source of indignation.

But the Vietnam issue did not, of course, stand alone. Offering vivid testimony to another bitter current of controversy was a simple, though little-noted, pattern in the popular presidential vote itself: while some 97% of black voters in the nation cast their ballots for Hubert Humphrey, less than 35% of white voters did so. Thus the presidential vote must have been as sharply polarized along racial lines as at any time during American history.[4] One major irony surrounding this cleavage was the fact that it was the comfortable white majority that was agitating to overturn control of the White House, while the aggrieved black minority was casting its vote as one in an effort to preserve the partisan status quo.

Indeed, this irony is compounded when the role of the Vietnam issue is jointly taken into account. We have indicated above that the public was deeply impatient with the Johnson administration, in part because of the handling of the war. Blacks stood out as the major demographic grouping most exercised about the entanglement in Vietnam. They were more likely than whites to opine that the government should never have undertaken the military commitment there. They also were more likely to feel that American troops should be brought home immediately, a position not generally associated with the Johnson administration. Nonetheless, as Table 2 (below) will document, Negro enthusiasm not only for Hubert Humphrey but for Lyndon Johnson as well remained high to the very end. It seems quite evident that when black citizens were making decisions about their vote, Vietnam attitudes paled into relative insignificance by contrast with attitudes toward progress on civil rights within the country; and that where such progress was concerned, the Johnson-Humphrey administration was seen as much more friendly than the other 1968 alternatives.

Because of the near-unanimity of the black vote, many of our analyses below have been focussed on differences within the white vote taken alone.[5] At the same time, this treatment

[3] *Congressional Quarterly*, November 22, 1968, p. 3177.

[4] The percentage difference of 62% in candidate preference between blacks and whites is substantially larger than class differentiation or other social cleavages and partisanship within the United States in recent history or for democracies of Western Europe.

[5] Such segregation is indicated simply because of the fact that within the black vote in 1968 there is next to no meaningful "variance" to be

TABLE 1. COMPARISON OF ATTITUDES ON CURRENT VIETNAM POLICY AND RACIAL DESEGREGATION, 1964 AND 1968, FOR WHITES ONLY

"Which of the following do you think we should do *now* in Vietnam?

1. Pull out of Vietnam entirely.
2. Keep our soldiers in Vietnam but try to end the fighting.
3. Take a stronger stand even if it means invading North Vietnam.

	Pull Out	*Status Quo*	*Stronger Stand*	*Don't Know, Other*	*Total*
			Northern Democrats		
1964	8%	25	29	38	100%
1968	20%	39	35	6	100%
			Northern Republicans		
1964	8%	19	38	35	100%
1968	20%	39	36	5	100%
			Southern Democrats		
1964	8%	25	28	39	100%
1968	17%	36	38	9	100%
			Southern Republicans		
1964	10%	18	42	30	100%
1968	15%	29	48	8	100%

"What about you? Are you in favor of desegregation, strict segregation, or something in between?" (This was the fourth question in a series asking about others' attitudes toward racial desegregation.)

	Desegregation	*Mixed Feelings*	*Strict Segregation*	*Other*	*Total*
			Northern Democrats		
1964	31%	50	17	2	100%
1968	38%	45	14	3	100%
			Northern Republicans		
1964	32%	51	13	4	100%
1968	35%	50	10	5	100%
			Southern Democrats		
1964	12%	35	52	1	100%
1968	18%	45	30	7	100%
			Southern Republicans		
1964	15%	44	40	1	100%
1968	15%	60	20	5	100%

must not be allowed to obscure in any way the deep imprint of racial cleavage on the election outcome. The additional "between-race" variance in the vote, concealed when data are presented only for whites, remains extreme, and is a faithful reflection of the crescendo to which civil rights tumult had risen over the four preceding years. It should be kept in mind.

To say that Vietnam and civil rights were dominant issues for the public in 1968 is not equivalent, however, to saying that voter positions on these issues can account for the large-scale voting change we have observed for whites between 1964 and 1968. As the comparisons provided by Table 1 suggest, changes in public thinking about strategic alternatives in Vietnam or civil rights outcomes over this period were rather limited. Where Vietnam was concerned, opinion was somewhat more crystallized in 1968 than in 1964 but there had been no sweeping shift of sentiment from hawk to dove in mass feeling. On civil rights, the drift of white opinion had been if anything toward a more liberal stance, and hence can hardly explain a vote which seemed to vibrate with "blacklash." Thus public positioning on these two central issues taken alone seems no more capable of illuminating vote change from 1964 to 1968 than the inert partisan identifications.

What *had* changed, of course, was the public view of the success of Administration performance in these areas. As we have discussed elsewhere, throughout the 1950's citizens who felt the Republicans were better at keeping the country out of war outnumbered those who had more confidence in Democrats by a consistently wide margin, much as the Democratic Party tended to be seen as better at keeping the country out of economic depression. In 1964, however, the pleas of Barry Goldwater for an escalation of the Vietnam War in order to produce a military victory served to frighten the public, and rapidly reversed the standing perception: by the time of the November election more people felt the Democrats were better able to avert a large war.[6] But this novel perception was transient. President Johnson himself saw fit to authorize an escalation of bombing in Vietnam almost immediately after the 1964 election. By the time of the 1966 congressional election, the balance in popular assessments had already shifted back to the point where a slight majority chose the Republicans as more adept in avoiding war. By 1968, exasperation at the handling of the war had increased sufficiently that among people who felt there was a difference in the capacity of the two parties to avoid a larger war, the Republicans were favored once again by a margin of two to one.

To the bungled war in Vietnam, the white majority could readily add a sense of frustration at a racial confrontation that had taken on

"accounted for." When categories of "Nixon voters" and "Wallace voters" are presented, they are necessarily "lily-white" in composition. Therefore when "Humphrey voters" are contrasted with them, it is confusing if differences may be totally a function of the large admixtures of blacks in the Humphrey support, as opposed to differences which would stand up even with comparisons limited to whites.

[6] See "Voting and Foreign Policy," by Warren E. Miller, Chapter 7 in James N. Rosenau (ed.), *Domestic Sources of Foreign Policy* (New York: The Free Press, 1967).

increasingly ugly dimensions between 1964 and 1968. Although national opinion had evolved in a direction somewhat more favorable to desegregation, largely through the swelling proportions of college-educated young, some persistently grim facts had been underscored by the Kerner Commission report in the spring of the year: forbidding proportions of the white citizenry outside of the South as well as within it had little enthusiasm for the redress of Negro grievances to begin with. And even among whites with some genuine sympathy for the plight of blacks, the spectacle of city centers aflame had scarcely contributed to a sense of confidence in the Administration handling of the problem.

From Vietnam and the racial crisis a corollary discontent crystallized that might be treated as a third towering issue of the 1968 campaign, or as nothing more than a restatement of the other two issues. This was the cry for "law and order" and against "crime in the streets." While Goldwater had talked in these terms somewhat in 1964, events had conspired to raise their salience very considerably for the public by 1968. For some, these slogans may have had no connotations involving either the black race or Vietnam, signifying instead a concern over rising crime rates and the alleged "coddling" of criminal offenders by the courts. More commonly by 1968, however, the connection was very close: there were rallying cries for more severe police suppression of black rioting in the urban ghettos, and of public political dissent of the type represented by the Vietnam peace demonstrations at Chicago during the Democratic convention.

In view of these latter connotations, it is not surprising that people responsive to the "law and order" theme tended, like George Wallace, to be upset at the same time by civil rights gains and the lack of a more aggressive policy in Vietnam. Therefore it might seem redundant to treat "law and order" as a third major issue in its own right. Nevertheless, we have found it important to do so, even where the "order" being imposed is on black militants or peace demonstrators, for the simple reason that many members of the electorate reacted as though the control of dissent was quite an independent issue. This becomes very clear where support for blacks and opposition to the war are accompanied with a strong revulsion against street protest and other forms of active dissent. And this combination occurs more frequently than an academic audience may believe.

One would expect, for example, to find support for peace demonstrations among the set of people in the sample who said (a) that we made a mistake in getting involved in the Vietnam War; and (b) that the preferable course of action at the moment would be to "pull out" of that country entirely. Such expectations are clearly fulfilled among the numerous blacks matching these specifications. Among whites, however, the picture is different. First, a smaller proportion of whites—about one in six or seven—expressed this combination of feelings about Vietnam. Among those who expressed such feelings it remains true that there is relatively less disfavor vented about some of the active forms of peace dissent that had become customary by 1968. What is striking, however, is the absolute division of evaluative attitudes toward peace dissenters among those who were themselves relative "doves," and this is probably the more politically significant fact as well. Asked to rate "Vietnam war protestors" on the same kind of scale as used in Table 2, for example, a clear majority of these whites who themselves were opposed to the Administration's Vietnam policy located their reactions on the negative side of the scale, and nearly one-quarter (23%) placed them at the point of most extreme hostility.

Even more telling, perhaps, are the attitudes of these same whites toward the peace demonstrations surrounding the Democratic convention at Chicago, for in this case the protestors were given undeniably sympathetic coverage by the television networks. Keeping in mind that we are dealing here with only those whites who took clear "dove" positions on Vietnam policy, it is noteworthy indeed that almost 70% of those giving an opinion rejected the suggestion that "too much force" was used by Chicago police against the peace demonstrators, and the *modal* opinion (almost 40%) was that "not enough force" had been used to suppress the demonstration.[7]

It should be abundantly clear from this description that the white minority who by the autumn of 1968 felt our intervention in Vietnam was a mistake and was opting for a withdrawal of troops turns out to fit the campus

[7] A separate analysis, carried out by a colleague in the Survey Research Center Political Behavior Program and using the same body of data from the SRC 1968 election study, suggests, moreover, that many voters who thought the police used too little force deserted Humphrey in the course of the campaign while the minority who objected that too much force was used voted more heavily for the Democratic nominee. See John P. Robinson, "Voter Reaction to Chicago 1968," Survey Research Center (1969), mimeo.

TABLE 2. AVERAGE RATINGS OF MAJOR 1968 POLITICAL FIGURES BY A NATIONAL SAMPLE, NOVEMBER–DECEMBER, 1968

	TOTAL SAMPLE	Non-South White (N's of 785–843)	Non-South Black (N's of 54–64)	South White (N's of 315–340)	South Black (N's of 55–66)
Robert Kennedy	70.1	70.4	94.1	60.5	91.2
Richard Nixon	66.5	67.7	53.0	67.8	56.6
Hubert Humphrey	61.7	61.2	86.1	53.4	85.8
Lyndon Johnson	58.4	56.6	81.9	53.7	82.7
Eugene McCarthy	54.8	56.5	59.1	49.8	54.0
Nelson Rockefeller	53.8	54.4	61.6	50.7	53.5
Ronald Reagan	49.1	49.6	42.9	50.0	41.8
George Romney	49.0	50.4	48.3	45.6	50.2
George Wallace	31.4	27.7	9.4	48.2	13.2
Edmund Muskie	61.4	62.7	71.0	54.7	68.9
Spiro Agnew	50.4	50.9	37.7	52.9	42.4
Cirtus LeMay	35.2	33.6	21.1	43.9	22.9

image of peace sentiment rather poorly. Such a disjuncture between stereotypes developed from the mass media and cross-section survey data are not at all uncommon. However, as certain other aspects of the election may be quite unintelligible unless this fact has been absorbed by the reader, it is worth underscoring here. This is not to say that the more familiar Vietnam dissent cannot be detected in a national sample. Among whites resenting Vietnam and wishing to get out, for example, a unique and telltale bulge of 12% gave ratings of the most extreme sympathy to the stimulus "Vietnam war protestors." Now this fragment of the electorate shows all of the characteristics expected of McCarthy workers or the New Left: its members are very young, are disproportionately college-educated, Jewish, and metropolitan in background, and register extreme sympathy with civil rights and the Chicago convention demonstrations. The problem is that this group represents such a small component (one-eighth) of the 1968 dove sentiment on Vietnam being singled out here that its attitudes on other issues are very nearly obscured by rather different viewpoints held by the other 88% of the dove contingent. On the larger national scene, in turn, those who opposed Vietnam policy and were sympathetic to Vietnam war protestors make up less than 3% of the electorate—even if we add comparable blacks to the group—and law and order were not unpopular with the 97 percent.

In the broad American public, then, there was a widespread sense of breakdown in authority and discipline that fed as readily on militant political dissent as on race riots and more conventional crime. This disenchantment registered even among citizens who apparently were sympathetic to the goals of the dissent on pure policy grounds, and everywhere added to a sense of cumulative grievance with the party in possession of the White House. Thus the "law and order" phrase, ambiguous though it might be, had considerable resonance among the voters, and deserves to be catalogued along with Vietnam and the racial crisis among major issue influences on the election.

While the 1968 situation bore a number of resemblances to the basic ingredients and outcome of the 1952 election, the analogy is far from perfect. In 1952, the public turned out to vote in proportions that were quite unusual for the immediate period, a phenomenon generally taken to reflect the intensity of frustrations over the trends of government. It is easy to argue that aggravations were fully as intense in 1968 as they had been in 1952, and more intense than for any of the elections in between. Yet the proportion turning out to vote in 1968 fell off somewhat from its 1964 level.[8]

[8] The decline was only on the order of 1½ percent nationally, but the overall figures are somewhat misleading. Enormous efforts devoted to voter registration projects among Southern blacks between 1964 and 1968 appear to have paid off by increasing voter participation in that sector from 44% to 51%. Perhaps in counterpoint, Southern whites increased their turnout by 2%, thereby inching ever closer to the national norm. Thus the decline in turnout was concentrated outside the South, and there approached the more substantial drop of 4%. Even this figure

Of course any equation between indignation and turning out to vote does presuppose the offering of satisfactory alternatives, and there was somewhat greater talk than usual in 1968 that the candidate options in November were inadequate. Certainly the array of potential candidates was lengthy, whatever the actual nominees, and our account of the short-term forces affecting the electorate would be quite incomplete without consideration of the emotions with which the public regarded the dramatis personae in 1968. Just after the election, respondents in our national sample were asked to locate each of twelve political figures on a "feeling thermometer" running from zero (cold) to 100° (warm), with a response of 50° representing the indifference point. Table 2 summarizes the mean values for the total sample, as well as those within relevant regional and racial partitions.

Numerous well-chronicled features of the campaign are raised into quantitative relief by this tabulation, including Wallace's sharply regional and racial appeal, Muskie's instant popularity and near upstaging of Humphrey, and the limited interest that McCarthy seemed to hold for Negroes compared to other Democratic candidates. At the same time, other less evident comparisons can be culled from these materials, although the reader is cautioned to keep in mind that these scores refer to the period just after the election, and not necessarily to the period of the spring primaries or the summer conventions.[9] This may be of particular importance in the case of the ratings of Eugene McCarthy. When respondents were asked before the election which candidate from the spring they had hoped would win nomination, over 20 percent of Democrats and Independents recalling some preference mentioned McCarthy. However, many of these citizens gave quite negative ratings to McCarthy by November, so it appears that some disenchantment set in between the primaries and the election.

The question of timing poses itself acutely as well where Robert Kennedy is concerned. Taken at face value, the data of Table 2 imply that aside from the tragedy at Los Angeles, Kennedy should have been given the Democratic nomination and would have won the presidential election rather handily. Yet how much of this massive popularity is due to some posthumous halo of martyrdom? It seems almost certain that at least some small increment is of this sort, and that the harsh realities of a tough campaign would have eroded the bright edges of Kennedy appeal. Nevertheless, both in contested primaries and poll data of the spring period,[10] as well as in the retrospective glances of our autumn respondents, one cannot fail to be impressed by the reverberations of Kennedy charisma even in the least likely quarters, such as among Southern whites or among Republicans elsewhere. And rank-and-file Democrats outside the South reported themselves to have favored Kennedy for

is misleading, since whites outside the South showed a 3% loss in percentage points of turnout, while nonwhites declined by almost 11 percentage points! See *Current Population Reports*, "Voter Participation in November 1968," Series P-20, No. 177, December 27, 1968. Although such turnout figures, apart from the more general mobilizing of Southern blacks, are consistent with a proposition that whites were more eager to "throw the rascals out" than blacks, and that among whites, Southerners had the fiercest grievances of all, there is no hiding the fact of anemic turnout in most of the country in 1968. Interestingly enough, the decline from 1964 was uniformly distributed across the entire spectrum of party allegiances from loyal Democrats to strong Republicans.

[9] The reader should also keep in mind several other things about Table 2. The "South" here refers, as it will throughout this paper, to the Census Bureau definition of the region that includes 15 states and the District of Columbia. Hence such border states as Maryland or West Virginia are included along with the deeper southern states of the old confederacy. Presumably, for example, George Wallace's rating among whites of a more hard-core South would be correspondingly higher. Secondly, it should be remembered for some of the lesser candidates that respondents knowing so little about a candidate as to be indifferent to him would end up rating him "50°." Thus it would be questionable to conclude from Table 2 that LeMay was more popular than George Wallace, except in a very limited sense. Actually, three times as many respondents (nearly one-third) left LeMay at the indifference point as did so for Wallace. Thus lack of visibility helped to make him *less unpopular*. But among those who reacted to both men, LeMay was less popular than Wallace. Similarly, Wallace's low rating must be understood as a compound of an admiring minority and a hostile majority. The variance of Wallace ratings is much greater than those for other candidates, even in the South.

[10] Just after the decision of Robert Kennedy to run and before Lyndon Johnson's withdrawal, the Gallup poll showed Democrats favoring Kennedy as the party's nominee by a 44–41 margin.

the nomination over Humphrey by two-to-one margins, and over McCarthy by nearly three-to-one. Clearly a Kennedy candidacy could not have drawn a much greater proportion of the black vote than Humphrey received, although it might have encouraged higher turnout there. But there is evidence of enough edge elsewhere to suggest that Robert Kennedy might have won an election over Richard Nixon, and perhaps even with greater ease than he would have won his own party's nomination.

As it was, Humphrey received the mantle of party power from Lyndon Johnson and, with Robert Kennedy missing, captured the Democratic nomination without serious challenge. At that point he faced much the same dilemma as Adlai Stevenson had suffered in 1952: without gracelessly biting the hand that fed him, how could he dissociate himself from the unpopular record of the preceding administration? In 1952, Stevenson did not escape public disgust with the Truman administration, and was punished for its shortcomings. The 1968 data make clear in a similar manner that Humphrey was closely linked to Lyndon Johnson in the public eye through the period of the election. For example, the matrix of intercorrelations of the candidate ratings presented in Table 2 shows, as one would expect, rather high associations in attitudes toward presidential and vice presidential candidates on the same ticket. Thus the Humphrey-Muskie intercorrelation is .58, the Nixon-Agnew figure is .59, and the Wallace-LeMay figure is .69. But the highest intercorrelation in the whole matrix, a coefficient of .70, links public attitudes toward Lyndon Johnson and those toward Hubert Humphrey. Humphrey was highly assimilated to the Johnson image, and his support came largely from sectors of the population for which the administration had not "worn thin."

When we consider the relative strength of Kennedy enthusiasts as opposed to loyal Humphrey-Johnson supporters among identifiers with the Democratic Party within the mass public, the line of differentiation that most quickly strikes the eye is the noteworthy generation gap. As we have seen above, Kennedy supporters enjoy a marked overall plurality. However, this margin comes entirely from the young. For Democrats under thirty, only about one in five giving a pre-convention nomination preference picks Humphrey or Johnson, and Kennedy partisans outnumber them by nearly three to one. Among Democrats over fifty, however, Humphrey-Johnson supporters can claim a clear plurality.[11] The "wings" of the Democratic Party that emerged in the struggle for the nomination had an "old guard" and "young Turk" flavor, even as reflected in a cross-section sample of party sympathizers.[12]

This completes our summary of the setting in which the 1968 election took place. We have seen that despite great continuity in party loyalties and a surprising constancy in policy positions of the public, there was an unusual degree of change in partisan preference at the presidential level by comparison with 1964. This change occurred in part as a response to increased salience of some issues, such as the question of "law and order," and in part because of the way in which contending leadership cadres had come to be identified with certain policies or past performance. The Democratic party lost, as quickly as it had won, its perceived capacity to cope with international affairs and the exacerbating war in Vietnam. Hubert Humphrey, long a major figure in his own right, could not move swiftly enough to escape his links with a discredited regime.

Let us now pursue some of the more obvious analytic questions posed by the general discontent among voters in 1968, and by the Wallace movement in particular. We shall first consider influences on the actual partitioning of the vote on Election Day, and then examine some of the attitudinal and social bases underlying the outcome.

II. HYPOTHETICAL VARIATIONS ON THE VOTE OUTCOME

Impact of the Wallace Ticket. There were signs of some concern in both the Nixon and Humphrey camps that the success of George Wallace in getting his name on the ballot might divert votes and lower their respective chances of success. Nixon was more alarmed by the

[11] Interestingly enough, the same generational cleavages among Southern white Democrats occur at an earlier age than those elsewhere. In that region, Humphrey-Johnson preferences hold a plurality in all age cohorts over 30, despite the fact that Kennedy support has an edge of better than three to one among those under 30 (N of 34), perhaps because the latter group has less of a memory of the fury in the deep South at the Kennedy family prior to the assassination of President John Kennedy in Dallas in 1963.

[12] Although there is some slight tendency for pre-convention supporters of McCarthy to be relatively young, the distribution by age is more homogeneous than expected, and much more so than is the case for Kennedy. It is possible that young people supporting McCarthy as the only alternative to the Administration switched more heavily than the middle-aged to Kennedy when he announced his candidacy.

TABLE 3. DISTRIBUTION OF THE WALLACE VOTE, BY TRADITIONAL PARTIES AND CANDIDATES

		PARTY IDENTIFICATION		
		Democratic	Independent	Republican
Rating of Two Major Candidates	HUMPHREY over NIXON	4% (347)	26% (23)	21% (24)
	Tied	24% (79)	9% (11)	6% (17)
	NIXON over HUMPHREY	26% (132)	15% (53)	7% (314)

The percentage figure indicates the proportion of all voters in the cell who reported casting a ballot for Wallace. The number of voters is indicated between parentheses.

prospective loss of the electoral votes in the Deep South that Goldwater had won in 1964, while Humphrey was alarmed in turn by intelligence that Wallace was making inroads outside the South among unionized labor that had been customarily Democratic since the New Deal. At the very least, the Wallace ticket was responsible for the injection of unusual uncertainty in a game already replete with unknowns. Now that the dust has settled, we can ask more systematically how the election might have been affected if Wallace had been dissuaded from running.

Numerous polls made clear at the time of the election that Wallace voters tended to be quite disproportionately nominal Democrats, and data from our sample are congruent with this conclusion, although the differences were more notable in the South than elsewhere. For the South, 68% of Wallace voters considered themselves Democrats, and 20% Republicans.

Outside the South, proportions were 46% Democratic and 34% Republican. Yet these proportions taken alone do not address in any satisfying fashion what might have happened if Wallace had not run. In the first place, these partisan proportions among Wallace voters do not differ very markedly from those which characterize the regional electorates taken as a whole. Indeed, as we shall see, the overall association between partisanship and attitudes toward Wallace (the rating scale) shows Republicans slightly more favorable across the nation as a whole, although this fact is faintly reversed with blacks set aside, and the main lesson seems to be that the "true" correlation is of utterly trivial magnitude (.05 or less). More important still, however, is the obvious fact that Democrats voting for Wallace were repudiating the standard national ticket, as many as a third of them for the second time in a row. If Wallace had not run, we can have little confidence that they would have faithfully supported Humphrey and Muskie.

It is clear that the crucial datum involves the relative preferences of the Wallace voters for either Nixon or Humphrey, assuming that these preferences would have been the same without Wallace and that these citizens would have gone to the polls in any event. This information is available in the leader ratings used for Table 2. In Table 3 we have arrayed the total sample according to whether Humphrey or Nixon was given the higher rating, or the two were tied, as well as by the respondent's party identification. Within each cell so defined, we indicate the proportion of the vote won by Wallace, and the number of voters on which the proportion is based. The latter figures show familiar patterns. Of voters with both a party and a candidate preference, more than four-fifths prefer the nominee of their party. And while Democrats are in a majority, it is clear that the tides are running against them since they are suffering the bulk of defections.

It is interesting how the Wallace vote is drawn from across this surface. While the numbers of cases are too small to yield very reliable estimates in some of the internal cells, it is obvious that Wallace made least inroads among partisans satisfied with their party's nominee, and showed major strength where such partisans were sufficiently disgusted with their own party nominee actually to prefer that of the opposing party. Conceptually, it is significant that these protestors included Republicans unenthusiastic about Nixon as well as the more expected Democrats cool to Humphrey. Practically, however, Nixon Democrats so far outnumbered Humphrey Republicans that while Wallace drew at nearly equal rates from both

groups, the majority of his votes were from Democrats who otherwise preferred Nixon rather than from Republicans who might have given their favors to Humphrey.

This is turn provides much of the answer to one of our primary questicns. While the data underlying Table 3 can be manipulated in a variety of ways, all reasonable reconstructions of the popular vote as it might have stood without the Wallace candidacy leave Nixon either enjoying about the same proportion of the two-party vote that he actually won or a slightly greater share, depending on the region and the detailed assumptions made. In short, unless one makes some entirely extravagant assumptions about the mediating electoral college, it is very difficult to maintain any suspicion that the Wallace intrusion by itself changed the major outcome of the election.

Impact of the McCarthy Movement. If he was ever tempted at all, Eugene McCarthy decided against mounting a fourth-party campaign for the presidency. At the same time, he withheld anything resembling enthusiastic personal support for Hubert Humphrey. In view of his devoted following, some observers felt that McCarthy's refusal to close party ranks after Chicago cost the Democratic nominee precious votes, and conceivably even the presidency.

In order to understand the basis of McCarthy support at the time of the election, it is useful to trace what is known of the evolution of McCarthy strength from the time of the first primary in the spring. It will be recalled that McCarthy was the sole Democrat to challenge the Johnson administration in the New Hampshire primary. With the aid of many student volunteer campaign workers, he polled a surprising 42% of the vote among Democrats, as opposed to 48% drawn by an organized write-in campaign for President Johnson. Although he failed to upset the president in the vote, most observers saw his performance as remarkably strong, and a clear harbinger of discontent which could unseat Lyndon Johnson in the fall election. This reading was plainly shared by Robert Kennedy, who announced his own candidacy for the nomination four days later, and probably by Johnson himself, who withdrew from any contention less than three weeks later.

Sample survey data from New Hampshire at the time of the primary show some expected patterns underlying that first McCarthy vote, but also some rather unexpected ones as well. First, the vote among Democrats split toward Johnson or McCarthy in obvious ways according to expressions of satisfaction or dissatisfaction with Administration performance in general and its Vietnam policy in particular. The McCarthy vote in New Hampshire certainly reflected a groundswell of anger at the Johnson administration, and an expression of desire for a change which was simply reiterated in November. Surprisingly, however, in view of McCarthy's clear and dissenting "dove" position on Vietnam, the vote he drew in New Hampshire could scarcely be labelled a "peace vote," despite the fact that such a conclusion was frequently drawn. There was, of course, some hard-core peace sentiment among New Hampshire Democrats that was drawn quite naturally to McCarthy. Among his supporters in the primary, however, those who were unhappy with the Johnson administration for not pursuing a *harder* line against Hanoi outnumbered those advocating a withdrawal from Vietnam by nearly a three to two margin! Thus the McCarthy tide in New Hampshire was, to say the least, quite heterogeneous in its policy preferences: the only common denominator seems to have been a deep dissatisfaction with the Johnson administration.[13] McCarthy simply represented the only formal alternative available to registered Democrats. This desire for an alternative was underlined by the fact that most of the 10 percent of the Democratic vote that did not go to Johnson or McCarthy went to Nixon as a write-in candidate on the Democratic ballot.

The entry of Robert Kennedy into the race did provide another alternative and, as we have seen, a very popular one as well. He made major inroads into the potential McCarthy strength, and by the time our autumn sample was asked what candidate of the spring would have been preferred for the Democratic nomination, 46% of those Democrats with some preference cited Kennedy first while only 18% mentioned McCarthy. Nevertheless, even this 18% cannot be thought of as constituting hard-core McCarthy support at the time of the actual election, since almost two-thirds of this group had turned their attention elsewhere, giving at least one of the other presidential hopefuls a higher rating than they gave McCarthy in the responses underlying Table 2. The remainder who reported McCarthy as their preconvention favorite and awarded him their highest ratings just after the election, make up some 6% of Democrats having some clear candidate preference, or 3% of all Democrats. Along with a handful of Independents

[13] See also the account for New Hampshire by Louis Harris, "How Voters See the Issues," *Newsweek*, March 25, 1968, p. 26.

and Republicans showing the same reiterated McCarthy preference, these people can be considered the McCarthy "hard-core."

While it is this hard-core whose voting decisions interest us most, it is instructive to note where the other two-thirds of the pre-convention McCarthy support among Democrats went, over the course of the campaign. If these migrations are judged according to which presidential aspirant among the nine hopefuls of Table 2 was given the highest rating in November, one discovers that a slight plurality of these erstwhile McCarthy backers found George Wallace their preferred candidate in the fall. Slightly smaller groups favored Kennedy and Nixon, and a scatter picked other Republicans like Reagan and Rockefeller, despite their own Democratic partisanship. Very few of these McCarthy Democrats—about one in seven—migrated to a preference for Hubert Humphrey. Where the actual presidential vote was concerned, the choice was of course more constrained.

Since the McCarthy movement was commonly thought of as somewhat to the left of Humphrey and the administration, while Wallace was located rather markedly to the right, a major McCarthy-to-Wallace transfer of preferences may seem ideologically perplexing. Were McCarthy supporters so furious with the Humphrey nomination that pure spite overcame issue feelings and led to a protest vote for Wallace? Although there were rumors of such a reaction at the time, our data suggest a somewhat simpler interpretation. We have already noted the attitudinal heterogeneity of McCarthy voters in New Hampshire. Those in our autumn sample who recall a preconvention preference for McCarthy are similarly heterogeneous. Indeed, on some issues of social welfare and civil rights, preconvention McCarthy supporters are actually more conservative than backers of either Humphrey or Kennedy.

This heterogeneity declined markedly, however, as the size of the McCarthy group eroded over the summer to what we have defined as the hard-core. If we compare the attitudes of that hard-core on major issues with those of the professed early backers of McCarthy who subsequently supported Wallace, the differences are usually extreme. The McCarthy-Wallace group was against desegregation, in favor of an increased military effort in Vietnam, and was highly indignant with the situation in the nation where "law and order" was concerned (see Table 4). People supporting McCarthy to the bitter end took opposite positions on all of these major issues. Similarly, the winnowing down of the McCarthy support operated very sharply along demographic lines. Among non-Southern white Democrats who reported a preconvention McCarthy preference, for example, the hard-core that remained enthusiastic about McCarthy through to the actual election were 60% of college background, whereas, of those whose ardor cooled, only 18% had had any connection with college.

In short, then, it is evident again that among Democrats particularly, McCarthy was an initial rallying point for voters of all policy persuasions who were thoroughly displeased with the Johnson administration. When the Wallace candidacy crystallized and his issue advocacies became more broadly known, that portion of the discontented to whom he spoke most directly flocked to him. Hence it seems very doubtful that Humphrey would have won many votes from this group even if McCarthy had lent the Vice President his personal support in a whole-hearted fashion. The main motivation of this group was to register its disgust with incumbent leaders concerning civil rights advances, timidity in Vietnam and outbreaks of social disorder. It may well be that by September, with the far more congenial candidacy of Wallace available, Senator McCarthy would already have become a relatively negative reference point for this two-thirds of his early support, especially if he had joined forces with Humphrey. Therefore if we are to search for votes withheld from Humphrey because of the kinds of discontent McCarthy helped to crystallize, they are much more likely to be found among the McCarthy hard-core.

We persist in looking for such withheld votes, not simply because of rumors they existed, but also because there are rather tangible signs in the data that they were present in 1968. Such votes could take any one of four major alternative forms: they could be located among citizens who went to the polls but did not vote for president; they could be reflected in votes for minor party candidates; they could involve staying at home on election day; or they could take the form of votes spitefully transferred to Humphrey's chief rival, Mr. Nixon. Easiest to establish as "withheld votes" are the first two categories. Although their incidence is naturally very limited, both types can be discerned in the sample and do occur in conjunction with strong enthusiasm for McCarthy. Projected back to the nation's electorate, perhaps as much as a half-million votes are represented here, lying primarily outside the South. This is only a faint trace when sprinkled across the political map of the nation, however, and taken alone would probably have made little or no

TABLE 4. ISSUE DIFFERENCES AMONG WHITES PREFERRING MCCARTHY AS THE DEMOCRATIC NOMINEE, ACCORDING TO NOVEMBER PREFERENCES FOR MCCARTHY OR WALLACE

		McCARTHY "HARD CORE"[a]	VOTED WALLACE[b]
"Are you in favor of desegregation, strict segregation, or something in between?"	DESEGREGATION	79%	7%
	IN BETWEEN	21	50
	SEGREGATION	0	43
		100%	100%
		(24)	(14)
"Do you think the (Chicago) police (at the Democratic Convention) used too much force, the right amount of force, or not enough force with the demonstrators?"	TOO MUCH FORCE	91%	0%
	RIGHT AMOUNT	9	50
	NOT ENOUGH	0	50
		100%	100%
		(23)	(12)
"Which of the following do you think we should do now in Vietnam: pull out of Vietnam entirely, keep our soldiers in Vietnam but try to end the fighting, or take a stronger stand even if it means invading North Vietnam?"	PULL OUT	50%	7%
	STATUS QUO	50	7
	STRONGER STAND	9	86
		100%	100%
		(24)	(13)

[a] This column is limited to whites whose pre-convention favorite was Eugene McCarthy and who continued to give him their top rating after the November election.

[b] It is to be emphasized that this column includes *only* those Wallace voters who said that in the spring of 1968 they had hoped Eugene McCarthy would win the Democratic nomination. This fact explains the small case numbers. However, in view of the relative homogeneity of respondents in the table—all are whites who reported a pre-convention McCarthy preference and most happen in addition to be nominal identifiers of the Democratic Party—the disparities in issue position are the more impressive.

difference in the distribution of votes from the electoral college.

It is more difficult to say that specific instances of abstinence from any voting in 1968, or "defection" to Richard Nixon, reflect an abiding loyalty to McCarthy that Humphrey could not replace, and would not have occurred but for the McCarthy intrusion. There is a faint edge of non-voting that looks suspiciously of this sort, but it is again very limited: most ardent McCarthy fans were too politically involved to have thrown away a chance to vote at other levels of office. Far more numerous are the defections to Nixon on the part of voters of liberal and Democratic predispositions, who reported sympathy toward McCarthy. Here, however, it is difficult to be confident that McCarthy made any necessary contribution to the decision equation: the situation itself might have soured these people sufficiently, McCarthy or no. Nevertheless, when one begins to add together putative "withheld votes" from the preceding three categories one does not need to factor in any very large proportion of these defectors to arrive at a total large enough to have provided Humphrey with a tiny majority in the electoral college, without requiring any gross maldistribution of these new-found popular votes outside the South.

We should reiterate, of course, that any such hypothetical reconstructions must be taken with a grain of salt. If McCarthy had embraced Humphrey on the final night in Chicago, not all of his most fervent supporters would necessarily have followed suit, and Humphrey would have needed most of them for a victory. Or if Humphrey had catered more dramatically to the McCarthy wing in

terms of Vietnam policy after the election, he might have suffered losses of much greater proportion to Wallace on his right, for there is simply no question but that Democrats sharing the circle of ideas espoused by Wallace outnumbered the Democrats attuned to McCarthy by a very wide margin—perhaps as great as ten to one. Moreover, it is appropriate to keep in mind our earlier suggestion that the Wallace intrusion hurt Nixon's vote more than Humphrey's: if we now remove Wallace as well as McCarthy from the scene, the net result might remain a Nixon victory.

However all this may be, it seems probable that the entire roster of prominent Democratic candidates—McCarthy, Wallace, Kennedy, McGovern—who were in their various ways opposing the administration, must have contributed cumulatively to Humphrey's problem of retaining the loyalty of fellow Democrats in the electorate. Certainly the failure of liberal Republican leaders to rally around the Goldwater candidacy in 1964, itself an unusual departure from tradition, had contributed to the Republican disaster of that year. 1968 provided something of a mirror image, and the result was an inordinate movement of the electorate between the two consultations.

III. THE "RESPONSIBLE ELECTORATE" OF 1968

In describing the current of discontent that swirled around the Democratic party and the White House in 1968, we indicated that disgruntled Democrats rather indiscriminately supported McCarthy in the earliest primaries, but soon began to sort themselves into those staying with McCarthy versus those shifting to Nixon or Wallace, according to their more precise policy grievances on the major issues of Vietnam, civil rights, and the problem of "law and order." By the time of the election, the sorting had become remarkably clean: in particular, differences in issue position between Wallace supporters and what we have called the McCarthy hard core are impressive in magnitude.

Even more generally, 1968 seems to be a prototypical case of the election that does not produce many changes of policy preferences but does permit electors to sort themselves and the candidates into groups of substantial homogeneity on matters of public policy. This trend over the course of the campaign calls to mind the posthumous contention of V.O. Key, in *The Responsible Electorate,* that the mass electorate is a good deal less irrational, ill-informed or sheep-like than it had become fashionable to suppose. He presented empirical materials to develop a counter-image of "an electorate moved by concern about central and relevant questions of public policy, of governmental performance, and of executive personality."[14] He argued that in a general way voters behaved rationally and responsibly, or at least as rationally and responsibly as could be expected in view of the pap they were frequently fed by contending politicians, while recognizing in the same breath that contentions of this unequivocal nature were necessarily overstatements.

To our point of view, Key's general thesis represented a welcome corrective on some earlier emphases, but his findings were hardly as discontinuous with earlier work as was often presumed, and the "corrective" nature of his argument has itself become badly exaggerated at numerous points. We cannot begin to examine here the many facets of his thesis that deserve comment. However, several features of the 1968 campaign seem to us to demonstrate admirably the importance of the Key corrective, while at the very same time illustrating vividly the perspective in which that corrective must be kept.

It is obvious, as Key himself recognized, that flat assertions about the electorate being rational or not are of scant value. In New Hampshire, as we have observed earlier, Democrats exasperated at Johnson's lack of success with the Vietnam war voted for Eugene McCarthy as an alternative. The relationship between this disapproval and the vote decision is exactly the type of empirical finding that Key musters in profusion from a sequence of seven presidential elections as his main proof of voter rationality and responsibility. In the New Hampshire case, however, we might probe the data a little farther to discover that more often than not, McCarthy voters were upset that Johnson had failed to scourge Vietnam a good deal more vigorously with American military might, which is to say they took a position diametrically opposed to that of their chosen candidate. This realization might shake our confidence somewhat in the preceding "proof" of voter rationality. But then we push our analysis still another step and find that many of the New Hampshire people fuming about Vietnam in a hawkish mood voted for McCarthy without having any idea of where he stood on the matter. Hence while they may have voted directly counter to their own policy preferences, they at least did not know this was

[14] V. O. Key, Jr., *The Responsible Electorate: Rationality in Presidential Voting, 1936–1960* (Cambridge, Mass.: Belknap Press, 1966), pp. 7–8.

what they were doing, so the charge of irrationality may be a bit ungenerous. In the most anemic sense of "rationality," one that merely implies people have perceived reasons for their behavior, these votes perhaps remain "rational."

However, when we reflect on the rather intensive coverage given by the national mass media to Eugene McCarthy's dissenting position on Vietnam for many months before the New Hampshire primary, and consider how difficult it must have been to avoid knowledge of the fact, particularly if one had more than the most casual interest in the Vietnam question, we might continue to wonder how lavishly we should praise the electorate as "responsible." Here, as at so many other points, pushing beyond the expression of narrow and superficial attitudes in the mass public to the cognitive texture which underlies the attitudes is a rather disillusioning experience. It is regrettable that none of the data presented in *The Responsible Electorate* can be probed in this fashion.

Key was interested in showing that the public reacted in a vital way to central policy concerns, at least as selected by the contending political factions, and was not driven mainly by dark Freudian urges, flock instinct, or worse still, the toss of a coin. With much of this we agree wholeheartedly. In addition, to put the discussion in a slightly different light, let us imagine, in a vein not unfamiliar from the literature of the 1950's, that voting decisions in the American electorate might be seen as a function of reactions to party, issue and candidate personality factors. Let us imagine furthermore that research suggests that these determinants typically have relative weights in our presidential elections of 60 for the party factor, and 40 divided between the issue and candidate determinants. The exact figures are, of course, quite fanciful but the rough magnitudes continue to be familiar. Since classical assumptions about voting behavior have attributed overweening weight to the issue factor, it is scarcely surprising that investigative attention shifts heavily away from that factor to the less expected party and candidate influences. If the issue factor draws comment at all, the finding of greatest interest is its surprisingly diluted role.

It is at this point that the Key volume exerts its most useful influence. Key points out that there *is*, after all, an issue factor, and he develops an analytic format which dramatizes the role that issue reactions do play. This dramatic heightening is achieved by focussing attention on voters who are shifting their vote from one party to the other over a pair of elections. If we set for ourselves the explanatory chore of understanding why the change which occurs moves in the direction it did, it is patently evident that the party factor—which merely explains the abiding finding that "standpatters" persistently outnumber "changers" by factors usually greater than four—is to be set aside as irrelevant. If this in turn leaves candidate and issue factors sharing the explanatory burden, our sense of the relative importance of the issue factor is, of course, radically increased, even though it is our question that has changed, rather than anything about the empirical lay of the land. Key was quite explicit in his desire to explain movement and change in the electorate, rather than voting behavior in a more general sense, and there is no gainsaying the fact that from many points of view it is indeed the change—marginal gains and losses—which forms the critical part of the story of elections.

In our analyses of such changes in the national vote over the course of presidential elections in the 1950's and 1960's we have been impressed with the magnitude of the effects introduced as new candidates focus on different issues of public policy, and as external events give particular candidate-issue intersections greater salience for the nation.[15] However, 1968 provides an opportunity to examine relative weights of party, candidate and issue factors under more varied circumstances than United States presidential elections usually proffer. We have talked above for illustrative purposes as though there were "standard" relative weights that would pertain for these three factors in some situation-free way. This is of course not the case: we can imagine many kinds of elections which would vastly shift the weights of such factors, if indeed they can be defined at all.

The Wallace movement is a good case in point. By Key's definition nobody who voted for Wallace could have been a "standpatter": all must be classed as "changers." Therefore party identification as a motivating factor accounting for attraction to Wallace is forced back to zero, and any variance to be understood must have its roots distributed between Wallace's attraction as a personality and the appeal of the issue positions that he advocated.[16]

[15] Donald E. Stokes, "Some Dynamic Element of Contests for the Presidency," this REVIEW LX (March, 1966), 19-28.

[16] This is not to say that it would be inconceivable for identification with one of the two traditional parties to correlate with preferenc

In point of fact, the Wallace candidacy was reacted to by the public as an *issue* candidacy, a matter which our data make clear in several ways. For example, about half of the reasons volunteered by our respondents for favorable feelings toward Wallace had to do with positions he was taking on current issues; only a little more than a quarter of the reactions supporting either of the two conventional candidates were cast in this mode. Still more noteworthy is the relative purity of the issue feelings among the Wallace clientele where the major controversies of 1968 were concerned. Among the *whites* who voted for one of the two major candidates, only 10% favored continued segregation rather than desegregation or "something in between;" among Wallace voters, all of whom were white, almost 40% wanted segregation. Where the issue of "law and order" was concerned, a substantial portion of the voters felt that Mayor Daley's police had used about the right amount of force in quelling the Chicago demonstrations. However, among white voters for Nixon or Humphrey, the remainder of the opinion was fairly evenly split between criticizing the police for using too much force or too little, with a small majority (55%) favoring the latter "tough line." Among Wallace voters, the comparable ratio was 87–13 favoring a tougher policy. Or again, 36% of white voters for the conventional parties felt we should "take a stronger stand (in Vietnam) even if it means invading North Vietnam." Among Wallace voters, the figure was 67%. Much more generally speaking, it may be observed that all Wallace voters were exercised by strong discontents in at least one of these three primary domains, and most were angry about more than one. Wallace was a "backlash" candidate, and there is no question but that the positions communicated to the public and accounted for his electoral support in a very primary sense. The pattern of correlations between issue positions and the vote for these "changers" would support Key's thesis of a "rational" and "responsible" electorate even more impressively than most of the data he found for earlier elections.

Another way of organizing these preference materials helps to illuminate even more sharply the contrast between the bases of Wallace support and those of the conventional candidates. It will be recalled that all respondents were asked to give an affective evaluation of each of the three candidates taken separately, along with other aspirants. If we examine the pattern of correlations between issue positions and the ratings of Humphrey, Nixon and Wallace, we capture gradations of enthusiasm, indifference and hostility felt toward each man instead of the mere vote threshold, and we can explore the antecedents or correlates of the variations in sentiment toward the individual candidates.

Where the ratings of Wallace given by whites are concerned, patterns vary somewhat South and non-South, but substantial correlations with issue positions appear everywhere. In the South, the most generic question of civil rights policy shows a relation of .49 (gamma) with Wallace reactions; the most generic question on "law and order" shows a .39; and the central Vietnam policy question shows a relationship of .30. Party identification, however, shows a relation of only .04. Other ancillary questions probing more specific aspects of policy feelings in these areas vary around the most generic items somewhat, but tend to show fairly similar magnitudes of relationship. Outside the South, patterns are a little less sharp but remain unequivocal. Instead of the above correlations of .49, .39 and .30 in the main issue domains, the figures are .25 (civil rights), .27 (law and order), and .25 (Vietnam). The relationship of party identification to Wallace ratings among whites, however, is .01. Thus it is true in both regions that party identification is entirely dwarfed by any of several issue positions in predicting reactions to Wallace among whites, and in terms of "variance accounted for" the differences between issues and party would best be expressed in terms of *orders of magnitude.*

Differences that are almost as sharp turn up in the relationships surrounding the ratings of Nixon and Humphrey. Here, however, everything is exactly reversed: it is *party* that towers over all other predictors, and the central 1968 issues tend to give rather diminutive relationships. Thus comparable correlations (gammas) between partisanship and candidate ratings all

for some third-party candidate. For example, it is possible that most of the voters for Henry Wallace's Progressive Party in 1948 were identified with the Democratic Party. However, it is clear that in such an instance "party loyalty" would have been a rather spurious name for the motivating factor. In the case of George Wallace, even this kind of spurious correlation is absent, except insofar as his Democratic origins and the invisibility of his American Independent Party label made it easy for Democrats to support him. Indeed, in the context of this argument it will be fascinating to discover whether Republicans and Democrats invoked different images of Wallace's party location in order to satisfy their need for consonance while voting for a man who reflects their own issue commitments.

TABLE 5. CORRELATIONS BETWEEN ISSUE POSITIONS, PARTISANSHIP AND AFFECTIVE RATINGS OF THE MAJOR CANDIDATES

(Whites Only)[a]

ISSUE DOMAIN:	NON-SOUTH			SOUTH		
	Humphrey	Nixon	Wallace	Humphrey	Nixon	Wallace
A. Civil Rights (6 or 7 items)[b]	.17	.09	.27	.24	.08	.41
B. Law and Order (2 items)	.25	.05	.27	.19	.01	.35
C. Vietnam (2 items)	.05	.03	.23	.14	.02	.26
D. Cold War (4 items)	.12	.11	.15	.16	.05	.28
E. Social Welfare (2 or 3 items)[b]	.22	.20	.09	.26	.13	.10
F. Federal Gov't Too Powerful? (1 item)	.37	.18	.17	.49	.13	.15
SUM: 18 issue items	.19	.10	.20	.22	.07	.31
SUM: Three Major 1968 Issue Domains (A,B,C)	.16	.07	.26	.22	.07	.37
PARTISANSHIP: (3 items)	.47	.47	.04	.39	.36	.03

[a] Cell entries are average absolute values of gamma ordinal correlations between items of the types listed in the rows and affective ratings of the candidates noted in the columns.

[b] An item having to do with the role of the federal government in aid to local education was considered a social welfare item outside the South, but a civil rights issue within that region.

run between .36 and .44, varying only slightly by region and man. Where Nixon is concerned, the average correlation values for issue items in the three main domains emphasized in the 1968 election never get as high as .10, and fall as low as .01, with the central tendency about .05. Where Humphrey is concerned, somewhat higher issue values are observed, varying between .05 and .25 according to the region and the domain. Moreover, there is another issue domain not hitherto cited in which average values over three items for Humphrey considerably outstrip the Wallace correlation in both North and South. Significantly, this is the domain of items concerning governmental social welfare activities that one might associate with the period running from the New Deal through the 1950's.[17] Nevertheless, averaging correlations across all of these issue domains (the obsolescing as well as the three most salient in 1968) suggests that party identification still accounts for three to five times as much variance in Humphrey ratings as does the average issue among the 18 issues posed in the study. These correlation patterns are summarized by region in Table 5.

Such dramatic comparisons between types of support for Wallace on one hand and the conventional candidates on the other may be perplexing to the casual reader who is keeping the thesis of V.O. Key in mind. After all, it is the pattern of Wallace support that shows the kind of strong issue orientation Key sought to demonstrate, whereas evaluations of both Humphrey and Nixon seem to show a strong factor of traditional party allegiance suffocating most issue concerns into relative obscurity. Yet the span of time Key's data covered limited him almost completely to observation of races of the routine Humphrey-Nixon type. Did

[17] Another domain of issues surrounding the "cold war" as it confronted the nation in the 1950's with controversies over foreign aid and trade with communist countries shows only modest correlations with the candidate rankings, and Nixon and Humphrey ratings show more of a parity with the Wallace correlations, although in an absolute sense the latter continue to outrun the former sharply in the South and mildly elsewhere. See Table 5.

these earlier two-party races look more like the Wallace patterns for some unknown reason?

The answer, of course, is very probably not. However, if we set the Wallace phenomenon in 1968 aside and limit our attention in the Key fashion to two contrasting groups of "changers" between the 1964 and 1968 election (Johnson to Nixon; Goldwater to Humphrey) we can show correlations with issue differences which look very much like those presented in cross-tabulations by Key for earlier elections: some strong, some weak, but nearly always "in the right direction." There are, to be sure, other problems of interpretation surrounding such correlations that one would need to thrash out before accepting the Key evidence fully.[18] But our principal point here is the simple one that even with Wallace analytically discarded from the 1968 scene, the rest of the 1968 data seem perfectly compatible with the data Key used. The only reason there may seem to be a discontinuity, then, is due to the different nature of the question being asked by Key which, by focussing on marginal change from election to election, effectively defines party loyalty out of the explanation and correspondingly opens the way for greater orienting weight for issues.

It is because the change in vote division from election to election is so critical that V.O. Key's contribution is a welcome corrective. On the other hand, the configurations of 1968 data we have summarized here help to put that contribution into perspective. The patterns of Wallace support show how empirical data *can* look when issues play a strongly orienting role. The contrasts between these patterns and those generated by routine two-party politics may help to suggest why investigators have tended to be more impressed by the feeble role of issues than by their strength.

The lessons to be drawn are several. One is a simple point of methodology. It has been suggested upon occasion in the past that relationships between issue positions and voting choice turn out to be as pallid as they usually are because investigators fail to ask the right questions or word them in confusing ways. We feel that improvement in these matters is always possible. However, we have seen that exactly the same issue items which continue to look pallid in accounting for assessments of Humphrey and Nixon blaze forth into rather robust correlations where Wallace is concerned. Hence we conclude that poor item choice scarcely accounts for past findings.

Another lesson is more substantive. Some past findings have been to our mind "overinterpreted" as implying that issues are poorly linked to voting preferences because of innate and hence incorrigible cognitive deficiencies suffered by the mass electorate in the United States.[19] Merely the Wallace data taken alone would suffice to show, exactly as Key argued, that the public can relate policy controversies to its own estimates of the world and vote accordingly. The fact that it does not display this propensity on any large scale very often invites more careful spelling out of the conditions under which it will or will not.

It seems clear from the 1968 data that one of the cardinal limiting conditions is the "drag" or inertia represented by habitual party loyalties: as soon as features of the situation limit or neutralize the relevance of such a factor, issue evaluations play a more vital role. Much research has shown that partisanship is fixed early in life and tends to endure. As the individual moves through the life cycle, old political controversies die away and new ones arise toward which at least some individuals crystallize opinions. While the parties try to lead this new opinion formation among their faithful, and probably succeed on a modest scale, there are many independent sources of such opinion for the citizen. The average citizen either does not know his party's position well enough to be influenced on many matters, or if he knows, frequently resists the influence. As a result, policy opinions are very loosely or anachronistically linked to party preference at any point in time. But in the moment of truth in the polling booth, party allegiance seems the most relevant cue for many voters *if conditions permit it to be used.*

Another type of condition which mediates the links between citizen position on issues and

[18] These include such considerations as that of the causal direction underlying the observed relationships; or known and systematic biases in recollection of a presidential vote four years later; or the superficiality of the issues that show such patterns, as opposed to issues thought basic by sophisticated observers; or blatant misinformation supporting the issue positions registered; or a tendency for the less informed to "shift" more quickly than the better informed, with position on any given issue held constant, etc.

[19] We much prefer an interpretation which hinges on a general inattention which is endemic because information costs are relatively high where little information is already in hand, and the stakes are rarely seen as being very large. While such a "condition" is likely to persist in mass electorates, there is nothing about it which is immutable given the proper convergence of circumstances.

voting choice is the "objective" degree of difference between parties or candidates with respect to policy controversy, or the clarity with which any objective difference gets communicated to the populace. In every United States election there are accusations from one quarter or another that the two conventional parties provide no more than "tweedledee" and "tweedledum" candidates. However, these accusations as aired in the public media rose to something of a crescendo in 1968 from both the Wallace and the McCarthy perspectives. And even as measured a source as the *New York Times* noted wrily that it would take no more than the deletion of two or three codicils to make the official 1968 campaign platforms of the Democratic and Republican parties into utterly undistinguishable documents. If the main discriminable difference between Humphrey and Nixon began and ended with the party label then it would certainly not be surprising that the public sorted itself into voting camps by party allegiance and little more, save where Wallace was concerned. In this case, the public would be limited to exactly that "echo chamber" role which Key ascribed to it.

As a matter of pure logic, nobody can deny that policy differentiation between parties is likely to be a precondition for meaningful relationships between policy feelings and partisan voting decisions. Our only problem here is to evaluate whether the party/issue data configurations surrounding Humphrey and Nixon are the obvious result of some lack of policy difference peculiar to 1968, or represent instead some more abiding feature of presidential voting in the United States. Unfortunately, there is no obvious way to arrive at an objective measurement of "degree of party difference." Perhaps the closest approximation is to ask the public how clear the differences appear to be. Nevertheless, since some people invariably feel party differences are big and others feel they are non-existent, even this approach leaves one without reference points as to "how big is big" where reports of this kind are concerned, except inasmuch as trends in such reports can be observed over periods of time. In this light, it can be said while reports of "important differences" between the Democrats and the Republicans were slightly fewer in 1968 than in 1964 (the year of Goldwater's "choice, not an echo"), they show a reasonable parity with such reports for 1952 and 1960. Hence in the public eye, at least, differences between what the major parties stand for were not lacking in unusual degree in 1968.

It may be useful to note that whereas we have labelled the Wallace effort in 1968 an "issue candidacy" from the point of view of the electorate, we have not said that it was an ideological candidacy from that same point of view. From other viewpoints of political analysis, it was of course just that: a movement of the "radical right." Moreover, with occasional exceptions, data on issue positions show Wallace voters to differ from Humphrey voters in the same "conservative" direction that Nixon voters do, only much more so. Therefore by customary definitions, not only the leadership of the radical right, but the rank-and-file espoused clearly "rightist" positions of a sort which were frequently extreme, on highly specific questions of public policy.[20]

Yet there was an element of ideological self-recognition present among Goldwater voters in 1964 that was simply lacking among Wallace voters in 1968. One measure of ideological location which we use involves the respondent in rating the terms "liberal" and "conservative." If the respondent gives the highest possible score to the stimulus "liberal" and the lowest possible score to "conservative," he is rated as the most extreme liberal, with a score of 100. In the reverse case, the extreme conservative receives a score of zero. At 50 are clustered individuals who either do not recognize these terms, or give the same affective rating to both.[21] In 1964 there was a rather considerable relationship between such a measure and response to Goldwater, in the expected direction. In 1968, the same scale showed only a very limited correlation with reactions toward Wallace (gammas of .13 and .09 among whites within the South and outside, respectively). Indeed, as Table 6 shows, in both political regions of the country Wallace voters were more favorable to the "liberal" label than Nixon voters! Thus while Wallace supporters were entirely distinctive in their "backlash" feelings on public policy, they were much less ideologically attuned to a left-right spectrum than their Goldwater predecessors.

[20] This was not true across every issue domain. The most notable exception was in the area of social welfare issues such as medicare and full employment guarantees, on which issues Wallace voters were significantly more "liberal" than Nixon voters, and almost matched the liberalism of Humphrey voters. This admixture was of course familiar in Wallace's frequent appeals to the underdog and the working man, in the tradition of Southern populism.

[21] For reasons discussed elsewhere, a rather large proportion of the American electorate—nearly half—is found at this point of ideological neutrality.

Although Wallace supporters did not seem anywhere nearly as distinctive in terms of ideological measures as they did on specific issues, they did show some moderate trends in terms of other more generic political attitudes. In particular, various measures bearing on discontent with the responsiveness and probity of government show correlations with ratings given by whites to Wallace, and are related but with opposite signs to ratings of the "establishment" candidates, Humphrey and Nixon. Since Wallace was more of a mainstream candidate in the South than in the rest of the country, it might be thought that his appeal in that region might depend less strictly on this syndrome of political alienation than it would elsewhere. However, these relationships are stronger and more pervasive in the South, and seem only weakly mirrored in other parts of the nation. Within the South, white attitudes toward Wallace are quite sharply associated with our scales of political efficacy and cynicism about government. People drawn to Wallace tended to feel they had little capacity to influence government, and expressed distrust of the morality and efficiency of political leadership. These correlations reach a peak on items where the referent is most explicitly "the federal government in Washington," and it is plain that Southern voters felt more or less attracted to Wallace in the degree that they responded to his complaints that Washington bureaucrats had been persistently and unjustly bullying the South with particular respect to civil rights. Since there is no methodological need for it to be true, it is of particular interest that ratings of Humphrey show as substantial correlations in the opposing direction, in the South and other regions as well: people responding warmly to Humphrey had quite sanguine views of government.

All told, then, a sense of political alienation was a rather visible correlate of a sorting of the citizenry away from the conventional candidates toward Wallace, as was certainly to be expected and necessary if terms such as "backlash" are relevant. At the same time, it is worth keeping the apparent temporal sequences clear. The data suggest that Southern whites have become alienated with government because prior attitudes, particularly racial ones, have been contradictory to national policy for nearly twenty years. Thus there is a readiness to condemn government on a much broader front, and Wallace appealed in obvious ways to this readiness in the South. Outside the South Wallace also articulated the same array of specific grievances and received a clear response. However, the evidence suggests that any resonance he might have achieved in terms of a more generic condemnation of government, while present, was relatively limited.

TABLE 6. IDEOLOGICAL RESPONSES OF WHITE VOTERS FOR DIFFERENT PRESIDENTIAL CANDIDATES IN 1964 AND 1968[a]

	1964		1968		
	Johnson	Goldwater	Humphrey	Nixon	Wallace
NON-SOUTH	51.8	39.9	51.8	43.4	44.9
SOUTH	49.6	35.9	49.5	40.7	41.9

[a] The cell entry registers the mean value shown on the ideological scale described in the text for white voters for each of the candidates listed. A high value indicates that liberalism is held in relative favor; a low value means that conservatism is preferred.

IV. THE SOCIAL BASES OF WALLACE SUPPORT

A variety of facts already cited about the Wallace movement of 1968 makes clear that while there was some modest overlap in support for Goldwater in 1964 and Wallace in 1968, it was at best a weak correlation and the Wallace clientele differed quite notably from Goldwater's. Thus, for example, almost exactly half of our 1968 Wallace voters who had participated in the 1964 election reported that they had voted for Johnson. Or again, we have seen that the majority of Wallace voters, like the electorate as a whole, was identified with the Democratic party, while it is obvious that most Goldwater voters were Republican identifiers. Similarly, we have just noted that the Wallace movement had a much less clear ideological focus among its sympathizers than marked Goldwater supporters in 1964.

This discrepancy in clientele may seem perplexing. After all, in the terms of conventional analysis in political sociology both candidates were "darlings of the radical right." Yet the limited degree of overlap between Goldwater and Wallace voters is confirmed in equally impressive fashion when one compares their social backgrounds or even their simplest demographic characteristics. Among Goldwater voters, for example, women both South and non-South showed the same slight majority they enjoy in the electorate; Wallace voters in the South showed a similar balance, but elsewhere were rather markedly (almost 60–40) male. The Goldwater vote had been much more urban, while the Wallace vote was relatively rural and small-town, particularly in the South. Outside the South, the age distribution of Wallace voters departed markedly from that

shown by Goldwater in 1964, with the proportion under 35 being about twice as great and that over 65 only half as large.

The well-publicized appeal of Wallace to the unionized laboring man is clearly reflected in our data: outside the South, the proportion of white union members preferring Wallace over the other major candidates was more than three times as great as it was within households having no unionized members (19% to 6%); even in the South, where other appeals were present and the unionization of labor is more limited, the contrast between the preferences of union members and non-union households remains dramatic (52% to 28% giving top preference to Wallace over the conventional candidates). Indeed, in both regions the occupational center of gravity of Wallace popularity was clearly among white skilled workers. Nationwide, only about 10% of the Wallace vote was contributed by the professional and managerial strata, whereas persons of these occupations had given Goldwater almost half of his vote (46%). Needless to say, the proportion of unionized labor supporting Goldwater was very low. Along with these class differences, marked discrepancies in educational background can be taken for granted. In the South, one-third of Wallace's support came from whites with no more than grade school education, while the national figure for Goldwater was 13%. The proportion of voters of college experience backing Goldwater was about double that found voting for Wallace either in the South or elsewhere.

All of these comparisons help to underscore the major disparities in the social bases of support for Goldwater and Wallace, despite the apparent common policy ground of the relatively extreme right. While one should not lose track of the fact that there was a small and systematic overlap in clientele, it is abundantly clear that neither candidate exhausted the potential support for a severely conservative program in matters of civil rights, law and order or Vietnam. In a very real sense, it can be seen that Wallace was a poor man's Goldwater. As we suggested at the time, Goldwater pitched his campaign on an ideological plane which rather escaped some members of the electorate who might otherwise have found his positions congenial.[22] Wallace's perfectly direct appeal to citizens of this latter description, along with the undercurrent of populism alien to the Goldwater conservatism, apparently sufficed to put off some of the Arizona senator's more well-to-do supporters.[23] The Goldwater support was drawn from a relatively urbane and sophisticated conservatism; Wallace appealed to many similar instincts, but the style was folksy and tailored to the common man.

In a significant way, too, Wallace remained a regional candidate despite his discovery that he could win more than scattered votes in the North and his consequent presence on every state's ballot. Over half of his popular votes came from the states of the Confederacy. Everything, from his lack of political experience at a federal level to his marked Southern accent, suggested a parochial relevance that had rarely been salient where Goldwater was concerned. While electoral maps leave no doubt as to the regional nature of the response, sample survey data show that even these visible effects have been diluted by inter-regional migration. Thus, for example, while much has been written about the Wallace appeal in various European ethnic communities of northern cities, little has been said about the "American ethnic group" of southern white migrants, most of whom are blue-collar and frequently in a position to take special pleasure in the spectacle of a Southern compatriot coming north to give the Yankees what for. Our data indicate that Wallace drew over 14% of the vote from these migrants, and less than 7% otherwise outside the South. On the other hand, the significant stream of migration of Yankees into the South, the political implications of which we have described elsewhere,[24] provided something of a barrier to further Wallace successes. Heavily Republican in a non-Southern sense and now constituting better than one-seventh of white voters in the region, these migrants were even less interested in voting for Wallace than were Southern whites in the North, and gave the former Alabama governor only 10% of their vote while their native Southern white colleagues were casting almost one vote in every three for him.

Table 7 summarizes the affective ratings given Wallace by our respondents according to the region in which they grew up as well as their current region of residence. It is rather clear that the region of socialization is a more critical determinant of these assessments of

[22] P. Converse, A. Clausen, and W. Miller, "Electoral Myth and Reality: The 1964 Election" this REVIEW, 59 (June, 1965), 321–336.

[23] It is quite possible, however, that some of this support might have moved to Wallace had the Republican Party nominated anybody but Nixon or Reagan, among the main contenders.

[24] A. Campbell, P. Converse, W. Miller, and D. Stokes, *Elections and the Political Order* (New York: John Wiley, 1965), Chapter 12.

TABLE 7. REACTIONS OF WHITES TO WALLACE BY REGION OF SOCIALIZATION AND RESIDENCE

		RESPONDENT NOW RESIDES . . .		
		Outside the South	Within the South	TOTAL
RESPONDENT	Outside the South:	26.2[a] (757)	26.5 (51)	26.2 (808)
GREW UP . . .	Within the South:	34.7 (53)	50.0 (281)	48.5 (334)
	TOTAL:	26.7 (810)	46.3 (332)	

[a] Cell entries are mean values of ratings on a scale from 0 (hostility) to 100 (sympathy) accorded to George Wallace by white respondents of the types indicated.

Wallace than is the region of current residence. Moreover, it is easy to show that regional differences in correlates of Wallace preference also follow lines of socialization rather than those of current residence. For example, we have noted that Wallace's appeal to women outside the South was rather limited. For white women of Southern background living outside the South, the response was much as it was in the South. Setting the migrants aside, the sex ratio among white Wallace enthusiasts outside the South is even more sharply masculine.

It is not our purpose here to do more than briefly summarize the social and demographic correlates of Wallace preferences, for numerous other essays are being prepared to treat the subject in detail. However, one correlate which has frequently surprised observers deserves more extended discussion, both because of its practical significance and because of its high relevance to some of the theoretical issues uniquely illuminated by the 1968 election. We speak of the relationship between the Wallace movement and the generational cleavages so evident at other points in data from the presidential campaign.

It would seem self-evident that Wallace's primary appeal to traditional and even obsolescing American values, as well as his caustic treatment of the rebels of the younger generation, would have brought him votes that were even more heavily clustered among the elderly than those drawn by Goldwater in 1964. We have already noted that Wallace took issue positions that were communicated with unusual clarity, and that these positions determined in unusual degree the nature of his clientele. On almost every issue of nearly a score surveyed, the position characteristic of Wallace voters in our sample is also the position associated with older citizens, where there is any age correlation at all. Hence it is somewhat surprising to discover that among white Southerners there is actually a faint *negative* correlation between age and a Wallace vote. And it is perplexing indeed to discover that outside the South voting for Wallace occurred very disproportionately among the young. For example, Wallace captured less than 3% of the vote among people over 70 outside the South, but 13% of those under 30, with a regular gradient connecting these two extremes. One of the major ironies of the election, then, was that Wallace made his appeal to the old but mainly received the vote of the young.

However, a whole cluster of empirical theory has grown up in recent years which, without any particular knowledge of the Wallace platform, would predict that such a third-party candidate would draw votes primarily from the young in just this way. It is established, for example, that repeated commitments of votes to a political party tend to increase the strength of psychological identification with that party, and it is an immediate corollary that voters of the older generation are more fixed in their party loyalties than are relatively new voters.[25] It follows with equal logic that when some new candidate or *ad hoc* party arises to challenge the conventional parties of a system, it should have relative difficulty making headway among the older generation, even though it might have natural appeals to such voters.

We have never had a chance to test this

[25] Philip E. Converse, "Of Time and Partisan Stability," *Journal of Comparative Politics* (issue to be announced).

somewhat non-obvious expectation, although reconstructions of the fall of the Weimar Republic have always suggested that voters for the Nazi Party in its culminating surge were very disproportionately drawn from the youngest cohorts of the German electorate. Therefore the age distribution of Wallace support has been of uncommon interest to us. When issue appeals of a rather vital sort conflict with long-established party loyalties, as they must have in Wallace's case for many older voters, which factor is likely to exert most influence on the voting decision? The apparent difficulties older people had in voting for Wallace, particularly outside the South where he was a less "legitimate" Democrat and hence a less conventional candidate, seem to provide a rather clear answer.

However, if this interpretation is correct a variety of ancillary effects should be discernible in the 1968 data. For example, if prior party identification is truly the critical source of resistance to a Wallace vote simply because of the disloyalty implied, the prediction that the young would vote more heavily for him need not mean the young have any monopoly on admiration for him. Indeed, one could almost predict that the older generation should have shown more warmth of feeling toward Wallace per vote allotted him than would be true of the younger generation, simply because of the "artificial" inhibition on the vote represented by greater loyalty to a conventional party. Moreover, since strength of identification is measured explicitly in this study, it is of importance to show that it does indeed vary positively as in times past with age; that such identification with a conventional party is indeed negatively associated with voting for Wallace; and that the tendency of young persons to vote for Wallace did co-occur with weak conventional loyalties.

All of these empirical expectations are borne out, and usually in rather handsome fashion. First, while the young voted more heavily for Wallace, the correlation between age and affective rating of him as a political figure is nonexistent. Second, the old in 1968 were, as always, much more strongly identified with one of the two conventional parties than the young. Third, defection from a conventional party to vote for Wallace was indeed strongly related to degree of party identification, particularly outside the South:[26] the probability of a Wallace vote doubles there as one moves each step from strong through weak to "independent" or leaning identifiers. And finally, when strength of partisanship is controlled, the sharp inverse correlation between age and a Wallace vote outside the South is very nearly wiped out; within the South where it was a somewhat ragged relationship to begin with, it completely disappears or if anything, shows a slight reversal as though Wallace might in fact have had some extra drawing power for the older voter, aside from the complications posed by other allegiances.

This nest of relationships holds more than detached clinical interest in several directions. The reader concerned about the future of the Wallace movement as an electoral force on the American scene is likely to be interested in the fact that the clientele was young rather than aging. In one sense this is a pertinent datum and in another it is not. It is unquestionable that a Wallace candidacy in 1972 has a brighter future than it would have if its 1968 legions were dying out of the population. Nonetheless, the whole thrust of our argument above is that the Wallace movement is not in any special good fortune to have drawn young voters: this will be true of virtually any new party entering the lists in an old party system, and but for the habits which kept older voters with the conventional parties, the initial Wallace vote would probably have been significantly larger. Still more to the point, we would hazard that the future of the Wallace movement as a third party will be determined more by Wallace's personal plans and the organizational aspirations of his entourage on one hand, and by the evolution of events affecting national frustrations on the other, than by the age level of its 1968 voters.

Nevertheless, the youthful nature of Wallace's clientele provides a further irony to the backdrop of generational cleavage reflected in the 1968 campaign. For while such a cleavage was genuine and intense, as some of our earlier data have witnessed, one of the most important yet hidden lines of cleavage split the younger generation itself. Although privileged young college students angry at Vietnam and the shabby treatment of the Negro saw themselves as sallying forth to do battle against a corrupted and cynical older generation, a more head-on confrontation at the polls, if a less apparent one, was with their own age mates who had gone from high school off to the factory instead of college, and who were appalled by the collapse of patriotism and respect for the law that they saw about them. Outside of the election period, when verbal articulateness and

[26] The South shows somewhat diluted patterns here, compatible with the likelihood that for at least some Southern Democrats, a vote for Wallace was not conceived as a defection.

leisure for political activism count most heavily, it was the college share of the younger generation—or at least its politicized vanguard—that was most prominent as a political force. At the polls, however, the game shifts to "one man, one vote," and this vanguard is numerically swamped even within its own generation.

This lack of numerical strength is no intrinsic handicap: any cadre of opinion leadership is small in number. However, it must successfully appeal to some potential rank and file, and it certainly cannot risk becoming a negative reference point for large numbers of people if it expects to operate in a medium involving popular elections. In part because of collegiate naiveté concerning forms of dissent that maintain sympathy,[27] and in part because the public image of constructive efforts by the many can be so rapidly colored by a few whose needs are mainly to antagonize as much of society as possible, this vanguard became a negative reference point for most Americans. The result at the election thus had a different coloration from what went before: McCarthy did not run and Wallace captured a proportion of the vote which was historically amazing. Indeed, it was probably the political stodginess of the older generation so decried by campus activists which kept the vote of "people over 30" within the channels of the conventional parties and prevented the Wallace vote from rising still higher. Certainly it is true that in several major metropolises of the United States where party loyalty has been nullified in primary election settings in the spring of 1969, candidates of relative Wallace coloration have surprised observers with their mounting popularity.

There can be no question but that dramatic and persistent displays of dissent on the campuses between 1964 and 1968 helped to place question marks around "consensual" national policies which might otherwise have continued to be taken for granted by most of the citizenry. At the same time, disregard for the occasional junctures of electoral decision when the mass public has some say in the political process may mean that a battle was won but a war was lost. For some few, this *politique de pire* is quite intentional, being thought to help "radicalize" the electorate in ways that can be controlled and manipulated. For most student activists, however, success in raising questions is of little value if one is helping in the same stroke to elect "wrong people" to answer them. And quite apart from the nature of the leadership elected in 1968, it is obvious to any "rational" politician hoping to maximize votes in 1970 or 1972 that there are several times more votes to be gained by leaning toward Wallace than by leaning toward McCarthy.

If these facts were inevitable consequences of "raising the issues" from the campuses, the dilemma would be severe indeed. It is not clear to us, however, that any intrinsic dilemma is involved. Much of the backlash expressed in the 1968 voting received its impetus less from irreconcilable policy disagreement—although on civil rights there is more than a modicum of that—than from resentment at the frequency with which the message of dissent from the campuses was clothed to "bait" conventional opinion. In the degree that the feelings and opinion reflexes of the common man, including age peers of lower circumstances, were comprehended at all by campus activists, they tended to be a subject for derision or disdain. Strange to say, such hostile postures communicate with great speed even across social gulfs, and are reciprocated with uncommon reliability. Fully as often, of course, there was simply no comprehension of the dynamics of public opinion at all.

Whether one likes it or not, the United States does retain some occasional elements of participatory democracy. A young and well-educated elite-to-be that is too impatient to cope with this bit of reality by undertaking the tedium of positive persuasion may find its political efforts worse than wasted.

[27] The American public seems to have a very low tolerance for unusual or "showy" forms of political dissent. Responses to an extended set of items in the 1968 study on the subject are appalling from a civil libertarian point of view. At the most acceptable end of the continuum of "ways for people to show their disapproval or disagreement with governmental policies and actions" we asked about "taking part in protest meetings or marches *that are permitted by the local authorities*" (italics not in original question). Less than 20% of all respondents, and scarcely more than 20% of those giving an opinion, would approve of such subversive behavior, and more than half would disapprove (the remainder accepted the alternative presented that their reaction "would depend on the circumstances"). In view of such assumptions, the overwhelmingly negative reaction to the Chicago demonstrations despite sympathetic media treatment (cited earlier) is hardly surprising.

RATIONAL POLITICAL MAN: A SYNTHESIS OF ECONOMIC AND SOCIAL-PSYCHOLOGICAL PERSPECTIVES*

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I. INTRODUCTION

In recent years the welter of data accumulated on American voting behavior has been continually reanalyzed by social scientists interested in building theories of electoral choice. Most of the original data-gathering enterprises were guided by general theoretical frameworks which, for the most part, were not developed to a point where the ensuing analyses addressed themselves unambiguously to the overall conceptions by which they were guided. As a result much of our knowledge about voting behavior is in the form of generalizations about what social and psychological variables account for voting choices while we lack conceptual frameworks which systematically interrelate these generalizations and provide comprehensive and parsimonious explanation.[1] If any one unifying conception has emerged from the original large scale studies it is that the average voter is irrational. This inference has been derived from a variety of empirical relationships coupled with varying conceptions of rationality.[2]

The more recent reanalyses of these data sets have been characterized by a theoretical sophistication that was lacking heretofore. One of these, a theory of the calculus of voting, has applied some formal rigor to the question of the rationality of the decision to vote, selected empirical equivalents of theoretical entities from survey data on national elections, and conducted a successful test of the theory.[3] Unlike traditional approaches to the rationality question which infer the degree of rationality from quantities of information possessed or from correlates of decisions (background, party affiliation, group memberships, etc.), this investigation conceived of rationality in terms of the kind of calculus employed by the individual in deciding among alternatives (in this case whether or not to vote).

The present investigation is an attempt to test a general theory of political behavior whose structure is designed to further illuminate the question of rationality. In this instance the theory is applied to voting intention data obtained with a view toward maximally facilitating the testing of the theory. The substantive focus of the theory is upon the rationality of electoral decisions. As has been suggested above, while most empirical investigations of voting behavior have implicitly or explicitly addressed themselves to the rationality question, the variety of conceptions of rationality along with the differing notions as to what constitutes the appropriate operational procedures with which to approach the question renders an

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[1] The major original voting studies are of course those of the Columbia and Michigan groups whose theories and data analyses are presented for the most part in Lazarsfeld and his associates' *The People's Choice* (New York: Columbia University Press, 1944) and *Voting* (Chicago: University of Chicago Press, 1954) and in Campbell *et al.*, *The Voter Decides* (Evanston, Ill.: Row Peterson, 1954), *The American Voter* (New York: John Wiley and Sons, 1960), and most recently *Elections and the Political Order* (New York: John Wiley and Sons, 1966). Much has been said with respect to both the important contributions and theoretical shortcomings of these studies. What is perhaps the most significant commentary on the state of our knowledge about voting behavior is the structure of a recent book, Lester Milbrath's *Political Participation* (Chicago: Rand McNally, 1965), which relies heavily on the findings of all but the most recent work cited above. Rather than presenting theories of participation, Milbrath is forced to offer a propositional inventory whose only unifying theme is a conceptual model of his own with which he attempts to bring together the various discrete propositions belonging to a variety of conceptions.

[2] Inferences with respect to the irrationality of the voter have been based upon the relative stability of partisan affiliation, the low information level with respect to national policy questions and the apparent influence of primary groups on partisan attitudes. A major attack on the irrationality hypothesis is presented in V. O. Key's *The Responsible Electorate* (Cambridge: Harvard Univ. Press, 1966), which is based upon an analysis of voters who switch parties from one election to the next, a switch which Key's analysis indicates to be issue oriented.

[3] William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," this REVIEW, 62 (March, 1968), 25–42.

assessment of the evidence on either side problematic at best. Here, as in the study discussed above, rationality will be considered in the context of the kind of calculus which can be ascribed to the decision-making behavior of the individual voter.

Difficulties in making inferences about individual voter rationality extend beyond the problem of the variety of conceptions of rationality and the diversity of tests employed in their behalf. Many attempts at developing theoretical approaches to voter, or more generally, participant rationality have employed images of man which are more comprehensive than the actual models of man articulated for purposes of theory construction.[4] These images of man are generally responsive to preferences for particular forms of collective rationality. Behind the conceptions utilized to identify individuals often lurk real or envisioned political systems, the operating imperatives of which provide the criteria for assessing the rationality of the citizen participant. This becomes apparent when inquiries into the determinants of individual political behavior result in claims about political systems. A frequent pattern of inference in studies about the political behavior of individuals is that given what has been discovered about the behavior of individuals, this or that political system would appear to perform well. The circularity involved in such a procedure is obvious when explicated in this manner but is easily overlooked when, with consummate skill or cultivated inattention, theorists smuggle political systems into basic concepts about human nature, human potential, or human behavior.

Certain substantive theories of participation have been particularly prone to such circularity. A recent critique of consensus or civic culture approaches to political man, for example, has stressed the need for more individual oriented investigations which analyze, " . . . the structure of man's mind in relation to the surrounding social universe . . . " rather than juxtaposing, " . . . as the civic culture man does, individuals and institutions and tabulating the former's relations to the latter on a preconceived scale of integrative and disintegrative traits viewed through the eyes of the best available model of state organization . . . "[5] Indeed many of the inquiries into the nature of political man employ conceptions which embody preconceived political systems. Such studies have lacked a sensitivity to the images of individual-other relationships upon which their models of man are based and have thus predicated assertions about individual rationality on disguised commitments to models of collective rationality.

When a particular political system is presupposed in an investigation of political man, almost inevitably the conclusions reached permit questions about the worthiness of individuals rather than about the system. Because, for example, individuals do not exhibit consistent belief systems or ideological orientations with respect to public policies in the form in which they are articulated for and by national legislators, we cannot *ipso facto* assume that their voting choices are irrational, capricious, irresponsible, or even non-policy oriented.[6] Such an inference is predicated on presuppositions about the rationality of the political system which permit the definition of significant policy questions to be undertaken by political elites who aggregate issues with diverse implications into categories presented to non-elites for purposes of informing their electoral decisions. As Rogin has suggested, "An ideology usually reveals more about the preoccupations of elites than of masses."[7]

[4] This distinction between images of man and models of man has been recently discussed by Donald D. Searing, "Models and Images of Man and Society in Leadership Theory," *Journal of Politics*, 31 (Feb., 1969), 3–31. Generally models of man can be thought of as tentative conceptual frameworks which are recognized as being empirically relevant while images are more unselfconscious, reality-filtering mechanisms.

[5] Otto Kircheimer, "Private Man and Society," *Political Science Quarterly*, 81 (March, 1966), 1–24. For a similar critique of consensus approaches which argues explicitly for rational choice conceptions to avoid biasing an analysis in favor of desired political structures, see John Harsanyi, "Rational-Choice Models vs. Functionalistic and Conformistic Models of Political Behavior," *World Politics*, 21 (July, 1969), 513–538.

[6] Defining a belief system in terms of the degree of constraint among idea elements, Converse found that with respect to national, domestic and foreign policy, the average voter demonstrated very little constraint. See Philip Converse, "The Nature of Belief Systems in Mass Publics," in David Apter (ed.) *Ideology and Discontent* (New York: Free Press, 1964), pp. 206–261. For a similar approach emerging with the finding that American attitudes on national policy lack a clear pattern, see Robert Axelrod, "The Structure of Public Opinion on Policy Issues," *Public Opinion Quarterly*, 31 (Spring, 1967), 51–60.

[7] Michael P. Rogin, *The Intellectuals and McCarthy: The Radical Spector* (Cambridge, Mass.: MIT Press, 1967), p. 228.

In an attempt to avoid the conceptual difficulties considered above, this investigation of the rationality of voting intentions employs minimal presuppositions concerning the types of information that voters employ and the criteria they utilize in processing that information prior to their voting decisions. Rather than positing given stimuli to be generally relevant to the electorate with respect to a presidential campaign, an attempt has been made to utilize the information and voting criteria which the voter himself suggests as significant in his voting decision.[8]

II. THEORETICAL OVERVIEW

The theoretical calculus to be utilized in this study is what has been characterized as a rational choice model. In applying such a model to the problem of voting behavior, implicit assumptions are made concerning the cognitive capabilities of the average citizen-voter. Despite the controversy which the ascription of rationality has provoked in the social sciences, as it is employed here, the assumption in the calculus is quite a modest one. The utility models, developed for the most part by economists, which characterize individuals as maximizing expected gains minus expected losses, are predicated on a limited aspect of rationality. Whereas in a broad sense rationality implies a grasp and employment of the rules of inductive and deductive inference, rational choice models assume only the latter kind.[9] The requirement for deductive rationality is simply that the individual choose those actions which he believes to be consistent with his desired goals.

The results of a now classic experiment are supportive of the basic utility formulation, and recently the application of such theories to a variety of social and political problems has been markedly successful.[10] The problem of inductive rationality, on the other hand, has been largely ignored in the utility formulations of economists. This aspect of individual choice behavior has been treated for the most part by social psychologists whose concern has been with the nature of beliefs, attitudes, and values and with the cognitive structures of which these components are a part.[11] A primary focus has been on beliefs or perceptions which are hypothesized to result from other aspects of an individual's attitude-value complex. It has been these models emphasizing the subjectivity of perceptions which have contributed much of the theoretical underpinning to voting behavior studies.[12]

[8] The inspiration for this quasi open-ended interview technique was derived from the self-anchoring striving scale developed by Kilpatrick and Cantril. See F. P. Kilpatrick and Hadley Cantril, "Self-Anchoring Scaling: A Measure of Individuals' Unique Reality Worlds," *Journal of Individual Psychology*, 16 (November, 1960), 158–173. The scale's most notable application is in Hadley Cantril's *The Pattern of Human Concerns* (New Brunswick, N. J.: Rutgers University Press, 1965).

[9] The usefulness of distinguishing between deductive and inductive rationality has been discussed by Quentin Gibson, *The Logic of Social Inquiry* (London: Routledge & Kegan Paul, 1960), chapter XIV, "The Assumption of Rationality."

[10] The original experiment is reported in Frederick Mosteller and Philip Nogee, "An Experimental Measurement of Utility," *Journal of Political Economy*, 59 (1951), 371–404. In addition to the Riker-Ordeshook study cited above, applications of utility-type models include such diverse substantive concerns as foreign policy decision-making, e.g., Barry M. Blechman, "The Quantitative Evaluation of Foreign Policy Alternatives," *Journal of Conflict Resolution*, 10 (December, 1966), 408–426, and child rearing practices, reported in O. G. Brim, *et al.*, *Personality and Decision Processes* (Stanford, Calif.: Stanford University Press, 1962).

[11] The concept of inductive rationality, as it is employed here, refers primarily to the various cognitive consistency models which hypothesize that information processing in attitude formation and choice behavior is partly responsive to motivations for maintaining consistent cognitions about objects and situations. Rosenberg and Abelson, for example, speak of the "induction of new relations," when an individual seeks to render new information compatible with other pre-existing impressions: see Milton J. Rosenberg and Robert P. Abelson, "An Analysis of Cognitive Balancing," in Milton J. Rosenberg *et al.*, *Attitude Organization and Change* (New Haven: Yale University Press, 1960, pp. 112–163).

[12] Among the most significant theories of this type are Fritz Heider, "Attitudes and Cognitive Organization," *Journal of Psychology*, 21 (1946), 107–112, and *The Psychology of Interpersonal Relations* (New York: John Wiley and Sons, 1958); Charles E. Osgood and Percy H. Tannenbaum, "The Principle of Congruity in the Prediction of Attitude Change," *Psychological Review*, 62 (1955), 42–55; Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford, Calif.: Stanford University Press, 1957); Robert P. Abelson and Milton J. Rosenberg, "Symbolic Psychologic: A Model of Attitudinal Cognition," *Behavior*

Despite the basic differences in emphasis of the economic and social-psychological models of man there are some critical convergences which should not be overlooked. Some of the cognitive consistency models of social psychologists possess essentially the same theoretical structures as the utility or rational choice models.[13] We can express the normal utility maximizing formulation as follows:

$$E(U)_1 = \sum_{i=1}^{n} P_i V_i$$

[Where $E(U)_1$ is the expected utility of choice number 1, n is the number of possible outcomes, V_i is the ith outcome affected by the choice, and P_i is the probability that choice number 1 will result in the ith outcome (which may be positive or negative]. It can be seen that cognitive models such as that developed by Fishbein are essentially similar. Based on his behavior theory which combines both beliefs about objects and attitudes toward objects, Fishbein expresses his model algebraically as follows:

$$A_o = \sum_{i=1}^{N} B_i a_i$$

Where A_o is the attitude toward object o, B_i is the strength of belief i about o (i.e. the probability that o is related to some other concept (i), a_i is the evaluative aspect of B_i or the valuation of X_i and N is the number of beliefs about o]. The basic prediction as Fishbein expresses it is, "that an individual's attitude toward any object is a function of (1) the strength of his beliefs about the object . . . and (2) . . . the evaluative aspect of those beliefs."[14] Markedly similar cognitive theories have been formulated and tested by Rosenberg under the general conception of a consistency principle, and Zajonc in terms of a theoretical framework he refers to as the theory of "cognitive set."[15] Successful tests of the Fishbein, Bernstein, and, Zajonc theories along with those of other structurally similar social psychological models buttress, by implication, the basic utility conceptualization of economists.

Essentially, then, both the economic and social-psychological models of man emphasize two basic cognitive components, values and beliefs. The roles that values and beliefs are hypothesized to perform correspond roughly to the processes of prizing and appraising, or as many contemporary attitude theories express it, affect and cognition. The evaluative process which precedes the formation of an attitude or the making of a decision according to these two basic models consists of assessments as to the relative desirability of various outcomes (prizing) and of the objects, persons, or events that are likely to contribute to the outcomes (appraising).

It should be evident that from both the economic and social-psychological perspectives human decision-making is seen to result from the way in which these two basic cognitive components process information from the individuals environment to yield the criteria on the basis of which a decision is reached. The economic rational choice models, emphasizing theoretical rigor, are characterized by a minimum of assumptions and relatively parsimonious theoretical formulations while the social-psychological models tend to be less theoretically compact but more amenable to comprehensive description and explanation of human information processing.

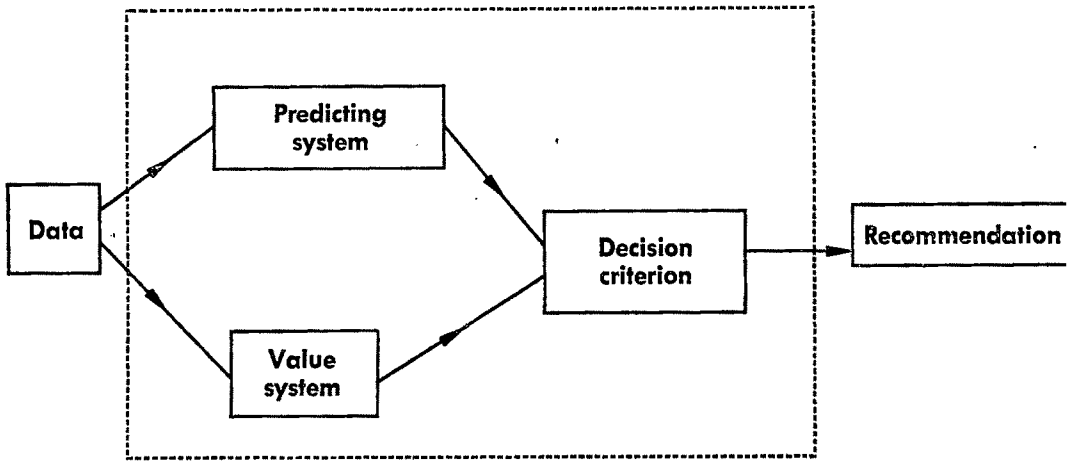
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cience, 3 (1958), 1–13; and Muzafer Sherif and arl I. Hovland, *Social Judgment* (New Haven, onn.: Yale University Press, 1961). A comprehensive review of the relevant literature can be und in William J. McGuire, "The Current tatus of Cognitive Consistency Theories," in . Feldman (ed.), *Cognitive Consistency: Motivational Antecedents and Behavioral Consequences* New York: Academic Press, 1966). Insightful itiques of a number of these theories can be und in Roger Brown, *Social Psychology* (New ork: Free Press, 1965), Chapter 11, and Chester . Insko, *Theories of Attitude Change* (New York: ppleton-Century-Crofts, 1967). The most recent rsions of the various consistency theories appear in Robert P. Abelson *et al.*, *Theories of ognitive Consistency: A Source Book* (Chicago: and McNally, 1968).

[13] This similarity has been noted explicitly in orman T. Feather, "Subjective Probability and ecision Under Uncertainty," *Psychological Review*, 66 (1959), 150–164; and Ward Edwards, Behavorial Decision Theory," *Annual Review of sychology*, 12 (1961), 473–498. I am indebted to ufus P. Browning for calling these references to y attention.

[14] Martin Fishbein, "A Behavior Theory Approach to the Relations Between Beliefs about an Object and the Attitude Toward the Object," in Martin Fishbein (ed.), *Readings in Attitude Theory and Measurement* (New York: John Wiley and Sons, 1967), p. 394.

[15] See Milton J. Rosenberg, "Cognitive Structure and Attitudinal Affect," *Journal of Abnormal and Social Psychology*, 53 (1956), 367–372, and R. B. Zajonc, "Structure of the Cognitive Field," unpublished doctoral dissertation, University of Michigan, 1954. Algebraic versions of both these theories are developed by Fishbein *op. cit.* pp. 394–395.



Block Diagram: Decision-Maker (from Irwin D. J. Bross *Design for Decision* (New York: The Free Press, 1953.)

FIG. 1. The rational choice perspective on decision-making.

From the point of view of the rational choice theorist, a sufficient characterization of an individual decision-maker is represented by models such as that depicted in Figure 1.[16] A belief system and value system are posited as independent information processing units which provide a basis for rational, deductive, utility maximizing, decisions.

Social-psychological models, on the other hand, manifest more concern with individual variation in the process of obtaining and interpreting information as well as with the way in which the information is formally structured to yield decision criteria. Emphasis is placed on various attributes of belief systems such as their degree of rigidity or the breadth of the categories. Similarly variations in value orientations, particularly in terms of breadth and flexibility, are a major consideration. In addition, many of these models focus on dynamic interrelations between cognitive components, dealing with propositions about such phenomena as the extent to which an individual's beliefs or perceptions are affected by intense value preferences.

These many additional concerns with the information processing aspect of cognitive structures is indicative of social psychologists' concern with inductive as opposed to deductive rationality.[17] Even those cognitive models which are structurally similar to utility formulations serve to interrelate hypotheses which focus on the ways in which information is fed into the cognitive structure as well as the way in which it is organized to yield attitude formation and behavior.

Because of their complementary concerns with the deductive and inductive aspects of rationality, it would appear that a synthesis of the economic and social-psychological approaches to decision-making might on the one hand provide a more comprehensive theoretical overview of individual choice behavior and on the other address itself more adequately to the questions of rationality such as that related to the rationality of voting decisions. The structural similarity between economic utility models and some social psychological models makes such a synthesis possible. With this in view, I have chosen a parsimonious rational choice formulation developed by economists and supplemented its usage with assumptions operational definitions, and hypotheses which are compatible with the social psychologists concern with the effects of cognitive structure on information processing in decisional contexts. What emerges is hopefully not only a calculus which is appropriate to the study of the rationality of voting decisions but also a general theory of individual decision-making which emphasizes the interrelationships between values and beliefs in both the assessmen

[16] This diagram is taken from Irwin D. J. Bross, *Design for Decision* (New York: Free Press, 1958), p. 29.

[17] A theory which focuses explicitly on the way in which cognitive structures process information is presented along with some interesting empirical applications in Harold M. Schroder, Michael J Driver and Siegfried Streufert, *Human Information Processing* (New York: Holt, Rinehart and Winston, 1967).

of decisional contexts and in the choice among alternative behaviors.

III. THE MODEL

The utility formulation chosen for this investigation of voting intentions is in the form of a quadratic loss function. The notation of Davis and Hinich (in linear algebra) is as follows:[18]

$$L_1 = (X_i - \theta_i)' A_i (X_i - \theta_i)$$
$$L_2 = (X_i - \theta_i)' A_i (X_i - \theta_i)$$
$$\vdots$$
$$L_n = (X_i - \theta_i)' A_i (X_i - \theta_i)$$

where L_1 through L_n represent an individual's loss functions as a result of the attributes (policies supported, personal characteristics, etc.) of candidates 1 through n; X_i is a vector representing the ith individual's preferred attributes of candidates; θ_i is a vector of the perceived positions of the candidates with respect to the attributes preferred by the ith voter, and A_i is an $n \times n$ positive semi-definite saliency matrix which weights each attribute on the basis of its salience for the ith voter.[19]

It should be apparent that this loss function model is an inverse of the standard utility formulation. Each L represents the disutility of each candidate for a given voter, the prediction being that a given voter will choose the candidate for which the attribute loss is a minimum, i.e., the smallest L.

This model, as conceived by Davis and Hinich, was addressed primarily to the problem of candidate strategy. Thus while the model represents a decision-making calculus ascribed to the individual voter, its primary function, along with the formulae produced by extending the model at a strictly formal level, is as a theory of influence on a group of voters with certain collective characteristics. For purposes of analytic development, then, the assumptions upon which the model is predicated are more restrictive than those which would be considered desirable for a model which focuses primarily on individual choice behavior.

In their analysis, Davis and Hinich assume that all the voters in the population use the same indices to measure a given policy and that each voter has a preferred position for each issue of policy injected into the campaign. These assumptions are reflected in their conception of the X_i vector of voter preferences. The number of elements in the vector is the same for all voters with each X representing a campaign issue and each voter having a score (on a commonly perceived interval scale) on each X, which represents his policy preferences. In like manner the θ vector is assumed to be perceived similarly by all the voters such that a candidate's stand on each issue is identified in the same way by each voter.[20]

The quadratic form of the model is primarily significant for the analytic development it permits for a theory of candidate strategy. There are, however, properties of the quadratic form which are convenient for the individual choice behavior aspect of the model. The calculus involved with this form yields a loss of zero in instances where an X_i-θ_i (when the candidate attribute is equal to the preferred attribute) and a positive loss whenever $X_i \neq \theta_i$. In addition, a quadratic loss function incorporates the concept of an increasing marginal utility which in this case relates to preferred attributes as opposed to the traditional application to economic currency.

In order to employ the model in a manner that provides an overview of information pro-

[18] See Otto A. Davis and Melvin Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in Joseph L. Bernd (ed.), *Mathematical Applications in Political Science II* (Dallas, Texas: Southern Methodist University Press, 1966), pp. 175–208, and an elaboration by the same authors, "Some Results Related to a Mathematical Model of Policy Formation in a Democratic Society," in Joseph L. Bernd (ed.), *Mathematical Application in Political Science III* (Charlottesville, Va.: The University of Virginia Press, 1967), pp. 14–38. For another application and extension of the same theoretical calculus see Peter C. Ordeshook, "A Theory of Electoral Behavior and Party Strategy," Unpublished doctoral dissertation, University of Rochester, 1968. I am indebted to Professor Ordeshook for bringing this theoretical formulation to my attention.

[19] The linear algebra notation is preserved in order to maintain continuity with other applications of the theory. In normal mathematical notation, the calculus could be rendered as $L_c = \sum_{i=1}^{n} d^2_i a_i$. (Where L_c is the loss experienced by an individual voter with respect to candidate c, d_i^2 is the square of the distance between the individual's ith attribute preference and his perception of the candidates' position on that attribute, n is the number of attributes that the voter deems to be relevant to his voting choice, and a_i is the salience or importance of the ith attribute of the candidate with respect to his voting choice.)

[20] In Davis and Hinich's original version of the calculus, the A matrix was assumed to be constant for all voters. In their reformulation, they changed this assumption to allow for variation in the A matrix for the population of voters.

cessing as it relates to individual choice behavior and thus to assess voter rationality more comprehensively than is usually the case, several of the above assumptions have been relaxed. In keeping with the social-psychological emphasis on definitions of the situation and subjectivity of perceptions, it is not assumed that the X vector is the same for all voters. Each voter is assumed to have his own criteria (preferred candidate attributes), and the number of criteria employed by the voters is assumed to vary. Similarly, the θ vectors are assumed to vary across the voting population with the actual position of each candidate on each attribute assumed to be ascribed by each individual voter. Of necessity, then, the A matrix will vary from voter to voter with each diagonal entry representing the relative importance of each of the corresponding candidate attributes adduced by the individual voter. The only unifying assumption for purposes of utilizing the model to explain the choice behavior of all the voters is that the same interval scales are used by the voters for rating candidates on attributes.

Thus while we have preserved the structure of the rational choice model which posits a given functional relationship between beliefs about candidate attributes and valuations of them, the standardizing assumptions have been revised to allow for the inclusion of assessments as to the actual substantive information which voters utilize to fill in such a formal calculus. With the revised assumptions the model is more compatible with social-psychological theories which hypothesize attitudes toward objects to be a function of a multiplicative relationship between objects or situations which are valued and beliefs about the instrumental relevance of the attitude object to those valued objects and states of affairs.

IV. OPERATIONAL PROCEDURES

The substantive decisional setting utilized to test this theoretical model was the 1968 presidential campaign. The theory was employed to predict and explain voting intentions of the electorate from among the five candidates who were, at the time, actively campaigning for the Democratic and Republican Party nominations, Humphrey, Kennedy, McCarthy, Nixon, and Rockefeller. The sample of over 200 of the electorate was selected at random from the population of registered voters on the Island of Oahu.[21]

[21] The sample was drawn on a random probability basis from the list of registered voters on the Island of Oahu which contains approximately seventy-five percent of the voting population of the State of Hawaii.

The interview schedule combined aspects of open-ended and forced-choice techniques. With the exception of party affiliation, each respondent was, within broad category designations, allowed to identify the criteria (major value referents) which he or she deemed important in voting decisions. The major categories for criteria (in addition to party affiliation) were personal qualities, issues, and interpersonal cues. For the party criterion, respondents were asked to identify their party affiliation on the typical seven point scale ranging from strong Democrat through independent to strong Republican. They were then asked to indicate both in general and specifically with respect to the forthcoming presidential election the importance of their party affiliation in their voting choice(s). The ascribed importance of party affiliation on a five point scale ranging from extremely important to totally irrelevant would serve as the weighting factor (in the A matrix) for each individual with respect to the losses which would result from voting for each of the five candidates. The values in the X and θ vectors for the party criterion were obtained by asking each respondent to rate each of the five candidates on the same party affiliation scale on which they had rated themselves. The party criterion thus appears as one value in each of the five loss functions for each individual with the loss with respect to each of the five candidates being a function of the distance between the individual's party affiliation and that ascribed to the candidate weighted by the importance which the individual ascribes to his party affiliation.

The number of values in the five loss functions for each individual resulting from the personal qualities, issue, and interpersonal influence criteria are variable. For these the respondent was asked to name the personal qualities, issues, and interpersonal cues which were of concern.[22] Then, as in the case of the party criterion, they were asked to indicate the general and specific (with respect to the forthcoming election) importance of each personal quality, issue or interpersonal cue in

[22] The questions asked to elicit voting criteria in the personal quality issue, and interpersonal cue categories are as follows: "In assessing the personal qualities of a presidential candidate, what things do you think about"; "What are the foreign and domestic policy questions with which you are, at present, most concerned"; "Can you think of any individuals or groups whose political opinions you pay attention to."

determining their voting decision. Respondents were then asked to rate each of the five candidates on a seven-point scale ranging from extremely close to extremely far in terms of the extent to which each candidate fulfills each named criterion. For practical purposes, the interview schedule provided for as many as eight criteria under each of the three categories.

In order to test the overall predictive capacity of the model, a voting intention was obtained for each of the respondents. They were asked, "If the election were to occur tomorrow, which of the potential candidates would you vote for?" Those who were undecided were asked to indicate which of the candidates they were still deciding between or among.

V. RESULTS

Before examining the extent to which the loss functions provide an explanation of the calculus of voting choices and, by implication, support for inferences about the deductive rationality of those choices, let us consider the nature and structure of the information which the voters themselves bring to such a calculus. Here we shall focus on the inductive aspect of decision rationality by considering the data from the perspective of social-psychological theories about cognitive determinants of information processing.

Looking first of all at the nature of the information selected by voters, the issues which the respondents deemed significant are presented in Table 1 for descriptive purposes. It is clear that the three most frequently mentioned issues are the Vietnam War, Civil Rights, and Civil Disturbances mentioned by 85, 50, and 30 percent of the respondents respectively.[23] The personal qualities mentioned ranged from moralistic attributes such as honesty, sincerity and integrity to skill attributes such as speaking ability, foreign affairs experience, and administrative experience. Interpersonal cues (respected opinions) included family, friends, coworkers, national commentators, all national and local politicians. The party criterion was, of course, standard for all respondents.

The data presented in Table 1 on issues along with responses on other voting criteria simply indicate, at a descriptive level, that

TABLE 1. ISSUE CONCERNS OF THE OAHU ELECTORATE*

Issue	N	Percentage
Vietnam War	183	85.0
Civil Rights	107	49.8
Civil Unrest	64	29.8
Foreign Aid	31	14.4
Poverty	24	11.2
Balance of Payments	24	11.2
Taxes	22	10.2
Korean Ship Seizure	20	9.3
The Draft	19	9.1
Urban Development	19	9.1
Inflation	11	5.1

* Includes only issues mentioned by more than 5% of the respondents.

voters mention party, issue, personal quality, and interpersonal cue criteria that purport to influence their voting choices. Although an overwhelming majority of our sample of voters cited the Vietnam War, for example, as a major voting criterion, one needs to know more about the way in which such a criterion affects information processing as it relates to candidate evaluation before inferences about inductive rationality are warranted.

Social psychologists have developed an explanation of perceptual processes which, when applied to the question of electoral choice, suggests that a voter's attitude toward candidates is not rational (in an inductive sense). If information processing by the electorate is to be ascribed as relatively objective, an image thesis would be implied whereby the attributes of candidates provide stimuli to which the electorate then responds. The conflicting explanation suggested by a number of social psychological models has been described as a perceptual balance thesis which suggests that evaluations of candidates are perceiver-determined. The later thesis would thus assert that the personal preferences such as partisan identifications provide a motivational basis for ascribing positions to candidates on other evaluative dimensions.[24]

Selective perception hypotheses belonging to the perceptual balance theory provided the major explanatory basis for one of the major

[23] An analysis which attempts to differentiate policy orientations in terms of their complexity or sophistication can be found in John Kessel, "Cognitive Dimensions of Political Activity," *Public Opinion Quarterly*, 29 (Fall, 1965), 377–389.

[24] An explication and empirical assessment of these two competing hypotheses is presented by Joseph E. McGrath and Marion F. McGrath, "Effects of Partisanship on Perceptions of Political Figures," *Public Opinion Quarterly*, 26 (Summer, 1962), 236–248.

TABLE 2. CORRELATIONS BETWEEN PARTISANSHIP AND PERSONAL QUALITY, ISSUE, AND INTERPERSONAL CUE RATINGS

Candidates	Personal Qualities	Issues	Interpersonal Cues
Partisan 1 (Humphrey)	.11	.18*	.08
Partisan 2 (McCarthy)	−.08	.02	.02
Partisan 3 (Kennedy)	.03	.08	.07
Partisan 4 (Nixon)	.23*	.17†	.15†
Partisan 5 (Rockefeller)	.16†	.02	.02

* Significant at .01 level.
† Significant at .05 level.

studies of the electorate. Since that time a number of studies have been specifically addressed to the question of whether such attitudes as partisan preference affect perceptions of candidates. Nevertheless, the findings remain ambiguous.[25] In order to see which of the explanations, image or perceptual balance, applies to the data in this study and thereby to investigate the inductive rationality of our sample of the electorate, we will focus on an aspect of the perceptual balance thesis that is especially compatible with the nature of our data.

If perceptual balance mechanisms are operating in our respondents, we would expect that evaluations of candidates (ratings on their distances from the respondent's preferred attributes) would be systematically related to the partisanship of the respondents. To test this implication of the perceptual balance thesis, the correlations between the partisanship ratings of candidates and their ratings on personal quality, issue, and interpersonal cue attributes are presented in Table 2. It is evident that partisan identification is only slightly related to evaluation of candidates on other attribute dimensions in our sample of the electorate, for the correlations are generally quite low in magnitude and only five of the fifteen coefficients are statistically significant.

We can thus conclude that perceptual balance mechanisms have a very modest effect on information processing by the voters in our sample and infer with some confidence that this is true of the Oahu electorate. Nevertheless, the slight relationship discovered warrants further investigation in terms of another facet of the perceptual balance thesis. In some formulations of this theory of information processing it is hypothesized that individuals who are strongly committed to certain attitudes or stands or who are extremely ego involved with their positions are more likely to enforce consistency on information relevant to their positions than are individuals who are less committed or ego involved.[26]

We can translate the above hypothesis into terms relevant to our substantive focus on electoral choice and arrive at the expectation that for those respondents who are more strongly partisan (express a strong party affiliation), we are more likely to find a relationship between ratings of candidates on the party criterion and ratings of candidates on other attributes (personal qualities, issues, and interpersonal cues). To test this aspect of the perceptual balance thesis an index of the closeness of rating between party and other attributes was computed for each respondent by summing the differences between their party ratings and other attribute ratings across all five candidates. Thus each score on the perceptual bal-

[25] The authors of *Voting* (see footnote 1) placed heavy emphasis on perceptual balance mechanisms in their explanation of voter decision-making. Since that time studies employing a variety of techniques have yielded contradictory results. Investigations which support the perceptual balance thesis include Lewis A. Froman, Jr. and James K. Skipper, Jr. "Factors Related to Misperceiving Party Stands on Issues," *Public Opinion Quarterly*, 26 (Summer, 1962), 265–272 and Roberta S. Sigel, "Effect on Partisanship on the Perception of Political Candidates," *Public Opinion Quarterly*, 28 (Fall, 1964), 483–496. Investigations which support a stimulus or nonperceptual bias thesis include Lewis A. Froman, Jr. and James K. Skipper, Jr., "An Approach to the Learning of Party Identification," *Public Opinion Quarterly*, 27 (Fall, 1963), 473–480, and McGrath and McGrath *op. cit.*

[26] See, for example, Carolyn Sherif, Muzafer Sherif, and Roger E. Nebergall, *Attitude and Attitude Change: The Social Judgment-Involvement Approach* (Philadelphia: Saunders, 1965). They found that the degree of ego involvement of respondents (with respect to their partisanship) was systematically related to their perceptions of campaign appeals in the 1960 presidential election.

ancing index for a given respondent is based on fifteen comparisons with a high score indicating a lack of relationship between party and other ratings and a low score indicating the existence of such a relationship.

This index was then correlated with three different measures of the strength of partisanship of the respondents, their general rating of the importance of their party affiliation, their rating of its importance with respect to this election, and their strength of partisanship as indicated by their distance from the independent or neutral position on the seven point party affiliation scale. The correlations (shown in Table 3) indicate that a relationship does indeed obtain, particularly with respect to the most traditional measure of partisanship, the distance from the independent position. In general, then, a perceptual balancing mechanism is only slightly in evidence, and to the extent that it is operating, it is more in evidence among those with stronger partisan commitments.[27]

We have thus far considered the extent to which beliefs and values in the partisanship domain are related to other evaluative dimensions and have found that for the most part non-party assessments of candidates are independent of the respondent's party leanings. In order to consider the degree of relationship among all four evaluative domains (party, personal quality, issue, and interpersonal cue) one can obtain a correlation matrix for all the salience and distance ratings with respect to all four criteria. A factor analysis of this correlation matrix will then reveal the extent of interdependence of evaluation and will provide a comprehensive overview of the question of the effects of perceptual mechanisms on information gathering in electoral decisions.[28]

The factor matrix shown in Table 4 contains six of the seven factors retained for rotation to a simple structure solution. The highest loadings in each factor have been italicized to facilitate identification of the major sources of variation. The varimax rotation scheme employed is designed to provide maximal clarity in interpretation of the factors by maximizing variance in factors so that a few high and many very low loadings result. If, then, party, personal quality, issue, and interpersonal cue criteria represent separate evaluative domains for the voters, we would expect the evaluative ratings within each domain to appear on different uncorrelated factors.

TABLE 3. CORRELATIONS BETWEEN STRENGTH OF PARTISANSHIP AND DEGREE OF PERCEPTUAL BALANCE

Partisanship Measures	Perceptual Balance
Party Importance (in general)	.16†
Party Importance (this election)	.12
Strength of Affiliation	.25*

* Significant at .01 level.
† Significant at .05 level.

A look at Table 4 indicates that this expectation is realized. Factor 1 is clearly an interpersonal cue factor with the only high loadings being the two salience ratings and the average distance ratings for the five candidates on interpersonal cues. Factor 2 is similarly unambiguous. All high loadings are represented by the salience and distance ratings in the personal quality domain. The other two criteria, party and issues, are also on separate factors, but in both cases their high loadings are on two different factors. The party domain is bifurcated in terms of salience and distance with Factor 3 being an unambiguous party distance factor (all high loadings are distance ratings for the five candidates) and Factor 5 being an equally clear party salience factor in which the three high loadings are the three party salience measures.

The bifurcation of the issue domain is of particular significance. Five out of seven of the issue ratings load highly on Factor 6. These loadings are for the two salience measures and the issue distance ratings on three of the candidates, Nixon, Rockefeller, and Humphrey. The distance ratings on issues for the two remaining candidates, McCarthy and Kennedy appear as the two highest loadings on Factor 4. This factor is not completely unambiguous, because there are two moderate loadings from the per-

[27] Because it is often asserted that various kinds of irrationality in voting are more in evidence among voters of lower socio-economic status, we have correlated income level with the index of the degree of perceptual distortion. The correlation coefficient in this case is a non-significant .04.

[28] The factor analysis model employed is a principle components model. All factors with an eigen value greater than one were preserved for rotation. For a comprehensive discussion of various factor models and their implications see Harry H. Harmon *Modern Factor Analysis*, (2nd ed.; Chicago: The University of Chicago Press, 1967).

TABLE 4. ROTATED (ORTHOGONAL) FACTOR SOLUTION OF CORRELATION MATRIX OF SALIENCE AND DISTANCE RATINGS FOR FOUR VOTING CRITERIA†

Variables*	Factors						Communalities
	1	2	3	4	5	6	
Party 1	−06	−06	−08	18	*82*	02	72
Party 2	02	04	−06	05	*83*	07	71
Party 3	15	−06	−07	−06	*65*	−04	46
Party 4	04	04	*−85*	01	09	04	74
Party 5	07	−02	*−73*	−05	29	14	65
Party 6	−05	15	*89*	−07	−13	05	85
Party 7	01	06	*79*	−05	20	13	69
Party 8	05	06	*86*	−03	−03	15	77
Personal Quality 1	05	*−63*	−05	04	16	−10	60
Personal Quality 2	02	*−70*	−03	−04	16	−13	69
Personal Quality 3	07	*−62*	−32	17	−09	−31	63
Personal Quality 4	02	*−65*	−16	−14	06	−13	50
Personal Quality 5	02	*−70*	28	09	−19	−15	64
Personal Quality 6	−03	*−62*	−12	−45	04	11	63
Personal Quality 7	05	*−50*	06	−63	05	17	70
Issue 1	00	−07	−04	−19	20	*−49*	68
Issue 2	−05	−18	−06	−19	18	*−50*	61
Issue 3	09	−17	−23	07	−02	*−79*	72
Issue 4	01	−05	−06	−32	−01	*−75*	68
Issue 5	−06	−27	30	−11	−20	*−61*	59
Issue 6	−03	03	01	*−79*	−10	−32	74
Issue 7	02	00	11	*−84*	−11	−19	77
Interpersonal Cue 1	*78*	05	−11	16	21	00	71
Interpersonal Cue 2	*82*	01	−13	16	20	−02	77
Interpersonal Cue 3	*87*	−07	−14	00	−06	−03	80
Interpersonal Cue 4	*91*	−04	−04	−02	02	−05	83
Interpersonal Cue 5	*84*	−11	18	03	−13	−04	77
Interpersonal Cue 6	*91*	01	02	−17	−04	03	86
Interpersonal Cue 7	*88*	03	13	−21	00	02	84

* Party 1, 2, and 3 are the measures of party salience, the general importance of party, the importance of party in this election, and the strength of the respondents partisan position respectively. For personal qualities, issues, and interpersonal cues, numbers 1 and 2 correspond to the general importance and importance in this election of those attributes and numbers 3 through 7 correspond to the distance ratings of Nixon, Rockefeller, Humphrey, McCarthy, and Kennedy respectively on those attributes.

† Decimal points in coefficients have been omitted.

sonal quality domain, but again these are the ratings for McCarthy and Kennedy. Perhaps the most compelling inference that can be made with reference to the separate McCarthy-Kennedy issue factor relates to the Vietnam issue. At the time the data were collected, McCarthy and Kennedy were the only two major candidates with "anti" positions on the Vietnam War, and it should be recalled (see Table 1 above) that 85 percent of the respondents referred to the Vietnam War as an issue that would affect their voting decisions.

The results of the factor analysis provide further evidence of the inductive rationality of the electorate by indicating a general conceptual independence among the four voting criteria and thus a general lack of perceptual balance mechanisms operating, and by indicating that the two candidates with similar distinguishing policy stands are similarly located in the electorate's perceptual space.

We are now prepared to turn to a consideration of the deductive aspect of voter rationality by examining the extent to which our loss function equations presented above successfully predict the voting intentions expressed by the

respondents. The sample of voters for this purpose is reduced from 215 to 201 by eliminating those respondents who either indicated no voting intention or an intention other than one of the five candidates for whom we have generated prediction equations. It was found that of these 201 respondents, the loss function equations correctly predicted the choices of 172. This represents an 85.5 percentage, indicating that a vast majority of our sample of the electorate exhibits a deductive (utility maximizing) rationality.[29]

The breakdown of the percentages correctly predicted for the different categories of party affiliation are shown in Table 5. While these partisan differences are not dramatic, the lowest predictability (and thus rationality) of the voters who identify themselves as independents should be expected. Systematic voting behavior studies over the past few decades have consistently indicated that the independent voter tends to be relatively uninformed and inattentive to political matters.[30]

We have found that generally the loss function equations perform well in predicting the voting intentions of the electorate. While the equations are relatively simple, they do represent a combination of elements which warrant individual assessment. In order to determine the contribution of the A matrix or saliency ratings to the overall result, the loss values of the respondents were recalculated using only the distances on the criteria they mentioned without weighting those distances in terms of the ascribed importance of each criterion. On the basis of these new values the number of voting intentions correctly predicted was diminished from 172 to 158 or from 85.5 to 77.5 percent, suggesting that the salience portion of the calculus contributes positively to the overall prediction.[31]

[29] The overall prediction of 85.5 percent is based upon two different kinds of voting intentions. 39 of the 201 respondents included in the prediction did not indicate a definite voting choice but rather named two or more candidates among whom they said they were deciding. They were considered correctly predicted if the loss function equations predicted one of those that they named. A separate analysis of only those with definitive choices, however, indicates that the 85.5 percentage is appropriate with or without the 39 respondents.

[30] The predictions in terms of the loss function equations were examined for different income groups and it was found that there is very little variation in percentage prediction on the basis of income level.

TABLE 5. PREDICTION RESULTS FOR PARTISAN CATEGORIES

Partisanship	N	Percentage
Democrats	94	88.3
Independents	56	81.5
Republicans	51	84.3
Total	201	85.5

The remaining portion of the equations to be assessed are the candidate distances on the four categories of voting criteria. Because the factor analysis indicated that these distances are uncorreleted, it is possible to employ multiple regression equations and obtain unbiased assessments of the relative contributions of these four criteria in predicting voting intentions on the five candidates. The regression coefficient and partial correlation coefficients for the regression of the voting intentions on the four voting criteria are presented in Table 6. Considering the regression coefficients, first of all, we note that for all five candidates, the personal quality criterion accounts for as much or more variation in voting intentions than any of the other three criteria. Personal qualities is the leading criterion for three out of five candidates, the exceptions being Nixon, for whom the party criterion has an equal impact, and Kennedy, for whom the issue criterion has an equal impact. In the case of Nixon, this is to be expected on the basis of our findings on the relationship between partisanship ratings and assessments on other criteria reviewed in Table 2. The perceptual balance mechanism was most in evidence with respect to Nixon for whom all three correlations between partisan position and other categories were statistically significant.

The issue criterion is most significant in the case of Kennedy for whom it shares the lead with the personal quality criterion. Generally, the least important influence in the equations is the interpersonal cue criterion. The regression coefficients are especially helpful in giving us a basic profile of the appeal of each candidate.

[31] It should be recognized that the sizes of the parameters in the loss function equations (a five point scale for salience and a seven point scale of attribute distances) are arbitrary. For this reason, several analyses of the overall predictions were conducted with the relative sizes of the saliences and distances varied. It was found that the scales as originally employed provided the optimum prediction.

TABLE 6. REGRESSION AND PARTIAL CORRELATION COEFFICIENTS FOR THE REGRESSIONS OF VOTING INTENTIONS FOR THE FIVE CANDIDATES ON THE FOUR VOTING CRITERIA

Candidate	Party	Personal Qualities	Issues	Interpersonal Cues
Humphrey				
regression	−.06	−.09	−.05	−.01
partial r	−.26*	−.31*	−.18*	−.05
McCarthy				
regression	−.03	−.05	−.03	−.01
partial r	−.15	−.20*	−.15†	−.05
Kennedy				
regression	−.04	−.05	−.05	−.03
partial r	−.19*	−.19*	−.22*	−.16†
Nixon				
regression	−.08	−.08	−.00	.02
partial r	−.38*	−.30*	−.01	.11
Rockefeller				
regression	−.02	−.06	−.02	−.03
partial r	−.10	−.19*	−.08	−.14†

* Significant at .01 level.
† Significant at .05 level.

A comparison of the coefficients in each row indicates the relative importance of each in relation to changes in voting intentions for each candidate. The partial correlation coefficients are indicators of strength of association and each coefficient in a given row is an index of the degree to which variation in a particular voting criterion is associated with variation in voting intentions for that candidate when we hold constant variation in the other three voting criteria.[32]

A general summary of the overall importance of each voting criterion in terms of the performance of the loss function equations is provided by considering the number of equations in which each criterion is significantly associated with voting intentions. If we employ the 95 percent confidence level, we find that again personal qualities lead, being significantly associated with the dependent variable in all five equations. Party is significantly associated with voting intentions in four out of five equations, issues in three out of five and interpersonal cues in only two out of five equations. The proportion of significant partial correlation coefficients can be employed as an indicator of the degree of redundancy in the loss function model in general. It can be noted that only six out of twenty of the partials are not statistically significant.

[32] For a discussion of the relative explanatory contributions of regression and correlation coefficients in multiple linear regression, see Mordecai Ezekiel and Karl A. Fox *Methods of Correlation and Regression Analysis* (New York: John Wiley and Sons, 1959), section III.

VI. CONCLUSIONS

The evidence adduced above, in connection with the theoretical formulation of electoral decision-making employed, suggests that for our sample and with respect to our referent population of voters, voting choices are rational. This claim rests, of necessity, upon the conception of rationality that has been utilized in this investigation and, in addition, upon the relationship between findings on information processing or inductive rationality and those on the utility maximizing or deductive rationality.

Considering, first of all, the conceptual aspect of this study, we began with an approach to the rationality question which attempted to exploit the structural similarity and substantive complementarity of economic and social-psychological approaches to decision-making. By focusing upon both perceptual mechanisms in the obtaining of decision relevant information and the affectual and cognitive nexus as it relates to choices among perceived alternatives, we have attempted to develop a theoretical formulation that comprehends both the inductive and deductive aspects of rationality, the former concerning itself with the degree of bias in information processing and the latter with the extent to which choices are consistent with a utility calculus.

By conceiving of rationality in terms of the structure of a decisional calculus and the interrelationships between perceptions and experience we avoid presuppositions about the particular values and substantive information relevant to decisions. In the case of voting decisions, for example, it is exceedingly problematic to choose a set of value criteria which belong to voting decisions irrespective of the nature of the voters involved. Some voters derive gratification from knowing that a given type of individual is in office, some are more concerned with the policy choices that an office holder is likely to make, while others find fulfillment in making choices which they feel are congruent with the choices of others whom they admire or respect. The elevation of any particular criterion above others such that choices are considered rational to the extent to which they are designed to maximize it, usually involves the pre-selection of some notion of collective rationality. The relationship between individual and collective rationality is indeed an important one, but it should be explicitly formulated if theoretical approaches to it are

to maintain some integrity. Unfortunately, many approaches to individual rationality attempt to smuggle in collective goals considered desirable so that individual choices not designed to fulfill them can bear the onus of being irrational.

Turning now to the results obtained with respect to our conception of rationality, it is important to note that the significance of the overall predictive capacity of the theoretical formulation (the loss function equations) is contingent upon the results of the hypothesis testing with respect to evidence of perceptual mechanisms affecting the information processing of voters. If there had been evidence indicating a substantial amount of perceptual balancing in the evaluating of candidates the predictive success of the loss function equations would not be particularly significant. The implication would have been that each respondent had simply given his preferred candidate a good rating on all attributes. This would suggest that the loss function equation was just a formalization of the hypothesis that voting intentions are indicated for the candidate whom the respondent likes. Because, however, ratings of candidates in the four categories, party, personal qualities, issues, and interpersonal cues, were shown to be independent, the loss function formulation appears to be a meaningful calculus which represents a reasonably complex decisional process.

In addition to their relevance to the rationality of voters, the results of this investigation (particularly the predictive success of the loss function equations) confer some respectability upon the decision-making theory derived from both economic and social-psychological perspectives on political man. In this application, the theory has been used to clarify the rationality question and explain voting choices. The theory is general in form, however, and might well be successfully applied to other decision-making contexts.

PARTICIPANT CITIZENSHIP IN SIX DEVELOPING COUNTRIES

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In this paper I will endeavor to do the following: (1) test how far certain concepts dealing with individual orientations to politics, previously used in studies of relatively advanced European societies, are appropriate to populations in developing countries; (2) ascertain how far these separate dimensions of individual political orientation cohere as a syndrome, indicating the existence of a general underlying dimension of "participant citizenship;" (3) identify elements among common orientations to politics which cannot be incorporated in this general syndrome; and (4) assess the importance of certain social experiences or forces in inculcating the qualities of participant citizenship in individuals exposed to these influences.

These objectives will be better understood if they are seen in the context of the larger research program of which this report is a part, namely the Harvard Project on the Social and Cultural Aspects of Economic Development.[1] The project is an investigation of the forms and sources of modernization *in individuals*. Its focus is on the person rather than the society or the institution, and its emphasis is socio-psychological rather than purely sociological or structural. Six countries are represented: Argentina and Chile, East Pakistan and India, Nigeria and Israel. This report will not, however, emphasize national differences, but rather treat each sample as another replication of the basic design. We assume that if something holds true for six such different countries, it must be a powerful connection indeed.

The study is based on an extensive interview, of up to four hours duration, administered to a highly purposive sample including subgroups of cultivators, migrants to the city newly arrived, urban workers earning their living outside large-scale productive enterprises, and workers in industry. Industrial workers are the largest group in each country, some 600 to 700, whereas the other sub-groups were to be 100 each. The targets were not, however, always reached. The selection of cases was on the basis of the respondent's meeting certain common characteristics as to sex (all male), age (18–32), education (usually 0–8 years), religion, ethnicity, rural or urban origin, residence, and, of course, the occupational characteristics already mentioned.

Respondents were chosen within "sites," the most important being the factory. Up to 100 factories were included in each country. In practice virtually everyone meeting the sample criteria was selected from each factory, except the very largest. In those, up to 20 men were selected at random from among the pool of eligible subjects. Factories were selected on the basis of differentiation by size (5 categories), product (7 categories) and relative "modernity" (2 categories). Villages were chosen on the basis of being either the same as those from which the migrant industrial workers had come originally, or as being precisely equivalent in region, culture, crop, and the like. Urban non-industrials (UNI's) had to work outside large-scale production organizations in the same cities as the workers, and otherwise meet the general sampling criteria. Since we are not making generalizations to the national populations, we more emphasized keeping the sub-samples like each other in all respects except occupation, rather than selecting them to be representative of any "parent" population. Nevertheless, our industrial samples prove to be very similar to their defined parent populations, although more so in some countries than others.

[1] My chief collaborators from the early days of the project were Howard Schuman and Edward Ryan, who served respectively as field directors for Pakistan and Nigeria, and David H. Smith who was my assistant in Chile and later was assistant director of the project in Cambridge. The field work and later analysis were greatly facilitated by the work of our local collaborators in all six of the countries. We owe particular debt to Juan Cesar and Carlotta Garcia, Perla Gibaja, and Amar Singh who were field directors for Chile, Argentina, and India, respectively, and to Olatunde Oloko who was assistant field director in Nigeria. In its different aspects, stages, and settings the research has been supported by the Rockefeller Foundation, the Ford Foundation, the National Science Foundation, and the National Institutes of Mental Health. The Cultural Affairs Division of the Department of State provided local currencies to support our field work in India, Israel and Pakistan, and the Office of Scientific Research of the U. S. Air Force supported technical exploration in problems of translation and computer analysis undertaken in Cambridge. All of these organizations gave their support through the Center for International Affairs of Harvard University, which is the sponsor and institutional home of our project.

The main objective of the larger research program is to test a theory concerning psychosocial modernity. Individual modernity is here conceived of as a complex set of inter-related attitudes, values, and behaviors fitting a theoretically derived model of the modern man, at least as he may appear among the common men in developing countries.[2] Whether this syndrome actually exists in nature, and is the same from country to country, is one of the prime questions to which the project addresses itself. We already have an answer to this first question: the syndrome does indeed exist in nature. Furthermore, the psychological "structure" of individual modernity is basically very much the same in all six of the very different countries we studied.[3]

The second major objective of the Project is to assess the determinants of individual modernity. We are asking: What makes a man modern? Data bearing on this issue have not previously appeared in print. In this article an answer to that question is essayed with reference to one component of our model of the modern man, namely his role as a participant citizen.

I. THE THEORY OF PARTICIPANT CITIZENSHIP

The Harvard program of research on the social and cultural aspects of economic development has been guided by two different, but not unrelated, models of the "modern" man. One, the "analytical" model, specifices a set of relatively abstract qualities, such as "openness to new experience," which we assumed to cohere as a syndrome defining a part,cular personality type. The second model, called the "topical," focused on substantive issues such as religion, family planning, and kinship obligations.[4] With regard to each of these separate topics, we were faced with the challenge of specifying what complex of attitudes, values, and behavior patterns we would consider a "modern" orientation in the given realm.

In the case of politics, the temptation was great to define as modern those qualities presumably characterizing the participants in a democratic polity. To do this, however, was to open ourselves to the charge of being culture-bound, a stigma which students of comparative systems wish to avoid at all costs. Moreover, the historical record gave little reason to assume any inevitable, or even highly probable, association of economic development and a democratic polity. Admittedly, Great Britain was the unchallenged forerunner in the industrial revolution, and the United States has these many years been its overwhelmingly outstanding exemplar, and both are well known for their long tradition of democratic politics. But Japan modernized in the reign of an Emperor who stood at the head of a state religion; Turkey made its great leap forward under a dictator who was rather like Russia's extraordinary modernizing Tsar Peter; the Soviet Union, and more recently China, undertook their modernization under the aegis of communist dictatorships; and Mexico, which many consider the outstanding example of development in Latin America, has made its progress under the tutelage of a one-party system even while maintaining the external forms of competitive democracy. Clearly each of these states must have demanded quite different qualities in its citizens, yet a number of them have rather impressive records of performance as distinctly modern societies.

In the face of these facts, we can hardly insist that the qualities which most characterize the democratic man are necessarily the best, certainly not those which exclusively suit one for life in the political structure of a modern society, at least not so long as we acknowledge the modernity of Russia, Japan, and Germany.[5] Nevertheless, we should not overlook the possibility that the political systems of

[2] A brief description of this model appears in Alex Inkeles, "The Modernization of Man," in M. Weiner (ed.), *Modernization* (New York: Basic Books, 1966). A full account is contained in Alex Inkeles, "A Model of the Modern Man," in Wm. A. Faunce and H. Garfinkel (eds.), *Cross-Cultural Research in Developing Areas* (East Lansing, and New York: Social Science Research Bureau and Free Press, (forthcoming).

[3] A preliminary report of these findings may be found in David H. Smith and A. Inkeles "The OM Scale, A Comparative Socio-Psychological Measure of Individual Modernity," *Sociometry*, 29 (1966), 353-377

[4] See Inkeles, "A Model of the Modern Man," *loc. cit.*

[5] We do not mean to ignore the substantial body of writing in which it has been argued that Russia, Germany, and Japan, while important examples of a modernizing tendency, have indeed not yet attained a "true" condition of modernity. To engage in that particular dialogue would take us far afield from our main task here. For further discussion see Robert E. Ward and Dankwart A. Rustow (eds.), *Political Modernization in Turkey and Japan* (Princeton: Princeton University Press, 1964); and Robert Bellah (ed.), *Religion and Progress in Modern Asia* (New York: Free Press, 1965).

England, the United States, Soviet Russia, Communist China, Japan, and Mexico might all make certain common demands on their citizens. If we could discover such common requirements to exist despite the enormous differences in the formal structure of these political systems, we surely would be entitled to speak of these qualities as truly appropriate to the *modern* (as opposed to the democratic) citizen.

What modern polities otherwise as diverse as the U.S., the U.S.S.R., England, and Japan have in common is most easily grasped if we contrast them with the societies of Europe before the rise of the national state, or with great but uncentralized culture areas such as India before and in the early stages of British colonial rule, or with much of tribal Africa today. The contrast can perhaps best be summed up in the word *citizenship.*[6] Before the advent of the modern centralized state, the individual is a *member* of a family or tribe and he *belongs* to a community, but his identity as a person does not include his being a *citizen* of a nation. When, in such societies, there is a higher and even quite powerful central authority, some king or emperor, the individual may know himself as subject to the sovereign's control but this subjugation is much like his bowing to the wind and waves. In such settings the common man does not believe that he can remotely influence the policy to which he is subjected, let alone make it.[7]

The modern nation-state, democratic or dictatorial, multi- or single-party, guaranteeing private property or exercising state control of wealth, requires and encourages its subjects to adopt a different relation to the government. First and foremost, it wants a man to know not merely his local community, his tribe and family, but to be aware of and to identify with that larger community which is the nation as a whole. It is, furthermore, expected that this new *awareness* will be linked to a shift in *allegiance,* from the local and peripheral to the national and central authority. A man must be prepared to follow not only his father and his tribal chief, but also, and even before these others, his country's prime minister or president.[8] The modern government does not value a passive citizen, but rather an active one. He is expected to participate in the process of running the country. Even in dictatorial and totalitarian systems, which drastically limit the citizen's ability to influence policy in actuality, the state nevertheless attempts to create the illusion that it expresses the common will, and urges almost everyone to take some part in political acts even though they may be fully staged and orchestrated by the supreme leader.

The modern state seeks to organize, and where necessary, to mobilize all its citizens to attain national goals of economic development, of civic improvement, of defense or expansion. This requires frequent communication between the government and the citizen, and he is expected to take an active *interest* in public affairs and to keep *informed* about important events and decisions. Yet neither interest nor information alone can guarantee effective performance of the citizen role. The complexity of modern political life requires of the citizen some understanding of the larger processes of a system in which bureaucratic rule and impersonal criteria of judgment replace treatment based mainly on special personal qualities, on family ties or friendship and connections.[9] The modern polity is suffused with that peculiar bureaucratic rationality which Weber identified as particularly outstanding in the economic behavior of modern man.

Thus, on theoretical grounds, we identify a set of related traits we may call "participant citizenship." The syndrome includes: freedom from traditional authority or, stated positively, identification with and allegiance to leaders and organizations transcending the parochial and primordial; interest in public affairs validated by keeping informed and expressed through participation in civic action; and an orientation

[6] See T. H. Marshall, *Class, Citizenship, and Social Development* (New York: Doubleday & Company, 1964).

[7] In this description we have condensed, and blended, elements of what Almond and Verba term the "parochial" and "subject" political cultures: Gabriel Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), ch. 1.

[8] Lucian Pye, *Politics, Personality and Nation Building* (New Haven: Yale University Press, 1962), identifies this problem of developing identification with and allegiance to the national state as the highest priority task of the developing nations.

[9] We are not unaware that pull and influence, family connections and bribery, are common enough in modern states, including and perhaps even especially, in the more democratic ones. But we believe it correct to assert that *expectation,* and the formal system of *values,* in the modern polity run very much counter to such behavior. Whether or not corruption is in fact less common in modern polities, which we believe it to be, would be difficult to establish beyond peradventure.

toward political and governmental processes which recognizes and accepts the necessity and desirability of a rational structure of rules and regulations. This syndrome is one any modern man might be expected to manifest, just as any modern polity is likely to desire, perhaps even require, that he possess it.

Those familiar with *The Civic Culture* will recognize these traits as very similar to those delineated by Almond and Verba as defining the model of a democratic citizen. According to them the citizen of a democratic polity is expected to take an interest in politics, engage in political discussion, participate by voting and other actions, have knowledge of politics, and express a general sense of competence to influence the government; in sum, to stress "activity, involvement, rationality." I quite agree that these are the qualities appropriate to and expected in the citizen of a democracy. But I hold that exactly the same qualities are appropriate to, and expected of, the citizen of a one-party dictatorship such as that found in the Soviet Union! The difference in what is expected of a citizen of a democracy as against one in an extremist or totalitarian regime lies not in the qualities designated above, but along quite different dimensions. I have elsewhere delineated the non-democratic syndrome as including: "exaggerated faith in powerful leaders and insistence on absolute obedience to them; hatred of outsiders and deviates; excessive projection of guilt and hostility; suspicion and distrust of others; dogmatism and rigidity."[10] Any, indeed all, of these qualities which are common in the non-democratic personality can quite easily be combined with any or all of the qualities which Almond and Verba defined as the characteristics of the democratic citizen. It seems, therefore, that Almond and Verba have labeled as specifically "democratic" something which is a more general requirement placed upon the citizen of a modern polity, whether democratic or otherwise.

By stressing those qualities which a Soviet, an American, a Japanese or a British citizen should have in common, we inevitably gloss over differences which are enormously important if one is interested not so much in the forms of politics as in its content, not so much in action as in values. Voting is a political act equally encouraged in the Soviet Union and the United States, but we consider it to be a purely symbolic gesture in Russia whereas we consider it an action of substantial consequence in America. A man may take an interest in politics and keep well informed, but he may nevertheless judge his government's action inadequate or even evil. Someone who joins a mob screaming for the head of the prime minister engages in a political act no less than one who dutifully salutes as he marches past the minister's grandstand in the independence day parade. We must then recognize the *evaluative* and *judgmental* element of the individual's confrontation with the state as a potentially independent component of his total political role. To be interested is not to approve. To act is not to acquiesce. A man's judgment of his governmental system may be as negative as his understanding of it is complete. He may very much feel himself to be a good citizen, but be deeply offended by the behavior of others he considers either bad citizens or unworthy of citizenship altogether. We touch here on the familiar themes of radicalism or rejection of the political system, and of apathy and alienation, or withdrawal from politics.

Although we can perhaps get ready assent to the hypothesis that the elements of participant citizenship constitute a syndrome of intimately related political orientations, the case for an underlying common dimension is not so obvious when we enter the evaluative realm. Radical rejection of one's political system perhaps assumes hostility to those in power, but a man who joined a revolutionary party might feel quite benign toward most other groups working in the political arena. A person may feel that the particular politicians now in office are a bad lot who are not doing their job of serving the public interest, but he may nonetheless consider the governmental *system* under which he lives the very best there is requiring not the slightest change in *basic* structure.

To some this line of argument will seem specious. In common experience men who want revolutionary change in the structure of government also hate those identified with the system they want changed. Communists are generally hostile not only towards capitalists, but towards their competitor socialists as well. Those who suffer from profound anomie seem to express rather general hatred, and those who hate usually want basic changes. Experience is here supported by systematic research, at least in the United States, which shows that alienation is a general syndrome. Those tainted by it reflect this basic orientation in all aspects of their political life. Such alienation, furthermore, is apparently strongly correlated with the holding of extremist views about the

[10] See Alex Inkeles, "National Character and Modern Political Systems" in Francis L. K. Hsu (ed.), *Psychological Anthropology: Approaches to Culture and Personality* (Homewood, Illinois: Dorsey Press, 1961).

structure of the political system.[11] Alienation, hostility, and radicalism might then also be a syndrome of intimately linked modes of relating to the political order, so that someone markedly characterized by one trait in the set would be so on the others as well.

One may, indeed, take the even more extreme position of arguing that both participant citizenship and political benignity generally go together. A case can certainly be made for the idea that the complex functioning of a modern political system requires that citizens be not only active and interested, but that they show tolerance for the other participants and have reasonable respect for those who are trying to run the government. Quite apart from what the political system may "require," one can argue persuasively on purely psychological grounds that a person who is informed, active, interested, and rational about politics is more likely to also be tolerant of politicians, to harbor friendly rather than hostile feelings towards other major groups in the society, and perhaps even to take a more positive view of the political system at large.

Faced by these competing theoretical positions, we adopted the narrower definition of the modern political man as the more or less official standard for our project. We were, on the whole, convinced that the same forces which act to make a man modern in other ways should also influence his attitudes toward political life and the style of his political participation. Nevertheless, we were not prepared to argue that political modernity is all of a piece. It seemed highly likely that active citizenship is indeed a cohesive syndrome of related characteristics. We were, however, less certain that alienation, resentment of the inadequacy of the polity to produce what is expected of it, and hostility to other groups in society also forms a separate cohesive syndrome. Forced to decide, however, we predicted this subset of measures would also cohere as a common factor. Whether or not there is a separate "resentment syndrome,"[12] however, we were not prepared to say whether it or its components should or should not be systematically related to the active participation syndrome. To the extent that participant citizenship and political benignity empirically went together we would be encountering in nature the embodiment of that set of qualities Almond and Verba used to describe the citizen of the democratic polity, who, they say, has:

> a sense of identity . . . [in] a political community in which one trusts and can cooperate politically with one's fellow citizens, and in which one's attachment to the political system is deep and effective. (503)

These theoretical concerns, approached within the limits set by the nature of the larger study, helped to define the set of four tasks which we assigned ourselves in undertaking the empirical analysis of our data on citizenship attitudes and behavior.

II. EMPIRICAL TEST OF THE THEORY

The first step in our analysis was to ascertain how far the concepts which had been used extensively to measure political orientations in more developed countries were meaningful in the study of our samples of the common man in six underdeveloped nations. In designing our questionnaire we conceived of the political realm broadly, and included questions to assess attitudes towards citizenship obligations, identification with the nation-state, and the degree and forms of the individual's participation in politics. We also included numerous questions to measure political information, to require people to evaluate the effectiveness of their government, to test the degree of their interest in politics, and to measure the amount of hostility they felt for other groups in the society. Altogether our core questionnaire included 33 items measuring attitudes and behavior relevant to politics, broadly conceived, which had been asked in more or less the same way in all six countries. Restricting ourselves to this set of core questions greatly facilitated cross-national comparisons.[13]

This core set of questions was grouped in

[11] H. McClosky and J. Schaar, "Psychological Dimensions of Anomie," *American Sociological Review*, 30 (1965), 14–39; H. McClosky, "Personality and Attitude Correlates of Foreign Policy Orientation," in J. N. Rosenau (ed.), *Domestic Sources of Foreign Policy* (New York: Free Press, 1967).

[12] We borrow here from the essay of Max Scheler, *Ressentiment* (New York: Free Press, 1961), in which he describes a psycho-political syndrome having the characteristics we have sketched above.

[13] The core set initially contained 39 questions, but local circumstances dictated the omission of certain items in some countries—3 in Pakistan, 2 in Argentina, and 1 in Israel. In Chile, Argentina, and Israel, in which time-pressure was less severe and the field directors felt greater freedom to touch on sensitive political issues, these 39 core questions were supplimented by others of local interest, up to a total of 54. Those additional questions are not considered in this report.

what we conceived as a set of scale areas, topics or themes. The original design of the questionnaire suggested the broad outlines for this grouping,[14] but we later adapted the arrangement in consideration of the theoretical issues raised above. The list of questions, each in necessarily abbreviated form, is presented in Exhibit A (see below), grouped under the relevant scale headings. After the items were thus grouped on the basis of theory and "face validity," they were obliged to pass an empirical test of coherence. The resultant scales were as follows:[15]

Identification and allegiance: to test the individual's identification of himself as a political personage, as a citizen rather than someone limited to primordial ties, and to probe how far he would go in following national or modern, rather than purely local or parochial, leadership in case he received competing advice.

Interest in polictics: to assess the extent of a man's interest in political rather than purely personal, familial, or parochial matters as judged by what he most follows in the news, is able to say about the problems facing his nation, and talks about with his wife.

Political information: to gauge the extent of a man's knowledge of international and national political figures and his ability to identify major world capitals. We hoped thereby to see how far expressed interest is translated into effective knowledge.

Participation in civic affairs: to measure whether a man actually joins civic organizations, contacts government officials or politicians, and is prepared to take active measures to oppose a government regulation he considers unfair.

Political rationality: to use the term rational here may assume too much, especially since we mean its opposite to be not necessarily irrational, but rather "particularistic," i.e., action outside the pattern of the formal rules usually pertaining in a modern bureaucracy or polity. In any event, we included here the belief that a government should sooner grant a petition to the man with the *right* to it by law rather than to a man more needy or influential; that education more qualifies a man to hold public office than do illustrious origins or charismatic personality; and that the country would benefit more from a good government plan or hard work by people than from prayer or good luck.

Political anomie: The concept of anomie has become one of the most widely used, and some would say abused, in contemporary political sociology. To add to the confusion it is often used interchangeably with the equally popular concept of alienation.[16] As originally used by Durkheim, anomie applied mainly to the social order, indicating a general condition of normlessness or failure of individuals to agree on and to follow the rules of society. It has since been widely used to designate a condition of the individual, either as himself not knowing or following the rules, or as *feeling* that others are not complying. We use it in the latter sense, and restrict its application to the individual's *perception* of the extent to which politicians and government officials pay attention to the common man, serve the public rather than their own careers, and keep their campaign promises after the election. Those individuals who see the politicians as failing in these obligations we define as having feelings of political anomie. Those who perceived politicans and government officials as fulfilling their obligations we score as non-anomic.

Evaluation of government effectiveness: we address ourselves here to judgments about how good a job the government is doing, what Almond and Verba would call the "output" side of government activity. We limited our study to judgments about the opportunities available to the common man, asking whether they were increasing or decreasing so far as concerned his chances to improve his economic condition, to get attention from the government, and to secure an education for his

[14] Along with a number, all items in our questionnaire bore letters which were codes designating the content or topic of the question. Those coded ID dealt with national identification; AC with "action in politics"; CI with citizenship; PA with "particularism," and so on.

[15] The individual's score on each sub-scale ranged from a minimum of 1 to a maximum of 2. This followed from the fact that each item was dichotomized as closely as possible to the median in each country, with a score of 2 assigned to the "modern" answer, 1 to the "traditional." Averaging by the number of items in the scale kept the range of the several scale scores uniform regardless of the number of items in the scale. To simplify reading the score results, we adopted the convention of treating scores of 1.00 as equal to 0 and 2.00 as equal to 100, enabling us to express individual scores on the common scale of 0–100.

[16] Melvin Seeman, "On the Meaning of Alienation," *American Sociological Review*, 24 (1959), 783–791. For discussion of findings, see H. McClosky and J. Schaar, *op. cit.;* Arthur G. Neal and S. Rettig, "Dimensions of Alienation Among Manual and Non-Manual Workers," *American Sociological Review*, 28 (1963), 599–608.

EXHIBIT A

Scale		Items Included	Possible Responses	
			Identity Primal	*Identity Political*
Allegiance[1]	CI-7	Whose advice would you more follow?	Church	Government
	CI-8	Whose advice would you more follow?	Friend	Political party
	CI-9	Whose advice would you more follow?	Father/Grandfather	Trade Union
			Low Interest	*High Interest*
Interest in Politics	MM-10, 11	Which news interests you most?	Sports/Religion	World/National/Local
	GO-2	What are your country's main problems?	Few problems	Many problems
	WR-4	Do you discuss politics with your wife?	Never	Often
			Uninformed	*Informed*
Political Information	IN-1	Who is Lyndon B. Johnson?[2]	Incorrect	Correct
	IN-2	Who is Nehru?	Incorrect	Correct
	IN-3	Who is . . . (national political figure)?	Incorrect	Correct
	IN-4	Who is . . . (national political figure)?	Incorrect	Correct
	IN-6	Where is Washington?[3]	Incorrect	Correct
	IN-7	Where is Moscow?	Incorrect	Correct
			Non-Participant	*Participant*
Participation in Civic Affairs[4]	AC-3	How many political organizations you belong to?	Few	Many
	AC-4	Ever written to government about public issues?	Never	Many times
	AC-6	Ever gotten concerned about a public issue?	Never	Many times
	CI-2	What would you do about an anti-local proposal?	Nothing	Take political action
			Traditional	*Rule Oriented*
Political Rationality	CI-13	What is important to occupy high public office?	Popularity/Family/ Tradition	Education
	CI-14	Who should the government grant a petition to?	Friend/Needy person	Man with legal right
	EF-11	What's most important for your country's future?	Help of God/Luck	Gov't plan/People's work
			Anomic	*Non-Anomic*
Political Anomie[5]	CI-10	Do politicians pay attention to ordinary people?	No	Yes
	CI-11	Who are politicians interested in serving?	Themselves	The public
	CI-12	Do you trust politicians' speeches?	No	Yes
			Alienated	*Non-Alienated*
Evaluation of Government Effectiveness	CH-8	Are opportunities for poor man to improve his economic condition	Decreasing	Remaining the same/ Increasing
	CH-9	Are opportunities for young man to get educated	Decreasing	Increasing
	CH-12	Is governmental consideration of the ordinary man's opinions[6]	Decreasing	Increasing
			Hostile	*Non-Hostile*
Group Hostility	CI-3	Do factory owners[7] do . . . ?	Harm	Good
	CI-4	Do unions[8] do . . . ?	Harm	Good
	CI-5	Do foreign capitalists[9] do . . . ?	Harm	Good

[1] For the National Identification and Allegiance scale, ID-2: "Would you sooner follow the advice of a local or national leader?" replaced CI-9 in Nigeria and CI-8 in India.

[2] In Pakistan and India, John F. Kennedy's name was substituted for Johnson's; in Nigeria, respondents were asked to identify Charles de Gaulle.

[3] In India, New York was used instead of Washington.

[4] In Argentina, AC-3 was omitted from the Participation scale.

[5] There is no Political Anomie scale for Pakistan.

[6] CH-12 for Argentina was slightly different: "Is political participation of the ordinary man decreasing or increasing?"

[7] Cultivators were asked if land owners did harm or good.

[8] In Pakistan and India, "businessmen" was substituted for "unions;" in Nigeria, the group used was "the Ibo people."

[9] In India and Pakistan, "government officers" was substituted for "foreign capitalists;" in Israel "extreme left parties" was used; and in Nigeria, the group used was the "Northern People's Congress."

children. Our emphasis thus accords with that of Lloyd Fallers, who argues that the politics characteristic of modern society tends toward egalitarianism.[17]

We scored as non-alienated those who manifested high expectation of vigorous government intervention to increase the opportunities for individual improvement. Insofar as our respondents saw little evidence of change toward increasing opportunity, we classified them as critical of the government's effectiveness (or "alienated").

Group hostility: In their analysis of the civic culture, Almond and Verba point to social trust, a sense of identity with one's fellow citizens and a feeling one can co-operate with them, as an essential ingredient for an effective democratic polity; and they note it may be

[17] Lloyd Fallers, "Equality, Modernity, and Democracy in the New States," in Clifford Geertz (ed.), *Old Societies and New States: The Quest for Modernity in Asia and Africa* (New York: Free Press, 1963), pp. 204–205.

especially lacking in the new nations.[18] Class antagonisms, and ethnic or racial cleavages are, of course, not unknown in highly developed and stable societies. Nevertheless, we may argue that a high level of personal antagonism against other groups is likely to disrupt the political process, and render difficult or even impossible the complex cooperation between the different segments of society required by the modern polity. A substantial amount of trust in the probity and goodwill of other groups is essential for the common undertaking of running a political system of the modern type. In the absence of such trust, solutions will likely be sought outside the regular channels of politics, often in ways disruptive or even destructive of the political system itself.

To measure the propensity to hostility we identified target groups such as industrialists, traders, trade-unions, the military and various ethnic minorities, and asked our interviewees whether they considered these groups to be harmful, beneficial, or neither to the interests of our respondents. The tendency to see such groups as harmful we scored as indicating a high level of inter-group hostility. Such hostility might, in turn, be considered inimical to the effective performance of one's citizenship role in a modern polity.

On the whole, the questions we asked in the political realm seemed to be understood by our respondents as well as most others we asked, and created no special strain in the interview.[19] The eight scales we developed rest on well grounded theory. Examination of the items on which they are based (see Exhibit A) indicates they have substantial face validity; and they pass a reasonably strict test with regard to coherence as scales.[20] Within the limits indicated by these statistical tests, we can say that concepts which had been fashioned for the study of political orientations in more developed countries provide a meaningful basis for measurement of the political attitudes, values, and action of the common man in underdeveloped countries.

Insofar as the scales in our set have substantial theoretical face validity, and further meet a reasonable test of cohering empirically, they provide the means for answering our second major question: How far is it true that the set of attitudes and values which, on theoretical grounds, we identified as representing the model or ideal of the participant citizen, actually exists in nature as an empirically coherent syndrome? There were, of course, various methods we could have followed in seeking an answer to this question. We chose to approach it by considering the pattern of interrelations among the scales. Guided by the theoretical considerations stated at the beginning of this article, we decided to test first the existence of the syndrome of active citizenship, excluding those scales which we considered to be judgmental or evaluative—i.e. the scales of anomie, governmental effectiveness, and hostility. Proceeding in this way did not foreclose the option of adding these evaluative scales to the first set, assuming that set itself met the test of coherence as a general measure of participant citizenship.

To ascertain the existence of a general syndrome of participant citizenship, we then explored the inter-relations, country by country, of five scales: those of allegiance, interest, information, civic participation, and rationality.[21] The relevant data are given in Table 1. There is no absolute basis for judging the coherence of a set of scales, but we interpret the facts as clear evidence that the five scale areas are indeed strongly related and indicate the existence of a general trait or syndrome of active citizenship. Certain of the scales do seem

[18] Almond and Verba, *op. cit.*, Ch. 15, esp. pp. 497–505.

[19] This refers only to the questions tapping general orientations towards political participation. Questions about which party they favored, or which candidate they supported, or what specific changes should be made in the government, were considered so delicate that we decided against even asking them in Pakistan. When they were asked in other countries, such questions did sometimes lead to refusal to answer and in other ways lessened rapport with the interviewee.

[20] Tastes and standards will, of course, vary, as to what strength of relationship between the items is required before one will accept a set of items as constituting a scale. We adopted as a rule of thumb that the average item-to-item correlations should be significant at least at the .05 level in at least four of the six countries. The most widely accepted standard of the quality of a scale is the measure of its reliability. Using the Spearman-Brown formula for the eight scales in six countries, we obtained a range of reliabilities from .600 to .906, with a mean of .707 and a median of .698.

[21] A second alternative would be to construct a summary scale based directly on the items making up the several sub-scales. This has been done, with results reported in footnote 25 below. A third alternative would be factor analysis of either the sub-scales or the original questions. This has not yet been done.

TABLE 1. INTERCORRELATIONS† OF THE 5 SUB-SCALES OF ACTIVE CITIZENSHIP: BY COUNTRY

ARGENTINA (N=820)

		Allegiance	Interest	Information	Participation	Rationality	Active Citizenship
C	Allegiance		.08*	.12**	.05	.08*	.47**
H	Interest	.10**		.27**	.17**	.11**	.61**
I	Information	.05	.33**		.23**	.24**	.70**
L	Participation	.09**	.32**	.37**		.03	.52**
E	Rationality	.10**	.13**	.33**	.14**		.48**
(N=	Active						
930)	Citizenship	.47**	.62**	.70**	.59**	.56**	

INDIA (N=1300)

		Allegiance	Interest	Information	Participation	Rationality	Active Citizenship
I							
S	Allegiance		.21**	.24**	.18**	.24**	.60**
R	Interest	.10*		.37**	.27**	.21**	.62**
A	Information	.13**	.27**		.19**	.39**	.75**
E	Participation	.15**	.28**	.25**		.15**	.53**
L	Rationality	.22**	.15**	.13**	.08*		.63**
(N=	Active						
740)	Citizenship	.59**	.61**	.61**	.58**	.56**	

NIGERIA (N=720)

		Allegiance	Interest	Information	Participation	Rationality	Active Citizenship
P							
A							
E K							
A I	Allegiance		.15**	.07*	.07*	.10**	.49**
S S	Interest	.22**		.22**	.39**	.13**	.63**
T T	Information	.15**	.29**		.27**	.27**	.64**
A	Participation	.13**	.22**	.36**		.11**	.60**
N	Rationality	.20**	.08**	.12**	.05		.55**
(N=	Active						
1000)	Citizenship	.60**	.64**	.66**	.58**	.46**	

† Correlations followed by * are significant at the .05 level; those followed by ** at the .01 level or better.

rather weakly related to the others in some countries, for example the allegiance scale in Chile, Argentina, and Nigeria. On the whole, however, there is evidence here of a strong set of inter-relations.[22] In no case is the correlation sign negative, and this pattern is maintained in all six countries without even the approxima-

[22] The statistical tests of significance used in Table 1 and at later points in this paper assume a random sample. Ours are in fact cluster samples selected according to the dictates of our research design. Within those limits we sometimes have not so much a sample as the entire eligible population. In other cases we have taken our subjects as we found them, and in yet other instances drew them at random from a defined population. Given the nature of our "samples" we consider the test of significance based on the assumption of random sampling to be a conservative one, although this might be challenged in some instances. In any event, no single statistic presented here should be considered as having major or minor weight on the sole basis of the test of significance attached to it. We have used the tests of significance in order to have some general standard which could be applied as an aid in interpreting the statistics presented. The tests of significance given should not be interpreted too literally. They should rather be taken as a rough criterion which permits only a very proximate estimate of the confidence limits of the statistics presented. Most weight should be given

tion of an exception. The coherence of the entire set of scales is further attested to by their individually high correlations with a summary scale based on the individuals' score on each of the five sub-scales identified in Table 1 as Active Citizenship.[23]

As theory indicated, there is clearly a general syndrome of individual psycho-political traits whose coherence can be established empirically. The man who is informed about politics is also more likely to identify with the national state in competition with local leaders, to take an interest in political affairs, to participate actively in civic matters, and to support the use of more rational rules impersonally applied as a basis for running governmental affairs. This set of attributes is not only theoretically consistent but now must be acknowledged to be empirically coherent as well. Such empirical coherence is generally expressed statistically by reference to a scale's "reliability." The reliability, country by country, of the summary scale based on the five sub-scales is quite respectable.[24] As an alternative we constructed the summary scale using the individual questions composing the five sub-scales as one undifferentiated pool of items. The resultant scale yielded even higher reliabilities.[25] All the evidence, therefore, supports the assertion that there definitely exists a general personal trait which may reasonably be called "active" or "participant citizenship."[26]

Having established the less problematic fact that participant citizenship is an internally consistent congeries of attitudes and values, we may turn to our second question, and inquire how far the *evaluative* and *judgmental* component of political orientations is consistently related to this activist syndrome. The evidence in Table 2 obliges us to give a negative answer. The participant citizen is not also consistently non-anomic, non-hostile, and satisfied with the performance of his government. Rather, we must say, "it depends" on the country—and no doubt on the segment of the population being studied, as well.

In India, for example, the citizenship scale is negatively correlated with that indicating a critical judgment of the government's effectiveness, but positively correlated with hostile views of other groups and bad opinions about politicians. In other countries a particular evaluative scale will be positively associated with some of the sub-scales, and negatively with others. Most serious, the pattern of association between the summary citizenship index and the evaluative scales is not the same in all the countries. Only in East Pakistan does the observed pattern support the unitary conception of the modern political man. There, those who are active citizens are also consistently more benevolent toward other groups and more satisfied with the government's performance. In India and Nigeria, on the other hand, those who score high on participant citizenship are more often anomic and hostile, and in Nigeria they are dissatisfied with the government's performance as well. In general, however, there is no consistent pattern of relationship between the evaluative scales and the measures of active citizenship.[27] The pat-

to the pattern of results across a variety of measures and across the set of six countries.

[23] The individual's score on this summary scale was the mean of his score on each of the five sub-scales. From this it follows that in the summary scale each sub-scale carries the same weight, i.e., one-fifth, regardless of the number of questions on which the sub-scale is based. But the fact that only five units are included in computing the summary score also contributes to the high correlations shown in Table 1 between the citizenship and each of the sub-scales. Those correlations are each, in part, auto-correlations. The correlations of each sub-scale to the summary scale constructed to exclude the given sub-scale would be substantially lower.

[24] Using the Spearman-Brown formula, the summary scale yielded the following reliabilities: Argentina .690; Chile .722; India .746; Israel .724; Nigeria .721; Pakistan .724.

[25] The reliabilities thus obtained ranged from .760 to .794 when computed by the Spearman-Brown formula. Using the Kuder-Richardson formula the reliabilities ranged from .693 to .735. These reliabilities are in the same range as are those for scales of comparable length dealing with political attitudes in the United States. Thurstone's Patriotism Scale, for example, yielded reliabilities ranging from .69 to .83. Some others have done better, but generally only with college student populations who are more consistent than our less well educated respondents can be expected to be. See Marvin Shaw and Jack Wright, *Scales for the Measurement of Attitudes*, New York: McGraw-Hill, Co., 1967, esp. pp. 193–229, 301–329.

[26] As indicated above, the evidence from a factor analysis is not yet available. However, it seems reasonably apparent from the pattern of inter-correlations in Table 1 that a factor analysis would confirm the existence of the basic underlying syndrome of participant citizenship.

[27] This inconsistency is not maintained when we consider only the relation of the evaluative scales to one another. In all six countries the scales of anomie, alienation, and negativism toward government effectiveness correlate quite

TABLE 2. CORRELATIONS† OF 3 EVALUATION SCALES TO THE SCALE OF ACTIVE CITIZENSHIP AND ITS SUB-SCALES

	ARGENTINA (N=820)			CHILE (N=930)		
	Anomie	Government Effectiveness	Group Hostility	Anomie	Government Effectiveness	Group Hostility
Allegiance	−.02	.02	−.03	−.17**	−.02	.02
Interest	.01	.05	−.04	−.08*	.00	.04
Information	.05	−.01	−.05	.00	−.02	−.02
Participation	−.01	−.06	−.02	−.06	.03	.05
Rationality	−.06	−.04	−.06	−.05	−.08*	−.07*
Citizenship	−.01	−.01	−.08*	−.12**	−.03	.00

	INDIA (N=1300)			ISRAEL (N=740)		
	Anomie	Government Effectiveness	Group Hostility	Anomie	Government Effectiveness	Group Hostility
Allegiance	.07**	−.05	.06*	−.04	.03	−.04
Interest	.08**	−.10**	.05	−.02	−.03	−.02
Information	.26**	−.05	.19**	.01	−.03	.08*
Participation	.03	−.08**	.02	−.07	.04	.04
Rationality	−.01	−.14**	.00	.01	−.06	−.09*
Citizenship	.15**	−.13**	.11**	−.04	−.02	−.02

	NIGERIA (N=720)			EAST PAKISTAN (N=1000)		
	Anomie	Government Effectiveness	Group Hostility	Anomie	Government Effectiveness	Group Hostility
Allegiance	−.04	−.04	−.03	Omitted	−.15**	−.03
Interest	.02	.06	.03		−.09**	−.08*
Information	.19**	.10**	−.01		−.07*	−.08**
Participation	.08*	.08*	.07		−.08*	−.07*
Rationality	.03	−.03	−.04		−.01	−.07*
Citizenship	.10**	.06	.01		−.14**	−.11**

† Correlations followed by * are significant at the .05 level; those followed by ** at the .01 level or better.

tern is unclear and the associations weak. Only an analysis which took account of the special conditions in each country could do justice to this set of findings.

How one *judges* the political process clearly depends not only on one's psychological disposition to participant citizenship, but is much influenced by the actual situation and policies of one's government. This conclusion is certainly not startling, and most may consider it obvious. It is nevertheless important to put the presumably obvious to an empirical test.[28] We must recognize, furthermore, that these facts have by no means been obvious to everyone. In discussion of the politics of developing countries one often finds that strident demands, hostility toward the government, political strikes and other forms of unrest are cited as evidence of the lack of political "maturity" or the dearth of good citizenship in the new nations. These analyses run together what our study prove to be two quite distinct elements

significantly, and appear to form a coherent "resentment" syndrome. The relation of this syndrome to the scale of participant citizenship is, however, again different in India and Nigeria from other countries, and throughout the relationship is weak.

[28] We will, later, see that some things which most everyone assumes to be obviously powerful causes of individual modernization nevertheless prove to be embarrassingly ineffective.

of the political orientation of those in developing countries. Good citizens are not necessarily happy, approving, or satisfied. In some countries the men who are informed, interested, and fully participant in the political process may also be alienated, anomic, and hostile. In some settings those manifesting such disgruntlement may be mainly expressing deep-seated personality tendencies, but in other lands they are making objective judgments perfectly consistent with their expectations as good citizens. Disgruntlement expressed in politics does not necessarily reflect a general personal tendency to anger and despair.

By contrast, it seems that the qualities of active citizenship are more general attributes, part of the individual personality, which will be manifested in much the same form regardless of the context of political action. Whether he disapproves or approves of what the government is doing, whether he feels friendly or hostile to other groups, the active citizen will keep informed, take an interest in public affairs, participate in public and civic organizations, and show preference for rational and orderly rules and regulations to govern political life. Participant citizenship is relatively "context-free," whereas the evaluative and judgmental orientations to politics cannot be understood outside of a specific setting. To understand them requires detailed contextual analysis, beyond the scope of this report.[29]

Since the five non-evaluative scales are so consistently related to each other, we will pursue the analysis of the sources of influence not separately for each element in the syndrome, but rather by using the summary scale. This composite, which we call the "active citizenship" index, has the virtue of being much broader in conception, and a more reliable measure than the individual scales. Its use greatly reduces the complexity of pursuing an analysis of five different scales through six different countries each of which is represented by several different occupational groups. Instead, each individual can be characterized by a single score, and he receives it on a measure which shows the widest possible variation from individual to individual within each national sample.[30]

III. SOCIAL FACTORS FOSTERING ACTIVE CITIZENSHIP

In our larger study of modernization, we identified and measured almost 50 different social attributes of the individual, his social position, or his social setting which theory and/or earlier research had identified as likely to contribute to making a man more "modern." These ranged from characteristics acquired at birth, such as religion and ethnicity, through those developed in youth, such as education or village residence, to those relevant mainly to the adult, such as occupation or income. Since we thought of active citizenship as a component of the larger syndrome of individual modernity, there was every reason to relate the same set of independent variables used in studying the general syndrome to the scores individuals obtained on the index of participant citizenship.

The simplest way to test the relative strength of the different modernizing influences is through the correlations between the index of active citizenship and the independent variables. The data for a variety of such variables are given in Table 3. It is apparent that many factors have a substantial effect on the citizenship scale. Of our battery of independent variables, some 45 were applicable in at least 5 of the countries. Some half of these correlated with the citizenship index at the .01 level or above in four of the countries or more; sixteen had a perfect record, correlating at the .01 level or better in 5 of 5 or 6 of 6 tests. This is an impressive performance, both in general survey work and our experience with other scales in this study.[31] It clearly establishes the index of participant citizenship as a highly sensitive

[29] In a separate set of reports on our research, we will explore these national differences in detail.

[30] Since, as noted above, the five sub-scales had each been scored from 0 to 100, the theoretical range on the summary scales was also 0 to 100. The range of the actual scores was as follows: Argentina 0–87; Chile 0–95; India 0–88; Israel 0–100; Nigeria 0–100; Pakistan 7–93. The scoring system for the sub-scales insured that each of them would produce a distribution very near the normal curve. In combining these five normal distributions, however, we could well have come out with a new distribution very far from the normal. In fact, the distribution of summary scores in each country was very close to normal. This could only have come about if the scores individuals received on any one sub-scale were in fact very similar to the scores they received on the other four scales, which further justifies our combining these five in one summary scale.

[31] We must acknowledge that not all these tests were truly independent. For example, the measures of total years in school, of performance on our literacy test, and the score on a vocabulary ("opposites") test were all highly inter-correlated and might be considered merely alternate, rather than independent, measures of intellectual capacity. Nevertheless, we feel our conclusion stands.

TABLE 3. CORRELATIONS[1] OF ACTIVE CITIZENSHIP SCALE WITH SELECTED INDEPENDENT VARIABLES IN SIX COUNTRIES

Independent Variables:	Argentina	Chile	India	Israel	Nigeria	East Pakistan
Education						
Formal Education	.46**	.42**	.69**	.37**	.42**	.42**
Opposites Test	.46**	.48**	.50**	.44**	.23**	.44**
Mass Media						
Radio Listening	.15**	.25**	.30**	.12**	.25**	.25**
Mass Media Exposure	.36**	.44**	.51**	.38**	.36**	.31**
Urbanism						
Years Urban Since 15	.29**	.33**	.06**	n.d.[2]	.26**	.15**
Urbanity of Present Residence	.29**	.13**	.17**	n.d.	.19**	.05
Work Experience						
Occupational Sub-groups	.37**	.39**	.27**	.12**	n.d.	.27**
Total Time in Factory	.23**	.32**	.19**	.20**	.24**	.22**
Objective Skill (Coder Rated)	.23**	.25**	.29**	.20**	.13**	.17**
Number of Factories Worked in	.17**	.13**	.08*	.06	.08*	.14**
Factory Environment						
Factory Modernity	.07*	−.06	n.d.	−.07	.07	.06
Factory Size	.06	−.07*	.17**	−.02	.03	−.06
Factory Benefits	.08*	−.05	.30**	.07	.23**	.09*
Standard of Living						
Consumer Goods Possessed	.30**	.30**	.38**	.19**	.35**	.29**
Income	.14**	.23**	.26**	.26**	.31**	.11**
Personal Qualities						
Age	.08*	.17**	.05	.12**	.19**	.09**
Perceived Modernity of Home-School	.05	.14**	.18**	−.09*	−.04	.05
Perceived Modernity of Factory	.02	.03	−.16**	.03	.09*	−.06
Self-Rated Skill Level	.12**	.15**	n.d.	−.03	.13**	.29**
Approximate Number of Cases[3]	820	930	1300	740	720	1000

n.d. = no data.

[1] Correlations followed by * are significant at the .05 level; those followed by ** at the .01 level or better.

[2] Variable "Years Urban Since 15" for Israel is blank because the whole sample was of urban origin and our procedure was to code "Years Urban Since 15" only for respondents of rural origin.

[3] N's given are approximate. Some of the correlation coefficients are based on sub-samples due to disqualification of parts of the sample on certain questions, e.g. "Years Urban Since 15" is coded only for respondents of rural origin.

measure having substantial cross-national relevance.

Formal education is clearly the most consistently powerful influence represented in Table 3, never dropping below a coefficient of .37. Indeed, in India, in which there was the widest educational range of all the national samples, the correlation reaches the impressive level of .686. Other measures closely reflecting formal schooling, such as the opposites test, which serves as a kind of intelligence test, perform about as well as does the measure of education. Thus, our experience in these six developing countries is consistent with that of Almond and Verba, who noted in their five-nation study that "among the demographic variables usually investigated—sex, place of residence, occupation, income, age, and so on—

none compares with the educational variable in the extent to which it seems to determine political attitudes."[32]

In our case, however, we must temper the absoluteness of Almond and Verba's statement, since a number of other variables compete quite well with education in their apparent power to influence political attitudes.[33] The most vigorous competition is offered by the measure of mass media exposure. Contact with the mass media is much related to education, especially in the case of newspaper reading, but the measure used in Table 3 shows powerful connections for radio listening alone, which is not dependent on literacy. There is good reason to assume that extensive newspaper reading and radio listening would make a man better informed, more interested, and active in civic and political affairs. But in this case we have no way of demonstrating that the direction of influence is not the reverse, so that those with strong interest in civic affairs come to more often read newspapers and listen to the radio.

The argument of reverse influence cannot be applied so effectively, however, to urban experience, especially as measured by the years of city residence enjoyed by those who grew up in the country and came to the city after the age of 15. The greater intensity of political activity in urban areas, the increased accessibility of the individual to influence attempts, the heterogeneity of the groups represented in the cities and the obvious clash of their interest all should contribute to stimulate political interest and civic involvement. Note that the effect of urban experience seems very much less in Pakistan and India than in the other countries. In Argentina, Nigeria, and Chile, those who came from the countryside entered major urban and truly metropolitan centers of large size such as Santiago, Buenos Aires, and Lagos. In India, by contrast, the "urban" centers included in our study, such as Ranchi, were in fact quite modest provincial towns. In East Pakistan the migrants came to the capital, Dacca; but it too is, in fact, little more than a large provincial town as compared to a city like Calcutta. It would appear then, that the degree to which an urban center is truly cosmopolitan can play a role in the extent of the influence it exerts in making men modern.[34]

Of special theoretical interest to our project was the influence of occupational experience as a modernizing experience. We attached particular importance to the role of the factory as a kind of practical school in modern ways of thinking and arranging things. The correlation coefficients produced by the measure of occupational type—running from cultivator to highly experienced worker—and the more precise measure of total months of factory experience, prove to be among the largest in each country.

The other attributes of the factory, however, do not produce a large or consistent effect. The size of the factory, for example, has been discussed both by Kerr and Lipset as making for discontent and worker radicalism; but it does not seem to play an important or consistent role in inducing active citizenship.[35] Factory product (not shown) also failed to exert influence. The rated modernity of the factory, to which our project assigned substantial importance, appears to play only a very modest role and not a completely consistent one. This inconsistency is, however, less marked if we consider the worker's perception of it. Those factories which were reported by their workers to offer more benefits such as sick pay, lunch rooms, or family allowances were more consistently, and in Nigeria and India, quite noticeably, likely to produce workers with higher scores on the index of active citizenship. On the whole, then, we may assign to the qualities which make a factory modern some weight in citizenship training, while cautioning that this effect will not be manifested in all places. By contrast, the sheer amount of factory experience, of whatever kind, seems the more regular and powerful influence.

The findings in Table 3 we least anticipated are those which indicate that individuals with a high standard of living are more likely to be active citizens. Evidently those who earn more, possess more goods and live in better housing

[32] *The Civic Culture*, p. 379.

[33] We realize, of course, that in itself a correlation coefficient is not proof of cause, but only of co-variation. Yet it also is evident that a man's current political attitudes could not have caused his educational level (except, of course, by affecting his recall of how much education he had had). The same case, although admittedly more vulnerable to challenge, may be made for other "objective" indices such as years of factory experience. It does not seem likely that most men enter and remain in factories because of their political attitudes.

[34] This theme is further pursued, with some surprising results, later in this paper.

[35] See Clark Kerr, *et al.*, *Industrialism and Industrial Man* (Cambridge, Mass.: Harvard University Press, 1960); S. M. Lipset, *Political Man* (New York: Doubleday & Co., 1960), esp. pp. 248–252.

(not shown) are decidedly more likely to keep informed and to participate more fully in civic affairs. It is often assumed that those most disadvantaged are likely to rush actively into the political arena seeking there to gain some advantage to offset that denied them in economic competition. Our data give no support to that theory.[36] On the contrary, they suggest that a man will not have the inclination or extra energy to take an interest in public life and participate in civic activity until he attains a reasonably decent standard of living.[37] Good citizenship is, then, a kind of luxury which can be "afforded" only by those who have secured a standard of living above the minimum required for mere survival. Of course, active citizens are no luxury, but rather a necessity for running a modern polity and the society which relies on it. The implication is clear. To leave men in a condition of poverty so extreme that they are outside politics, in effect non-citizens, is to create an apathetic mass which is not integrated in society and cannot be mobilized for the purposes of national growth and development. Furthermore, such individuals, to the extent that they feel themselves not sharing in the process of running their country and without rights or responsibilities, may constitute an unstable and even explosive force acting outside of, or against, the political system rather than within the framework established for the ordinary resolution of political differences.

To be completely candid we must weigh against this conclusion the fact that in the two countries in which we have a measure of political radicalism, namely Chile and Argentina, the feeling that the society and the economy need a "total and immediate change" is more common precisely among the groups who are good citizens and have experienced the most extensive contact with modernizing influences such as the school, the factory and the mass media.[38] The fact that the more modern men believe in basic changes, however, cannot be taken as evidence that they seek to effect these changes outside the framework of formal political action. After our study was completed, Chile had an extremely orderly election in which both the leading contenders campaigned on a platform calling for radical changes in the society. Although Argentina experienced substantial political instability after our study was completed, it was the military rather than the more advanced segments of the working class studied by us who made the revolution, overthrew the democratically elected government, and installed a junta to run the country.

The last set of variables in Table 3 which require our attention includes those which indicate the perceptual tendencies of the respondent—they reflect how he sees his world, and especially his place in it. These measures are important in enabling us to assess how far high scores on the citizenship index might be mainly a result of the tendency to take a rosy view of the world and the treatment one has received. Although there is evidence of some connection, it is not powerful, and is decidedly inconsistent. Since the citizenship scale requires passing an information test, it was unlikely that high scores could be obtained simply by "saying the right thing." Nevertheless, it is reassuring to find evidence that the active citizenship index apparently measures something more substantial than a mere "response propensity." Participant citizenship is not just talk.

IV. THE FACTORY AND OTHER VARIABLES COMPARED

Many of the variables identified by the correlation coefficients as important in stimulating active citizenship are closely related to each other. This is true not only in obvious cases such as the two different measures of standard of living presented in Table 3. An individual's

[36] Neither do numerous other studies, particularly those undertaken in the United States and Europe, which consistently show those less advantaged also to participate less in politics. See, for example, Almond and Verba, *op. cit.*, p. 176.

[37] This point should be understood as applying to active citizenship as defined in this study, and not as necesarily applying to the evaluation of the government's performance, nor to the degree of individual commitment to the existing political system.

[38] The correlation of scores on the participant citizenship scale with radicalism was .269 in Argentina and .251 in Chile. The per cent scoring "high" on the radicalism scale in Argentina rose from 7 among those with 1–3 years of schooling to 39 for those with 8 years or more. In Chile among those with less than 6 months of factory experience only 29 per cent scored high on radicalism, whereas among those who had spent 10 years or more in factories the figure rose to 46 per cent. Expressed as correlations, the association of radicalism and exposure to modernizing institutions in Argentina was, for education .247, factory experience .167, mass media .187. In Chile the correlations were, respectively, .163, .113, .181. All were significant at the .01 level or beyond.

TABLE 4. PER CENT IN NIGERIA SCORING "HIGH"[1] ON PARTICIPANT CITIZENSHIP SCALE: BY EDUCATION, OCCUPATION, ORIGIN

Occupation	Rural Origin Years Education		Urban Origin Years Education		
	1–7	8	1–7	8	8+
Cultivator	9* (96)	0 (4)	— (0)	— (0)	— (0)
New Factory Worker[2]	17 (30)	50 (12)	19 (27)	39 (39)	— (0)
Mid-Experienced Worker[2]	18 (57)	45 (49)	25 (57)	45 (103)	82 (28)
High-Experienced Worker[2]	67 (9)	76 (21)	36 (14)	71 (5)	82 (17)

[1] Respondents falling in the upper third of the distribution of scores on the participant citizenship scale are considered "high;" their actual scores, out of a possible 100, were 56 or better.

[2] New workers had less than 7 months experience; mid-experienced had 2 to 8 years; high-experienced had been workers 8 years or more.

* The figures in parentheses show the number of cases on which the percentage is computed.

standard of living, in turn, is dependent in good part on his skill level, and both skill and earnings increase with time in the factory. Variables such as total time in the factory and urban experience are also highly correlated, because most factories are in the cities, and people had to leave their villages and come to the city to obtain industrial employment.

To disentangle these influences, one must turn to some form of multi-variate analysis, usually by applying "controls" to selected variables, while examining the effect of others. Table 4, taking Nigeria as representative, presents an array of such data with simultaneous controls for education, occupation, and rural origin. Education evidently maintains its influence, with one minor exception, within the occupational and origin groups. Factory experience also maintains its influence, but less systematically. The role of rural origin, however, becomes quite ambiguous when controls for education and factory experience are applied. By this tabular method, however, it is difficult to express the stability or instability of these influences through a simple composite metric. Furthermore, even when we control for as many as three variables, others in a battery as numerous as ours may be acting independently—but hidden from view—to give us a spurious impression of the power of any variable whose uncontaminated influence we wish to assess. One way to escalate the number of controls is by using "matched" groups of respondents. In each "match" one set of men is compared with another set chosen to be in all respects like the first set except for the match variable—on which they are chosen to be as different as possible. In the matches the critical variable is more precisely defined than is usual in the ordinary cross-tabulation procedure. More important, we gain because up to eight major variables were simultaneously controlled in the matching process.[39]

Table 5 presents the correlations which result when matched groups are considered in their relation to the active citizenship scale. In the first match the industrial workers are divided into those low and high in education. The next two matches test the effects of factory experience. The match between cultivators and experienced factory workers is, perhaps, the most appropriate to put in competition with the match on the extremes of education. The cultivator-worker match, however, could be influenced by selective recruitment, with politically more "modern" farmers dispro-

[39] We also lose, however, because the matching process leaves us with very small numbers in the groups compared, and often selects a subset not truly representative of the larger group from which it is drawn. An alternative method would be multivariate regression analysis. This method, of course, presents still other peculiar difficulties. Nevertheless, we plan in a later publication to report the results of such an analysis.

TABLE 5. CORRELATIONS[1] OF SELECTED MATCHES WITH ACTIVE CITIZENSHIP SCALE: BY COUNTRY

Line	Match Content[2]	Argentina	Chile	India	Israel	Nigeria	East Pakistan
(1)	Education:[3] Low vs. high	.57** (10)	.40** (21)	.56** (52)	.39** (35)	.57** (20)	.35** (50)
(2)	Cultivator vs. Experienced worker	.34** (63)	.40** (63)	.30** (69)	n.d.	.48** (21)	.28** (176)
(3)	Factory experience: Few years vs. many	.23* (44)	.27** (71)	.19* (80)	.16 (53)	.06 (62)	.16 (58)
(4)	Years urban: Few vs. many	−.11 (24)	−.15 (14)	.06 (40)	n.d.	.08 (24)	.02 (25)
(5)	Mass media: Low vs. high exposure	.19** (143)	.24** (147)	.20** (94)	.19** (97)	.19* (90)	.13* (124)
(6)	Cultivators vs. Urban non-industrials	n.d.	.49** (24)	.11 (73)	−.18 (20)	.23* (41)	.06 (96)
(7)	Urban non-industrials vs. Experienced workers	−.12 (33)	−.16* (82)	.16 (29)	.08 (89)	.11 (36)	.11 (42)
(8)	Origin of workers: Rural vs. urban	n.d.	.15 (62)	n.d.	n.d.	.16 (53)	.29 (22)

n.d. =no data.

[1] Correlations followed by * are significant at the .05 level; those followed by ** at the .01 level or better.

[2] The number of matched pairs on which each correlation coefficient is based is shown in the parentheses. For example, in Israel, the education match variable was created by giving 35 men with low education a score of 1 and assigning a score of 2 to 35 highly educated men of similar age, origin, skill and economic level and having the same amount of factory experience in factories of comparable size and modernity. This match variable was then correlated with the scores each man received on the participant citizenship scale. The resulting correlation coefficient, .392, is significant at the .01 level, indicating that active citizenship is in fact highly related to level of education.

[3] This match applies to factory workers only.

portionately drawn to city employment.[40] We, therefore, offer a third match in which both groups are already factory workers, and are divided by having been either few or many years in industry.

It is unambiguously the case that, with most everything else important under "control," education still has a powerful residual effect on active citizenship. The high zero-order correlations between education and citizenship which we earlier observed clearly did not result mainly from the incidental intercorrelation of education with other factors. In all six countries the education match produces resoundingly high correlations, which, despite the small sample size, are consistently significant at the .01 level or better.

[40] This fear is, however, hypothetical rather than real. We have carefully matched cultivators with men of the same villages who had recently migrated but had not yet been much exposed to industrial life. The evidence is clear-cut that, at least in our six countries, the men who migrate are not self-selected on the basis of being more modern in attitude and spirit than those who remain behind.

Matches 2 and 3 show that even with education and other powerful variables controlled, factory experience also still has a substantial independent residual effect. Certainly, the relationship is neither so strong, nor so consistent, as that observed in the education matches. Nevertheless, there can be no doubt that in itself the factory is, in significant degree, a school in citizenship, inculcating values of interest and active participation in the political process.

It is naturally tempting to take the difference in the size of the correlations for the education match as against the factory experience match as expressing the relative strength of the classroom versus the factory as "schools for politics." We must, however, urge extreme caution in comparing the relative size of these coefficients, since in a match the correlations are much affected by the size of the gap separating the high vs. low set.[41] To permit

[41] The education match might, for example, place in the "low" group men with 0–3 years and in the "high" group those with 8–10 years of schooling, resulting in a substantial gap of 5 years. Few of the high and low education group

a less ambiguous comparison, we turn to another measure based on the matches but not relying on correlation coefficients. We may divide the *years* of experience in education or factory work separating high and low match groups by the number of *points* separating them on the scale of citizenship. The result indicates the number of points on the citizenship scale an individual gained on the average, for each year of additional schooling or factory work he experienced.[42]

In looking at the results, presented in Table 6, it should be kept in mind that the citizenship scale was scored so that individuals could get anything from 0 to 100. Evidently each year of additional schooling, on the average, enables a man to earn approximately 2½ more points towards a rating as a fully participant citizen. The impact of factory experience is much more modest, the gain being generally about 1¼ points per year. If the objective, therefore, is good citizenship, an investment in keeping people in school longer will apparently bring greater returns than an investment in keeping them employed in factories.

Nevertheless, it is striking that even with the effects of education and other variables rigorously controlled, the factory still has so definite, regular, and appreciable an effect. The factory evidently is a school for citizenship. It may work less efficiently, or more slowly, than does education, but it is effective in transforming attitudes, values, and behavior so as to make men more participant citizens. Since, in most cases, an adult's education is more or less terminal, the fact that factory work fosters the continued growth of citizenship values takes on added significance. It means that even in countries in which little formal education is attained by the average person, his schooling in citizenship can be continued informally through work in a factory. The marginal cost to society, furthermore, is nil. The entire gain in citizenship values, which would run 2½ per cent per year if we take 50 as a mean "starting point," is essentially a windfall profit.

TABLE 6. ANNUAL LOSS OR GAIN IN POINTS ON CITIZENSHIP SCALE: BY TYPE OF LIFE EXPERIENCE

	Annual Point Gain Per Year of:		
	Education[1]	Factory Work[2]	Urban Residence[3]
Argentina	3.6	1.2	−0.7
Chile	2.0	1.6	−0.7
India	2.5	1.4	0.4
Israel	2.5	0.7	n.d.
Nigeria	3.5	1.5	0.6
Pakistan	2.2	1.2	0.1

n.d. = no data.

[1] Calculated on the basis of average gain in two matches: project match #35, comparing low vs. high educated factory workers, and match #34 comparing low vs. high educated cultivators.

[2] Calculated on the basis of the average gain in two matches: #4, comparing cultivators with experienced factory workers, and either match #63, 64 or 65 comparing different years of factory experience.

[3] Calculated on the basis of match #12 using UNI's and factory workers grouped as having "many" or "few" years urban experience.

There remains, however, the question raised earlier as to whether it is the factory or the town which is more truly the adult "school for citizenship." The matches permit us some resolution of the debate. First, how lasting are the effects of rural as against urban origin, at least for those who later leave the countryside and go on to industrial labor? We can answer the question for three countries. In each there is evidence of some long-term residual disability attached to rural origins, but in no case is the difference significant (line 8, Table 5). "Urban experience after 15" does not seem to be any more powerful in its influence on citizenship. When industrial workers and urban but non-industrial workers (UNI's) are matched on all major variables except years of urban residence, the correlations with the citizenship scale are consistently low, and fail to attain significance in any country (line 4, Table 5). Furthermore, the two strongest associations are negative; that is, the score on the citizenship scale was ordinarily *less* for those with longer urban residence. The same impression is given by examining the points gained or lost on

would then overlap in attitude, and a high correlation would result. If the factory work match succeeded in placing men in the "high" and "low" experience categories with only a two year gap, the overlap would be great and a lower correlation might be expected. Under such circumstances this lower correlation would not be correctly interpreted as showing that factory experience is a "weaker" variable than education.

[42] For example, in the educational match in Argentina the highly educated set had, on the average, six years more schooling than the low-educated. Their mean scores on the citizenship scale were 60 and 38, respectively. The mean difference of 22 points on the scale, divided by 7 for the year's difference in education, yields a point gain of 3.6 per year.

the scale per year of urban residence. The average change per year is very small where it is positive, and in Argentina and Chile the change is in the negative direction; that is, those longer in the city score lower on citizenship (Column 3, Table 6).

Under the circumstances, it is difficult to argue that it is the stimulation of urban life rather than his experience at work which makes the factory worker more fully a participant citizen. The seemingly substantial influence of urban experience which was earlier suggested by the zero-order correlations (in Table 3) evidently came about mainly because of the inter-relation of work experience and years of urban residence.[43] When matching controlled for the amount of factory work a man had, not much predictive power remained in the measure of years of urban experience.

The larger cosmopolitan centers, moreover, may be particularly counter-productive so far as concerns the inculcation of good citizenship. Thus, the match pitting the capital, Santiago, against the provincial city of Valdivia, produced a strong negative correlation of −.358, significant at above the .01 level. The mean score on the citizenship scale was 57 to 47 in favor of Valdivia. A comparable gap separated the Nigerian group in Lagos from those residing in Ibadan and other lesser cities and towns. Although Buenos Aires nosed out provincial Córdoba with mean scores of 45 to 41, respectively, the difference was not significant. Despite what we were tempted to conclude earlier, the city does *not* seem the place to learn participant citizenship, the *big* city least of all.[44]

If urbanism is not making the factory workers more active citizens, it presumably should not be having much impact on the urban non-industrials either. Since the UNI does not enjoy the experience of factory work as a school, he should have lower scores than the industrial worker of the same level of education. Cross-tabulations did not support this conclusion.[45] The matches permit a more precise check on this point.

Comparing lines 2 and 6 of Table 5 we see that factory work turns cultivators into good citizens more powerfully and more consistently than does work as a non-industrial. Nevertheless, we observe that non-industrial work does have the apparent capacity to change the cultivator into a more modern citizen. Indeed, in two of five countries the correlation reaches significant levels. When we pit the UNI's directly against the industrial workers, the latter do better in four of six countries, but not even at the .05 level of significance in any; and they do less well in Argentina and Chile, significantly so in the latter (line 7, Table 5).

To explain these facts in any depth would require a complex and detailed exposition, including an examination of the peculiarities of the urban non-industrial sample, which was unfortunately extremely variable from country to country. Such an exploration is not possible in this context. We therefore confine ourselves to acknowledging that in some countries, with some sets of urban non-industrials, either the peculiarities of their social or cultural origins or the conditions of their employment stimulate them to participant citizenship in degree equal to or greater than that of workers in industrial organizations. Of these factors, I believe it is mainly the UNI's extensive contact with the public, and their economic activity as petty entrepreneurs, which generates the heightened citizenship qualities of the taxi drivers, barbers, waiters, kiosk operators, and others who make up these sub-samples. Industry is a fine school for citizenship, but it is apparently not the only form of employment which may act in this way.

Although the matches knocked out urbanism as an independent determinant of participant citizenship, the mass media survive this severe test. Even with education, factory experience, and other powerful variables controlled, the mass media matches in *all* countries yield correlations with the citizenship scale significant at the .05 level and above (line 5, Table 5). The match does not, of course, resolve the ambiguity as to whether exposure to the mass media inculcates citizenship values, or whether

[43] For Argentina we have available a correlation of years of urban with years of factory experience for a sub-sample of factory workers only. The figure is very high: .78. Presumably the situation is much the same in the other countries.

[44] This conclusion is supported by an unpublished independent multi-variate analysis done by David Smith using the Overall Modernity Scale as dependent variable. He concludes: "The weakest of the six independent variable clusters we have been examining is urbanity. This is a very great surprise indeed. For all six countries the average regression weight of urbanity is .01; factory experience averages .05, objective skill level .07."

[45] For example, in Nigeria, among men of rural origin and low education, 18 per cent of both the UNI and the mid-experienced industrial workers scored "high" on citizenship. In Israel among the low educated, both occupational groups had about 21 per cent "high."

those with an interest in politics look into the press and radio. No doubt the two tendencies act to reinforce each other.

V. SUMMARY

Our analysis of political orientations in the "common man" stratum of six developing countries yielded results which, on the whole, exceeded our expectations. There was reason to believe that purely local factors would loom so large as to make the pattern of attitudes in each country distinctive. We did, indeed, find that the evaluative component of political attitudes—as measured by our scales of anomie, group hostility, and alienation (or judgment of government effectiveness)—could not be fit to any pattern comparable for all six countries. We discovered, however, that another set of attitudes and behavioral tendencies, designated as the active or participant citizenship syndrome, cohered as a unified entity in much the same way in all the countries. This syndrome includes identification with or allegiance to supra-local and non-parochial public authority; interest in civic affairs; information about political figures; participation in public organizations; and adherence to rational organizational rules as a basis for running government affairs.

The syndrome is apparently inculcated as a result of certain life experiences. Of the early socialization influences, the amount of education a person enjoys seems the most powerful. Indeed, it emerges as one of the most important factors in stimulating active citizenship in competition with any other source of influence. From the distinctive point of view of our research project, however, it is notable that the experience of industrial work has considerable ability to inculcate the values and modes of behaving which go with active citizenship. Both of these determinants survived the very stringent test posed by our analysis with matched groups, although education comes through more powerfully and consistently than does factory experience. The results leave little doubt that these are both truly independent influences. Use of the mass media also qualifies as an independent force, although in this case it remains more ambiguous how far newspaper reading and radio listening are the "cause" and how far the "effect" of active citizenship. We also noted that a decent standard of living seems a necessary precondition to be met before a man will expend the extra energy required for full participation in civic life. Finally, it appears that certain personal qualities perhaps appropriately defined as personality traits, such as intelligence and a subjectively high estimate of one's skill and social standing, are also characteristic of those who manifest the syndrome of active citizenship. Unfortunately, using matches did not enable us to test how far either economic well-being or the tendency to see the world as benign are truly independent variables in their influence.

Education had previously been identified by many studies as a force making for fuller participation in the citizen role. That we confirm this finding in six developing countries, without exception, is gratifying but not startling. Our distinctive contribution, therefore, lies in establishing occupational experience, especially work in factories, as an independent source of influence. Since the general theory guiding our larger investigation into the sources of individual modernization gives special emphasis to the role of the factory as a "school for modernization,"[46] it is particularly gratifying to discover that industrial employment seems so effective in preparing men for more active participation in their role as citizens.

One might well have begun with the opposite prediction. The factory is organized hierarchically, and authority in it runs strictly along formal lines. Decisions are made not on the basis of consultation and vote, but on grounds of technical efficiency and position in the power hierarchy. Although the worker is expected to do his job as well as he can, it is a rare factory in which he is encouraged to feel a cooperative member of a larger community in whose affairs he takes an interest and in whose total efforts he is an active participant. The factory might, therefore, be seen as more likely to reinforce traditional habits and expectations in dealing with authority, rather than to stimulate a man to play a more modern citizenship role. But this conclusion assumes that the man entering the factory comes from an environment in which he already has experienced a good deal of centralized control, one in which the leader's power rests not merely on kinship or some related principle, but rather derives more formally by deputation from some higher, and presumably technically competent authority. This expectation will not be satisfied if the man comes from the typical village.

There may be a rough democracy in the traditional village, but it is also atomistically orga-

[46] See Alex Inkeles, "The Modernization of Man," *op. cit.* Almond and Verba, *op. cit.*, p. 502, also believed that "the occupational changes that accompany industrialization . . . may increase the channels of (political) socialization." They did not, however, gather any evidence from their research to bear on the question.

nized around more or less autonomous family units. This does not provide a focus for a sense of a community organized around a central authority. The factory might well, therefore, be an important way of schooling men in the acceptance of a central political authority not resting solely on the diffuse basis of kinship. The traditional small community has no general overarching goals, other than perhaps that of maintaining the peace which permits the family units to go about their own goals. The modern state, as a rule, has more definite collective goals and a national purpose. Here again the factory could well be a training ground in new values, since it has obvious goals of production, in the attainment of which every worker may see himself as a substantial contributor.

To carry this line of reasoning a step further, we may see the factory as a school in rationality. A factory runs largely according to formal rules guided by impersonal technical norms. Its system of authority and rewards is intimately geared to a more or less objective and precisely graded hierarchy of technical skill which, in turn, rests on formal education. Such an environment might well, therefore, encourage men to carry over to the political realm the expectation that high position should rest on education and skill, and that people should be treated not according to their unique personal qualities or their intimate relation with authority, but rather by rule and according to their rights.

Beyond the influence of the factory we observed, but could not adequately explain, the fact that urban but non-industrial workers are also often surprisingly high on the citizenship scale. This might be thought to result from the effect of urban life. But our most striking finding is precisely that urbanism, despite its high zero-order correlation, fails to meet the test of being an independent school for citizenship. Neither urban origins, nor the number of years of urban experience after age 15, produce significant increases in active citizenship when other variables are controlled. This is confirmed by many special matches.[47] Indeed, it appears that the larger and more cosmopolitan the city, the less the frequency of active citizenship in the common-man stratum of society. These findings invite a host of speculations we cannot pursue in any detail here.[48] Urban centers in developing countries are very often poorly organized, and may be chaotic. The rate of population growth exceeds, often swamps, the capacity of the local authorities to provide essential services, let alone any of the amenities of urban living. Housing, in particular, is scarce, and shanty-towns arise everywhere with their odd structures arranged in a disorderly jumble expressing none of the rationality of urban planning. Social control often breaks down in communities of unrelated, often totally heterogeneous, populations drawn to the city from distant corners. These and related features of rapidly growing urban centers in developing countries put them at a marked disadvantage in competition with the factory as schools in modernization. Order, rationality, firm organization, effective technical competence, justice in the distribution of rewards and punishment, even intimate personal relatedness, are often much more common in the factory than in the urban milieu which houses it in developing countries.

Finally, we may note that when a number of the more effective determinants of active citizenship combine their influence, the result is a very sharp increase in the proportion who manifest that characteristic in "high" degree.[49] That is to say, impact of the determinants is apparently *cumulative*. In a group which is engaged in agriculture, has enjoyed few of the benefits of education, and is little exposed to the mass media, the proportion who score high on citizenship participation may be practically zero. In a more favored group, better educated, working in industry, and in regular contact with the mass media, the proportion showing a high level of active citizenship may rise to the point where it is the norm for the group.

In Chile, for example, only 3 per cent of the cultivators with low education scored high on active citizenship, but among the better educated, more experienced industrial workers the proportion rose to 72 per cent. In Nigeria the spread was from 9 per cent to 82 per cent, and in the other countries the results were strictly comparable. The implications of these findings for the political future of these developing nations are substantial. Many students of politics have expressed grave concern as to whether a

[47] Only some of which have been presented in this report.

[48] The issues will be dealt with in a detailed chapter on urbanism in the general report on our project by Alex Inkeles and David H. Smith to be titled *Becoming Modern* (in preparation).

[49] We scored individuals as "high" on the scale of active citizenship if they fell in the upper third of the distribution of scale scores in their country. It is thus a highly relative designation. In less relative terms, a man had to select an alternative at the modern end of the continuum in somewhat more than half of the questions in the battery in order to score in the upper third of the distribution.

modern polity can ever hope to function effectively in these countries, given their lack of a cultural tradition which can spontaneously inculcate in young people the qualities essential to active and effective citizenship in a modern polity. Our research indicates that what is lacking in the traditional culture may be provided by the institutions of a modern society—by the school, the factory, the newspaper and the radio. These sources of influence evidently have an independent power to effect political socialization, training men to know more about politics, stimulating them to take an interest in political events and to participate in civic affairs, and fostering a shift in allegiance from tribal and local leaders to those representing a wider community of interest.

A nation's political development will be balanced to the extent that the institution of modern political forms does not outrun the growth of schools, industry, mass communications, and other social forces which act to inculcate the necessary attributes of active citizenship in the population. The balance is, however, inherently delicate. The same forces which train men in active citizenship may act to increase the demands made on the government for effectiveness, efficiency, innovation, and change. The political system may then become overloaded, or for other reasons fail to meet expectations. Alienation, anomie, and radical and extremist forms of political action may result. Then follow those perennial stalemates, coups, revolts, riots, and repressions we have come to know as endemic in so many newly developing nations.

CORRUPTION, MACHINE POLITICS, AND POLITICAL CHANGE*

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The study of political influence in the West has for the most part focused on the process by which interest groups affect the content of legislation; hence, the input process has occupied the center of attention.

Students of politics in the new states of Africa and Asia who have adopted this perspective, however, have been struck by the relative weakness both of interest structures to organize demands and of institutionalized channels through which such demands, once organized, might be communicated to decision-makers. The open clash of organized interests is often conspicuously absent during the formulation of legislation in these nations. To conclude from this, however, that the public has little or no effect on the eventual "output" of government would be completely unwarranted. Between the passage of legislation and its actual implementation lies an entirely different political arena that, in spite of its informality and particularism, has a great effect on the execution of policy.

Much of the expression of political interests in the new states has been disregarded because Western scholars, accustomed to their own politics, have been looking in the wrong place. A large portion of individual demands, and even group demands, in developing nations reach the political system, not before laws are passed, but rather at the enforcement stage. Influence before legislation is passed often takes the form of "pressure-group politics"; *influence at the enforcement stage often takes the form of "corruption" and has seldom been treated as the alternative means of interest articulation which in fact it is.*[1] The peasants who avoid their land taxes by making a smaller and illegal contribution to the disposable income of the Revenue Officer are as surely influencing the outcome of government policy as if they formed a peasant union and agitated for the reduction of land taxes. In a similar fashion, the businessmen who protect their black-market sales by buying protection from a well-placed politician are changing the outcome of policy as effectively as they might by working collectively through chambers of commerce for an end to government price controls.

Competing and complementary explanations for this prevalence of influence at the enforcement stage—usually offered as "causes of corruption"—abound in the expanding literature on this subject.[2] Among the most frequently cited factors contributing to widespread corruption in the new states are: the weak legitimacy of the formal political system compared to the persuasive ties of family or ethnicity, the relative importance of government as a source of employment and social mobility, the existence of wealthy elites denied access to direct, formal influence on policy and in the lack of strong commitments to the rule of general laws by either the elite or populace.

Implicit in each of these causes is the fundamental fact that the nature of most political

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[1] This process of influence, the groups which are likely to benefit from it most, and related issues are treated in much greater detail in my "The Analysis of Corruption Developing Nations. "*Comparative Studies in Society and History* (June, 1969). While not all corruption occurs at the enforcement stage and not all "influence at the enforcement stage" is corrupt, the empirical referents of the two terms overlap considerably. A striking exception, of course, is the legitimate arena of "regulatory politics" that largely involves contending interpretations of statutes governing private sector activity.

[2] Among others, see Ronald Wraith and Edgar Simkins, *Corruption in Developing Countries* (London, Allen and Unwin, 1963); M. McMullan, "A Theory of Corruption," *The Sociological Review* (Keele), 9 (July, 1961), 132–152; W. F. Wertheim, *East-west Parallels: Sociological Approaches to Modern Asia* (Chicago: Quadrangle Books 1965), pp. 103–131; J. S. Nye, "Corruption and Political Development," *This Review*, 61 (June, 1967), 417–427; J. David Greenstone, "Corruption and Self-interest in Kampala and Nairobi," *Comparative Studies in Society and History*, 7 (January, 1966), 199–210; Colin Leys, "What is the Problem About Corruption?," *Journal of Modern African Studies*, 3 (1965), 215–230.

demands in transitional nations is such that they are simply not amenable to the legislative process. Family-centered demands—e.g., a family's desire to secure a civil service post for its eldest son—are generally not expressible in legislative terms. When demands are made on behalf of a wider grouping, they are likely to refer to ethnic, linguistic, or regional blocs and can seldom be given legislative form.[3] The problem thus lies less with the weakness of the interest structures at the legislative stage than with the very character of loyalties in a transitional nation and the demands fostered by such loyalty patterns. Couched as it is in universalistic language, legislation is not a suitable vehicle for the expression of particularistic interests.[4] Influence at the enforcement level—whether it meets the legal definition of corruption or not—is, on the other hand, almost exclusively particularistic. It is scarcely surprising, then, that many of the narrow, parochial demands characteristic of new nations should make their weight felt during the implementation of legislation rather than during its passage. Suitable though it may be for the few groups in the modern sector organized along occupational lines, the modern legislative machinery of new nations cannot effectively cope with the host of special pleadings coming from outside the modern sector.

There is one political form that, in spite of these formal obstacles, has not only been able to respond to particularistic interests but has thrived on them. I am, of course, referring to the urban "*machine*," a form that flourished in the United States around the turn of the century. Although now virtually extinct, the machine once managed in immigrant-choked cities to fashion a cacaphony of concrete, parochial demands into a system of rule that was at once reasonably effective and legitimate. What follows is essentially an effort to outline the contours and dynamics of the "machine model" in comparative perspective, an attempt to show that the social context that fostered "machine politics" in the United States is more or less present in many of the new states. After 1) sketching the general character of "machine politics," the paper 2) suggests a developmental model to account for the machine, 3) indicates how the social context of the machine makes narrow self-interest the crucial bond of political organization, 4) examines the stabalizing or conservative effect of the machine, and 5) analyzes the reasons for the uneven development and instability of the machine in less developed nations.

[3] Malaysia, for example, is an instance where one ethnic group, the Malays, is given explicit, constitutional preference in access to certain bureaucratic posts. Similarly, the *harijan* castes in India are accorded preferential treatment with respect to education and government employment.

[4] "Pork-barrel" legislation catering to regional interests is an exception to this rule and is discussed at greater length below.

I. THE MACHINE

To abstract the basic characteristics of a political machine obviously does some violence to the great variety of entrepreneurial talent that was devoted to creating this form. Nevertheless, as all but a few beleaguered machines have succumbed to the forces of "reform," analysis has replaced accusation and the central features of most machines are reasonably clear.[5]

It will be recognized at the outset that the machine form can occur only in certain political settings. At a minimum, the setting of the machine requires:

1. the selection of political leaders through elections
2. mass (usually universal) adult suffrage
3. A relatively high degree of electoral competition over time—usually between parties, but occasionally within a dominant party

These conditions reflect the fact that since machine politics represents a distinctive way of mobilizing voters, it arises only in systems where getting out the vote is essential to gaining control of the government. While these conditions are necessary for machine style politics, they are by no means sufficient, as we shall see below.

Always applied to a political party in power, the term "machine" connotes the reliable and repetitive control it exercises within its jurisdiction. What is distinctive about the machine, however, is not so much its control as the nature of the organizational cement that makes such control feasible. The machine is not the disciplined, ideological party held together by class ties and common programs that arose in continental Europe. Neither is it typically a

[5] Some of the more successful efforts at careful description and analysis include: V. O. Key, Jr., *The Techniques of Political Graft in the United States* (Chicago: Univ. of Chicago Libraries, 1936); Seymour J. Mandelbaum, *Boss Tweed's New York* (New York: Wiley, 1965); Edward C. Banfield and James A. Wilson, *City Politics* (Cambridge: Harvard University Press, 1965).

charismatic party, depending on a belief in the almost superhuman qualities of its leader to insure internal cohesion. The machine is rather a non-ideological organization interested less in political principle than in securing and holding office for its leaders and distributing income to those who run it and work for it.[6] It relies on what it accomplishes in a concrete way for its supporters, not on what it stands for. A machine may in fact be likened to a business in which all members are stockholders and dividends are paid in accordance with what has been invested.[7]

"Patronage," "spoils," and "corruption" are inevitably associated with the urban machine as it evolved in the United States. As these terms indicate, the machine dealt almost exclusively in *particularistic, material rewards* to maintain and extend its control over its personnel. Although pork-barrel legislation provided inducements for ethnic groups as a whole, the machine did most of its favors for individuals and families. The very nature of these rewards and favors naturally meant that the machine became *specialized in organizing and allocating influence at the enforcement stage.* The corruption it fostered was not random greed but was finely organized and articulated to maximize its electoral support.

The machine is thus best characterized by the nature of the cement binding leaders and followers. Ties based on charisma, coercion, or ideology were occasionally minor chords of machine orchestration; the 'boss" might take on some heroic proportions, he might use hired toughs or the police now and again to discourage opposition, and a populist ideological aura might accompany his acts. Such bonds were, for the machine, definitely subsidiary to the concrete, particularistic rewards that represented its staple means of political coordination. It is the predominance of these reward networks—the special quality of the ties between leaders and followers—that distinguishes the machine party from the non-machine party.

The vaguely populist image of the machine party was based less on its pronouncements of general policy (which are rare) than on a myriad of acts that symbolized its accessibility, helpfulness, and desire to work for the "little man." For the rank and file, the machine boss represented a patron of those at the bottom of the social pyramid, and while the court system with its rational justice may have favored property interests, the boss typified for them an empirical justice "that works more consistently in the interests of the poor, for attention is focused upon their concrete needs and deprivations."[8] Hints of municipal corruption and graft were winked at, even applauded, by the machine clientele as the social banditry of an urban Robin Hood in spite of their long-run costs to the city. More than most types of political leaders, then, the boss' image was fashioned largely from concrete acts and only marginally from policy platforms or rhetoric.

Given its principal concern for retaining office, the machine was a responsive, informal context within which bargaining based on reciprocity relationships was facilitated. Leaders of the machine were rarely in a position to dictate because those who supported them did so on the basis of value received or anticipated. The machine for the most part accepted its electoral clients as they were and responded to their needs in a manner that would elicit their support. The pragmatic, opportunistic orientation of the machine made it a flexible institution that could accommodate new groups and leaders in highly dynamic situations.

The applicability of this basic political form to the parties of the new states has been noted by quite a few political analysts.[9] In his perceptive examination of the party states of West Africa, Aristide Zolberg emphasizes the limited authority of the dominant parties and explicitly suggests that they could be usefully described as political machines. "The party," he asserts,

> was initially a loose movement which naturally incorporated the characteristics of the society in which it grew; it was eventually transformed into a political machine but continued to reflect the state of complete integration of the territorial society.[10]

[6] Banfield and Wilson, *op. cit.*, p. 116.

[7] This analogy was made by former Liberal Party President, José Avelino of the Philippines in the *Manila Chronicle*, Jan. 18, 1949. Quoted in Virginia Baterina "A Study of Money in Elections in the Philippines." *Philippine Social Sciences and Humanities Review*, XX (March, 1955), 39–86.

[8] Murray Edelman, *The Symbolic Uses of Politics* (Urbana: University of Illinois Press, 1964), p. 118.

[9] See for example, Myron Weiner, *The Politics of Scarcity* (Chicago: University of Chicago Press, 1962), pp. 70–71; Henry L. Bretton, *The Rise and Fall of Kwame Nkrumah* (New York: Praeger, 1966)., Herbert Feith, *The Decline of Constitutional Democracy in Indonesia* (Ithaca: Cornell University Press, 1962).

[10] Aristide R. Zolberg, *Creating Political Order: The Party States of West Africa* (Chicago: Rand McNally, 1966). p. 123.

The particular "machine" qualities Zolberg has in mind include the flexible, bargaining character of the party that permits it to adjust to demands from a heterogeneous clientele on the basis of self-interest, its capacity to respond to social change through informal mechanisms, and finally its potential for stability and popularity in a context scarcely hospitable to either.

In the West African context, at least, some allowance must be made for the fact that American political machines were typically electoral machines, whereas the parties Zolberg analyzes have done their best to eliminate their dependence on winning votes. The absence of acute electoral pressures—plebiscites hardly impose equivalent pressures—considerably narrows the scope of the machine pattern; beyond a certain point the regime would then resemble an oligarchy based on coercion more than a machine relying on votes. Judging from Zolberg's analysis, that point was seldom approached in most party states of West Africa.

Describing the ruling parties in the states of India, Myron Weiner also has recourse to the machine model, not only to explain what occurs, but also to recommend the "machine" to Indian politicians as a form to emulate. Like Zolberg, Weiner focuses on the ability of the machine to reconcile competing ethnic and particularistic claims in a manner that, while corrupt, is "a small price to pay for acculturating immigrants into a democratic society."[11]

In India's state of Orissā, for example, the dominant Congress Party came more and more to resemble a machine party as it endeavored, amidst the deterioration of political bonds forged during the nationalist struggle, to win elections. F. G. Baily portrays Congress' local party leaders as machine brokers in a context where "workers and sometimes voters expect some tangible reward, not necessarily a bribe, but assistance of exactly the kind which brokers provide" (jobs, licenses, welfare payments, etc.).[12]

What all three authors have done, in effect, is to construct an analogy between the social context of political parties in Asia and Africa on the one hand, and late 19th-early 20th century urban America on the other. In the U.S., the rapid influx of new populations for whom family and ethnicity were the central identifications, when coupled with the award of important monopoly privileges (traction, electric power, and so forth) and the public payroll, provided the ideal soil for the emergence of party machines. Developing nations can be viewed as offering a social context with many of the same nutrients. New governments had in many cases only recently acquired control over the disposal of lucrative posts and privileges and they faced electorates that included many poor, newly urbanized peasants with particularistic loyalties who could be easily swayed by concrete, material incentives. The point each writer makes is not only that the machine is a suitable and relatively democratic political form that can manage such a complex environment, but that the social context typical of most new nations tends to encourage the growth of machine-like qualities in ruling parties. For America, Burnham has summarized the argument now being applied to less developed nations.

> If the social context in which a two-party system operates is extensively fragmented along regional, ethnic and other lines, its major components will tend to be overwhelmingly concerned with coalition building and internal conflict management. The need to unite for electoral [broad coalition building] purposes presupposes a corresponding need to generate consensus at whatever level consensus can be found.[13]

Given this sort of social context, so the reasoning goes, the price of effective political cooperation—at least in the short run—involves meeting narrow, particularistic demands, often through the patronage, favors, and corruption that are the hallmarks of machine politics. But why are other forms of association not feasible? What specific changes in the social context promote or undermine different styles of political collaboration? Unless the model is placed in a developmental perspective and considerably sharpened from its presently intriguing but impressionistic form, its explanatory value will remain limited.

II. THE SOCIAL CONTEXT OF POLITICAL TIES

The schema presented below focuses on changes over time in the nature of loyalty ties that form the basis of political parties. It is tailored to a bargaining—particularly, electoral—context and is less applicable where force or threats of force are the basis of coopera-

[11] Weiner, *op. cit.*, p. 71.

[12] *Politics and Social Change: Orissā in 1959* (Berkeley and Los Angeles: University of California Press, 1963), pp. 138–140.

[13] Walter Dean Burnham, "Party Systems and the Political Process," pp. 277–307, in William Nisbet Chambers and Walter Dean Burnham (eds.), *The American Party Systems: Stages of Political Development* (New York: Oxford University Press. 1967), p. 287.

tion. Nothing is intended to be rigidly deterministic about the movement from phase A to B to C. The phases are, however, largely based on the empirical experience of the U.S., England, and the new nations.

Although the phases in Table 1 have been separated for the purpose of conceptual clarity, they are likely to overlap considerably in the empirical experience of any nation. It is thus a question of which loyalty pattern is most common and which less common. Within new nations all three patterns typically co-exist; rural villagers may remain deferential to their traditional leaders, the recent urban migrants may behave more as free agents seeking jobs or cash for their votes, while a small group of professionals, trade union leaders and intellectuals may perhaps be preoccupied with ideological or class concerns. Even fully industrialized nations may contain recalcitrant, usually isolated, pockets where deference patterns have not yielded to more opportunistic modes of political expression.[17]

TABLE 1. DEVELOPMENT OF PARTY LOYALTY TIES

Phase A[14]	Political ties are determined largely by traditional patterns of deference (vertical ties) to establish authorities. Material, particularistic inducements to cooperation play a minor role except among a limited number of local power-holders.[15]
Phase B	Deference patterns have weakened considerably in a period of rapid socio-economic change. Vertical ties can only be maintained through a relationship of greater reciprocity.[16] Competition among leaders for support, coupled with the predominance of narrow, parochial loyalties, will encourage the widespread use of concrete, short-run, material inducements to secure cooperation. The greater the competitive electoral pressures, the wider the distribution of inducements is likely to be. Influence at enforcement stage is common.
Phase C	New loyalties have emerged in the process of economic growth that increasingly stress horizontal, (functional) class or occupational ties. The nature of inducements for political support are accordingly likely to stress policy concerns or ideology. Influence at the legislative stage becomes more appropriate to the nature of the new political loyalties.

Prior to fuller treatment below, a brief word is in order about the process of change implied by the model. Movement from Phase A to Phase B involves the shaking loose of traditional deference patterns which can occur in a variety of ways. For the United States, large-scale immigration by basically peasant populations was often the occasion for this change while, for less developed nations, the economic changes introduced by colonial regimes and rapid migration from village to city has provided the catalyst. The social disorganization that resulted was often exacerbated by ethnic, linguistic, or, even caste fragmentation, but similar patterns have arisen in Thailand and the Philippines, amidst comparatively homogeneous populations. Elections themselves have, of course, played a central role in this transformation in that they placed a new political resource of some significance at the disposal of even the most humble citizens.

Movement from Phase B to Phase C would appear to depend on the process of industrialization as new economic arrangements take hold and provide new foci of identification and

[14] The broad lines of this schema were suggested to me by an analysis of the use of money in elections contained in Arnold Heidenheimer, "Comparative Party Finance: Notes on Practices and Toward a Theory," pp. 790–811 in Richard Rose and Arnold Heidenheimer, eds., *Comparative Studies in Political Finance: A Symposium*, *Journal of Politics*, 25, 4 (November, 1963), especially pp. 808–809. Changes in the nature of political ties greatly influence the degree to which monetary incentives are successful in electoral campaigns, and I have thus borrowed from that analysis for the broader purpose of this paper.

[15] Traditional ties often allow some scope for bargaining and reciprocity; the ability of clients to flee to another jurisdiction and the economic and military need for a leader to attract and keep a sizable clientele provided subordinates with some leverage. The distinctions made here in the degree of reciprocity are relative, not absolute. See, for example, Herbert P. Phillips, *Thai Peasant Personality* (Berkeley: University of California Press, 1965), p. 89 or George M. Foster, "The Dyadic Contract in Tzintzunzan, II: Patron Client Relationships," *American Anthropologist*, 65: pp. 1280–1294.

[16] What appears to happen in the transitional situation is that the client is less "locked-in" to a single patron and the need for political support forces patrons to compete with one another to create larger clienteles. For a brilliant analysis of this pattern in Philippine politics see Carl H. Landé, *Leaders Factions, and Parties—The Structure of Philippine Politics*, Monograph No. 6 (New Haven: Yale University—Southeast Asia Studies, 1965), *passim*.

[17] In this context, party labels are deceptive. The existence of parties proclaiming an ideology or class position are often found in rural areas where the labels have been appropriated in toto in a continuation of traditional feuds between powerful families and their respective clienteles. The key is the nature of loyalty patterns, not the name of the organization. See Carlo Levi, *Christ Stopped at Eboli* (New York: Pocket Books, 1965).

loyalty. As the case of the United States illustrates, though, the presence of sharp ethnic and sectional cleavage—the latter reinforced by constitutional arrangements—may considerably dilute the strength of these new bonds.

The duration of Phase B, when the social context is most hospitable to machine style politics, may vary widely. When the social disorganization accompanying urbanization and economic change is particularly severe and of long duration, when it is compounded by deep cultural differences, when competitive elections with a universal suffrage are introduced early, the pressures toward machine politics will be vastly greater than when demographic change is gradual and less severe, when it occurs with a minimum of cultural cleavage, and when the electorate is restricted. The historical circumstances of both the U.S. and most new nations have been, in this sense, quite conducive to the development of machine politics as opposed, say, to the Western European experience.

Political parties must generally offer inducements of one kind or another to potential supporters. The pressures to enlist adherents is obviously greatest when the party faces a competitive electoral struggle but, in the absence of battles for votes, merely the desire to establish a broad following among the populace may create analogous pressures.

The sort of incentives most likely to "move" people is contingent, as the phase model clearly implies, on the kinds of loyalty ties that are most salient to the potential client. In the short-run, at least, parties that need supporters are more apt to respond to the incentives which motivate their clientele then to transform the nature of those incentives. Elaborating on this relationship between loyalty bonds and party inducements, the table below suggests the actual empirical patterns likely to occur.

TABLE 2. INDUCEMENTS AND THE NATURE OF LOYALTY

Nature of Loyalties	Inducement
1. Ties of traditional deference or of charisma	Mostly symbolic, non-material inducements[18]
2. Community or locality orientation (also ethnic concentration)	Indivisible rewards; public works, schools "pork-barrel" Communal Inducements
3. Individual, family, or small group orientation	Material rewards; patronage, favors, cash payments "corruption" Individual Inducements
4. Occupational or class orientation	Policy commitments tax law, subsidy programs, etc. "general legislation" Sectoral Inducements[19]

Parties in the real world commonly confront all four patterns of loyalty simultaneously and fashion a mix of inducements that corresponds to the mix of loyalties.[20] Inducements, moreover, are not unifunctional; public works usually carry with them a host of jobs and contracts that can be distributed along more particularistic lines while patronage can be wielded in such a way as to actually favor an entire community or ethnic group.

With these qualifications in mind, I am suggesting that, given pressure to gain support, a party will emphasize those inducements that are appropriate to the loyalty patterns among its clientele. Material inducements are as characteristic of occupational or class loyalties as they are of local or family loyalties; what is different is simply the scope and nature of the group being "bribed" by the party, not the fact of "bribery." In the case of occupational and class loyalties, the inducements can be offered as general legislation (and rationalized by ideology, too), whereas inducements at the individual or family level must often be supplied illegally ("corruptly") at the enforcement stage.[21] The classical machine faces a social context in which community and family orientations are most decisive. Responding to its environment, the machine is thus likely to become consumately skilled in both the political distribution of public works through pork-barrel legislation and in the dispensation of

[18] Charismatic ties naturally involve more symbolic inducements than do ties of traditional deference in which clients are generally assured a certain minimal level of material well-being (security) by their protector or patron in return for their loyalty.

[19] Term borrowed from Banfield and Wilson, *op. cit.*, p. 337.

[20] The importance of one or another pattern can, in addition, be amplified or diminished by structural characteristics of the political system; in the U. S., federalism and local candidate selection tend to amplify geographical ties. See Theodore J. Lowi, "Party, Policy, and Constitution in America," pp. 238–276 in Chambers and Nisbet, eds., *op. cit.*

[21] Political systems vary significantly in the extent to which favors and patronage can be carried out within the law. In the United States, for example, the traditional use of postmasterships, ambassadorial posts, and a number of state jobs exempt from normal civil service requirements provides a pool of party spoils denied most Indian, Malaysian, or Nigerian politicians.

jobs and favors through more informal channels.

Historically, the expansion of the suffrage together with the rupture of traditional economic and status arrangements have signalled the rise of particularistic, material inducements. In Robert Walcott's masterful portrait of electoral politics in 18th century England, this transition is vividly depicted in the contrast between the shire constituencies where traditional landholders still commanded the allegiance of a small electorate and the larger urban constituencies where elections

> were notoriously venal and turbulent. Wealthy beer-barons with hireling armies of draymen battled for the representation of Southwark: while the mass of Westminster electors were marshalled out, with considerable efficiency, to vote for candidates set up by the court.[22]

Southwark and Westminster, at the time Walcott describes them, were the exception rather than the rule, and English parliamentary politics revolved around coalitions of clique leaders, each of whom was generally accepted as the "natural" representative of his constituency. The transition, however, was underway.

A similar shift from patterns of deference to patterns of short-run material inducements is evident in contemporary Philippine politics. Like the English landed proprietor in the 18th century, the Filippino *haciendero* could, until recently, rely on his tenant laborers and peasants indebted to him to vote as he directed. Increasingly though, the economic arrangements and traditional patron-client ties that undergird this deference are eroding and the peasant now often requires cash or other special inducements.[23] Pork-barrel legislation is still of great electoral significance, but family and individual inducements (to the exclusion of broader sectoral demands) are the real currency of electoral struggles. As Landé describes them,

> Political parties in the Philippines, on the whole, find it unnecessary to make categorical choices between programs favoring . . . one or another social class. There are two reasons for this, the first being that Filippino voters allow their rulers to satisfy their needs particularistically . . . The second reason . . . is that most Filippino voters are not much disturbed by measures that go against the collective interests of their class or category for they have learned to expect that, as individuals, they may escape the effects of these laws.[24]

Furthermore,

> The ordinary voter also learns that what he does can have a direct effect upon certain substantive "outputs" of government. He knows that, rather than sell his vote for cash, he can trade it for the promise of a public works job, free medical care in a government hospital, protection against harassment by a local policeman, or exemption from the payment of taxes.[25]

As elsewhere, the decline of deference in the Philippines has encouraged the growth of machine style politics in which a mixture of public works and, above all, more particularistic rewards provide the fuel. The necessary incentives, as the description indicates, are generally arranged by influence at the enforcement stage—reflected in the widespread corruption for which the Philippines is noted. Philippine experience, in this regard, is reminiscent of changes in the conduct of American politics which reached decisive proportions by the mid-19th century. Till then, more oligarchic patterns prevailed which "depended on habits of deference or subordination on the part of voters toward established notables in local communities, who were recognized as natural leaders."[26] Only after such "habits of deference" had receded in the face of economic change and immigration could the machine style of particularistic, material rewards begin to thrive on a large scale.

Changes in modal patterns of loyalty help account for not only the development of machine politics but for its decline as well. In addition to other factors (which are discussed below), the growth of political ties in which family bonds were less important than before and in which occupational and/or class considerations played a more prominent role undercut the very foundation of the machines.[27] The

[22] Robert Walcott, Jr., *English Politics in the Early Eighteenth Century* (Cambridge: Harvard University Press, 1956), p. 13. The coincidence between the patterns Walcott describes and contemporary Philippine politics is discussed by Carl Landé, *op.cit.*, pp. 101–107.

[23] David Wurfel, "The Philippines," pp. 757–773 in Rose and Heidenheimer (eds.), *op. cit.*, p. 771

[24] Landé, *op. cit.*, p. 43.

[25] *Ibid.*, p. 115.

[26] William Nisbet Chambers, "Party Development and the American Mainstream," pp. 3–32, in Chambers and Burnham (eds.), *op. cit.*, p. 5.

[27] Family loyalties are always of significance but in the typical machine case narrow family ties become a central factor in the evaluation of

specific inducements which the machine was organized to supply worked their "magic" on a smaller and smaller proportion of party workers and supporters. Instead, as businessmen and laborers each came to appreciate their broader, longer run interest as a sector of society, they increasingly required general legislation that met their interests in return for political support. Here and there a social context tailored to the machine style remained, but the machine either reconciled itself to the new loyalties—becoming less and less a machine in the process—or was the electoral victim of social change. Parties still continued to offer palpable inducements to voters but the new inducements were more typically embodied in general legislation, where previously they had been particularistic and often outside the law. As Banfield and Wilson summarize the transition,

> If in the old days specific material inducements were illegally given as bribes to favored individuals, now much bigger ones are legally given to a different class of favored individuals, and, in addition, general inducements are proferred in packages to every large group in the electorate and to tiny but intensely moved minorities as well.[28]

III. THE ECOLOGY OF MACHINE COORDINATION

Historically the distinctive style of political coordination embodied in the machine has occurred in settings where, in addition to rapid social change and a competitive electoral system, a) political power was fragmented, b) ethnic cleavage and/or social disorganization were widespread, and c) most of the population was poor. Drawn mostly from studies of urban machines in the United States, these features of the environment seem applicable to a large degree to the many underdeveloped nations in which political parties have resembled machines.

a) *The Fragmentation of Power*: In accounting for corruption and machine politics in Chicago, Merriam lays particular stress on the multiplicity of urban authorities and jurisdictions that existed within the city. The eight main "governments," each with different powers, that he describes, created so many jealously guarded centers of power that a mayor faced a host of potential veto groups, any one of which could paralyze him.[29] He could secure cooperation with these authorities only by striking informal bargains—often involving patronage, contracts, franchises—and thus putting together the necessary power piece by piece.

Power was fragmented in yet another sense. Party candidates did not face one electorate but several; each ethnic group had its own special interests and demands and a successful campaign depended on assembling a temporary coalition on the basis of inducements suited to each group. The decentralization of power created by such a heterogeneous environment, meant that the "boss' " control was forever tenuous. His temporary authority rested on his continuing capacity to keep rewards flowing at the acceptable rate.

New York in the era of Boss Tweed strikingly resembles Merriam's picture of Chicago. Inspite of the prodigious manipulations attributed to him, Tweed was not especially powerful and had little control over party branches that could nominate their own candidates for many posts. What he did manage to do, however, was to create, for a time, a centralized, finely articulated coalition. Carefully assessing the nature of Tweed's feat, Seymour Mandelbaum declares:

> There was only one way New York could be "bossed" in the 1960's. The lines of communication were too narrow, the patterns of deference too weak to support freely acknowledged and stable leadership. Only a universal payment of benefits—a giant pay-off—could pull the city together in a common effort. The only treasury big enough to support coordination was the public till.[30]

Many leaders of developing nations might well sympathize with Tweed's difficulties. They also face a highly differentiated populace—divided not only along ethnic, religious, linguistic, or regional lines but also representing varying stages of incorporation into the modern sector

government action. Occupational, much less broad civic, sentiments play a marginal or even negligible role. Most immigrants to the U.S., for example, at first "took for granted that the political life of the individual would arise out of family needs. ... ": Richard Hofstadter, *The Age of Reform* (New York: Vintage Books, 1955), p. 9.

[28] Banfield and Wilson, *op. cit.*, p. 340.

[29] Charles E. Merriam, *Chicago: A More Intimate View of Urban Politics* (New York: Macmillan, 1929), pp. 68, 90. Merriam's analysis is especially valuable as he was simultaneously political scientist and politician throughout the period he describes.

[30] Seymour J. Mandelbaum, *op. cit.*, p. 58. See also, Edward J. Flynn, *You're the Boss* (New York: Viking Press, 1947), p. 21, for a 20th century account of New York City politics in which a similar argument is made.

and varying degrees of loyalty or hostility in the nation state. Except in those instances where physical coercion is preferred and sufficient to the task, rulers in the new states must reach some accommodation with enough of these interests to govern. Electoral struggles may vastly increase the pressure to reach an accommodation—for competitive elections enhance the value of popular support—but such pressures are in a sense endemic where state authority is weak. More often than not, the price of rule involves paying off each of a variety of interests in their own (usually particularistic) coin. The system of coordination thus comes to resemble machine politics.

b) *Social Fragmentation and Disorganization*: The immigrants who constituted the bulk of the clientele of the American urban machine came largely from the European peasantry. As such, they "required the most extensive acculturation simply to come to terms with urban-industrial existence as such, much less to enter the party system as relatively independent actors."[31] If the fragmentation of power made it advantageous for the politician to offer special inducements for support, the situation of the immigrant made him eager to respond to blandishments that corresponded with his most immediate needs. Machine inducements are thus particularly compelling among disoriented new arrivals who value greatly the quick helping hand extended to them by the party.

The dependence of machine parties on a clientele that is both unfamiliar with the contours of the political system and economically on the defensive, is underscored by the character of the small pockets where vestiges of once powerful machines still exist. One such example is the Dawson machine (really a submachine) in Chicago. This machine rests squarely on favors and patronage among the Negro population, most of which has come to Chicago from the rural south within the last generation. Deprived of even this steadily diminishing social base, the machine has elsewhere withered as the populations it assisted became acculturated and could afford the luxury of wider loyalties and longer range political goals.

It is no coincidence then, that machines florished during the period of most rapid urban growth in the U.S., when the sense of community was especially weak, and when social fragmentation made particularistic ties virtually the only feasible means of cooperation. The machine bound its clientele to it by virtue of the employment, legal services, economic relief, and so forth it supplied for them.

> For the lower strata, in return for their votes, it provided a considerable measure of primitive welfare functions, personalized help for individuals caught up in the toils of the law, and political socialization.[32]

With few qualifications, the social context that nourished machines in America matches the conditions in new nations. Rapid urban migration of rural peasantries, especially since W.W. II, when coupled with ethnic fragmentation, economic insecurity and a basic unfamiliarity with the Western governmental forms adopted by most new states, have conspired to create an analogous social context. As in the U.S. at the turn of the century, a large clientele is available that will respond enthusiastically to short-run material incentives and to the party that provides them.

c) *Poverty*: Perhaps the most fundamental quality shared by the mass clientele of machines is poverty. Machines characteristically rely on suffrage of the poor and, naturally, prosper best when the poor are many and the middle-class few. In America, Banfield and Wilson emphasized that

> Almost without exception, the lower the average income and the fewer the average years of schooling in a ward, the more dependable the ward's allegiance to the machine.[33]

Poverty shortens a man's time horizon and maximizes the effectiveness of short-run material inducements. Quite rationally he is willing to accept a job, cash, or simply the promise of assistance when he needs it, in return for his vote and that of his family. Attachments to policy goals or to an ideology imply something of a future orientation as well as wide loyalties, while poverty discounts future gains and focuses unavoidably on the here and now.

The attitudes associated with poverty that facilitate machine style politics are not just confined to a few urban centers in less developed nations, but typify portions of the rural population as well.[34] In such circumstances, the jobs, money, and other favors at the disposal of the government represent com-

[31] Burnham, *op. cit.*, p. 286.

[32] *Ibid.* Merriam calls the precinct worker "something of a social worker not recognized by the profession," *op. cit.*, p. 173.

[33] Banfield and Wilson, *op. cit.*, p. 118.

[34] For a more extended discussion of these attitudes and their origin, see my *Political Ideology in Malaysia* (New Haven: Yale University Press, 1968), Chapter 6.

pelling inducements. Deployed to best advantage, these incentives are formidable weapons in building coalitions and/or electioneering. The ease with which votes are bought—individually in many urban areas, and in blocs where village or ethnic cohesion is sufficient to secure collaboration—during elections in the new nations is a measure of the power of narrow material rewards in the social context of poverty.[35]

IV. SELF-INTEREST AS POLITICAL CEMENT: THE PATRONAGE

A popular regime is likely to reflect more of the social context in which it arises than a regime that rests primarily on coercion. The quest for broad popular support—whether because of electoral competition or, say, because of the desire to build a following wide enough to forestall potential coups—usually entails being responsive to the kinds of political demands coming from the society. Forging a wide coalition of interests in social contexts that resemble the features of the U.S. at the turn of the century or most new nations today, inevitably means developing a capacity to distribute short-run material incentives among potential clienteles. Few, if any, durable political bonds except that of material self-interest are available to build a large political party among poor, heterogeneous, transitional populations. Self-interest thus provides the necessary political cement when neither a traditional governing elite nor a ruling group based on ideological or class interest is available.

Small wonder, then, that, faced with the environment we have outlined, and with a steady deterioration of the symbolic bonds fashioned during the struggle for independence, parties in the new states have frequently taken on many machine-like traits. An overwhelming concern with the particularistic distribution of rewards to supporters so as to maintain the patchwork of interests uneasily contained within the ruling party and the decline of broader policy goals are the hallmarks of this transformation. The metamorphosis has been most striking in those cases where electoral anxieties have been most intense; e.g., the Philippines,[36] Ceylon, and, increasingly, India. In nations where a single party faces little competition but has not totally dismantled electoral procedures, machine features are often in evidence, too.[37] Only where the military has taken command has the distributive process been confined to such narrow proportions as to resemble more an oligarchy, or occasionally a dictatorship, than a machine.

Among the specific instruments of coordination available to the machine, the power of patronage is perhaps the most celebrated. The practice of patronage is by no means, however, confined to machine parties—nor is political graft on franchises, contracts, or licenses, for that matter. The court in 18th century England based its ability to create a viable parliamentary coalition on the distribution of public offices to the right people at the right time.[38] Nevertheless, the necessity of generating broad support on a continuing basis has typically meant that machine parties wield the patronage on a scale that is distinctive.

By exploiting the public purse to provide posts that may be dealt out according to political criteria, the machine party gains a staple means of maintaining internal discipline and cohesion. The diverse groups and individuals comprising the party are linked together by such material rewards as patronage, while these posts supply the party with a cadre of political workers who are constantly available to the organization and who will be responsive to commands from the leadership. It is precisely the diversity of groups within the party that renders this pool of available jobs an indispensible basis of organization. The effectiveness of patronage as a means of coordination hinges, of course, on the kind of clientele I have described—a clinetele for whom jobs are of central importance.

The political distribution of patronage was perfected to the point of fine art by the American urban machine. In the heyday of machines the number of posts dispensed by patronage reached prodigious levels. Around the turn of

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[35] Wurfel, for example, claims that 10–20% of Filippino voters *regularly* sell their votes. *Op. cit.*, p. 763. The differences between urban- and rural-based machines in the U.S. and less developed nations is an important subject that I hope to treat in a later article.

[36] Filippino parties resembled machines well before independence due to the powers they exercised within the colonial system and a pattern of early electoral competition.

[37] For single party states, the significance of material incentives has appeared to grow as the leader of the independence movement passed from the scene or as the charisma generated in that period diminished. What had been movements *par excellence* gradually became machine parties. Communist states in underdeveloped areas are, of course, exceptions.

[38] Walcott, *op. cit.*, p. 36.

the century, according to Lincoln Steffens, the Martin machine in Philadelphia had placed 15,000 people in office and "each of these 15,000 persons was selected for office because he could deliver votes, either by organizations, by parties, or by familes."[39] In New York City in the 1930's, the total annual pay for posts exempt from civil service regulations—that is, political jobs—exceeded seven million dollars even after some reforms had been enacted.[40]

While the bastion of patronage in the U.S. has remained at the state and local levels, federal jobs have historically served to knit together national parties as well. The most striking example of this may well be Lincoln's victorious Republican Party of 1860—a hodgepodge of ethnic groups, old Whigs, old Democrats, rural and urban interests, family-based factions, and so forth. As Carman and Luthin describe it, the Republicans

> had come together in the recent campaign like Highland clans to battle the common foe, the leaders of these various factions were still jealous of one another and often openly hostile. Lincoln realized that public jobs wisely distributed were the cement he must use to hold the Republican party together.[41]

The need for coordination by patronage resulted in "the most sweeping removal of federal office holders up to that time in American history,"[42] Occupants of roughly twelve hundred of the fifteen hundred posts covered were replaced with the party faithful. As it developed, the distribution of federal jobs not only cemented the party but served to cement a part of the republic itself by keeping some states within the fold during the Civil War. Carman and Luthin conclude that the liberal use of the patronage was largely responsible for containing secessionist pressures in Kentucky, Missouri, Maryland, and Delaware. Later, during the campaign of 1864, Lincoln also skillfully used the patronage in each state to help ensure his renomination and election. The fact that the distribution of jobs proved such a powerful political factor is due as much to the importance of federal employment in the minds of party workers as to Lincoln's talent for deploying it.

Given the heterogeneous composition of parties in new nations, the nature of their clientele, the scarcity value of government employment in poor nations, the patronage is, if anything, an even more significant adhesive agent than it was in the U.S. The "bandwagon" effect experienced by many ruling parties after independence was partly due to a popular recognition that the party would control the disposal of tangible rewards for some time to come and, conversely, the "negative bandwagon" effect whereby parties that failed to capture at least a local power base often disintegrated, was related to the absence of material rewards to bind their initial following.

Wielding the patronage in the new nations however, occurs in a markedly different legal context than it did in much of urban America. Saddled with the "very latest" in terms of civil service regulations, politicians in the new nations must regularly resort to practices that are either highly questionable or transparently illegal to find jobs for some of the party workers. In India, the political career of Partap Singh Kairon, Chief Minister of the Punjab, was cut short by a public inquiry that cited him for relatively minor financial peculation and for using his influence to persuade the state Public Service Commission to exclude a few posts from normal civil service requirements.[43] Deprived of open access to the spoils that built strong parties in America or 19th century England, Kairon was indicted for behavior that would scarcely have appeared unseemly in Boss Tweed's day. Congress Party politicians, despite the occasional risks, have increasingly made use of the available patronage (not to mention licenses, contracts, franchises) to maintain their electoral strength, secure party cohesion, and even to help contain mounting secessionist pressures.[44] Especially at the state

[39] Lincoln Steffens, *The Shame of the Cities.* (New York: Hill and Wang, 1963), p. 147. This was the case despite the fact that the Philadelphia machine made use of outright elector fraud as well.

[40] J. K. Pollock, "The Cost of the Patronage System," *The Annals,* 189 (1937, p. 29, quoted in V. O. Key, Jr., *Politics, Parties, and Pressure Groups* (New York: Crowell, 1966), p. 353.

[41] Harry J. Carman and Reinhard H. Luthin, *Lincoln and the Patronage* (New York: Columbia University Press, 1943), p. 10. For an account of patronage in the first few decades after independence, see Sidney H. Aronson, *Status and Kinship in the Higher Civil Service: Standards of Selection in the Administrations of John Adams, Thomas Jefferson, and Andrew Jackson* (Cambridge: Harvard University Press, 1964).

[42] *Ibid.*, p. 331.

[43] *Report of the Commission of Inquiry into the Matter of Pratap Singh Kairon* (Das Commission) (New Delhi: Government Printer, June 11, 1964), pp. 222, 224, 235.

[44] See, for example, the introduction to the "Santhanam Report," *Report of the Committee*

level, the Congress Party has come to resemble a classical political machine.

A wide variety of economic measures in new nations merit close analysis for their patronage functions. In this respect, programs of nationalization, while they may answer economic and patriotic needs too, are frequently tailored to maximize the number of patronage posts available to the ruling party. Indonesia prior to 1957 provides a striking example of these mixed motives. When a measure permitting only Indonesian firms to handle sea cargo was implemented, each of the major parties then in the cabinet established their own warehousing firms and simply assumed control of the existing facilities and business.[45] These favored enterprises—although formally within the private sector—supplied a source of both income and employment for the ruling parties.[46] Discussing the economic policy of the three cabinets from 1953–57 Herbert Feith concludes,

> On the whole, their measures of Indonesianization failed to bring about any increase in the *power* of Indonesian nationals within the economy, a fact which suggests that the patronage function of these measures may have been more important than their policy aspect.[47]

Analogous cases from other new nations could be cited at some length, but the Indonesian example is sufficiently illustrative of the general pattern. Patronage pressures are present in military regimes in new nations too,[48] but in party dominated regimes these tendencies arise not only from kinship ties and the desire for informal employment fees, but from the need to reward party workers and to maintain or broaden support.[49] The military clique need only secure the acquiescence of a relatively few powerful groups; the party characteristically faces patronage demands of greater scope since it must cultivate a large popular following.

The party's need to put together a broad coalition is often reflected in the structure and operation of development programs as well. Even in Italy, where the ruling Christian Democratic Party depends on votes from the rural south, LaPalombara has written that the Fund for the Development of the South "has become a gigantic *patronage organization* which employs people and awards developmental contracts strictly on the basis of political considerations."[50] Quite apart from its contribution to economic growth in the south, then, the Fund is managed so as to enhance Christian Democratic electoral strength in the "underdeveloped" south.

Rural development programs in many new nations are designed with this duality of function in mind. To mention only one other instance, the activities of Malaysia's Ministry of Rural Development are keyed closely to the electoral requirements of the ruling Alliance Party. The employment, "pork-barrel" funds, and contracts engendered by its programs are carefully distributed among rural Malays who are deemed assets to the party, and the few areas of solid opposition strength naturally receive little or nothing.[51] If anything, the political component of these programs has contributed to their economic success.

The Malaysian and Italian cases each exemplify the effects of machine pressures on development schemes. In fact, where parties in the new nations must build wide support (generally for elections) one would expect to

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on Prevention of Corruption (New Delhi: Ministry of Home Affairs, 1964); Ronald Segal, *The Crisis of India* (London: Johnathan Cape, 1965), Ch. 6; and Surendranath Dwivedy and G. S. Bhargava, *Political Corruption in India* (Delhi: Popular Book Services, 1967).

[45] Herbert Feith, *The Decline of Constitutional Democracy in Indonesia* (Ithaca: Cornell University Press, 1962), pp. 478–479. See also pp. 366–384 for a more general discussion.

[46] The multi-party setting of Indonesia at this time resembles less a strong machine party in power than a situation where two or more potential machine parties are vying for power. While a solid machine often looks to its long run interest and limits patronage and graft to moderate levels, the situation where potential machines vie for power is inevitably more hectic and less restrained. In Indonesia, the smaller parties, having few long-term concerns, were the most ravenous.

[47] Herbert Feith, *op. cit.*, p. 557, emphasis in original.

[48] See, for example, Fred W. Riggs, Thailand, *The Modernization of a Bureaucratic Polity* (Honolulu: East-West Center Press, 1967), pp. 270–271.

[49] The narrower scope of patronage distribution in military regimes does not preclude its overall volume being greater. If military leaders need not generate broad support in order to rule, neither need they be as concerned about the public reaction at the polls to inordinate official misconduct.

[50] Joseph LaPalombara, *Interest Groups in Italian Politics* (Princeton: University Press, 1964), p. 344, emphasis mine.

[51] For an example of how the most traditional areas are able to resist machine blandishments, see Manning Nash, "Tradition and Tension in Kelantan," *Journal of Asian and African Studies*, I (June, 1966), 310–314.

find greater emphasis placed on broad rural development programs—programs with a capacity to distribute incentives throughout much of the population—than where wide support is of only marginal concern. To some extent, these programs are the functional equivalent of the services American urban machines performed for first generation voters in the cities.

Patronage is but one of the many material incentives that parties aspiring to gain wide support among a "transitional" population must resort to. Graft for party funds, help with the law, and selective non-enforcement are, like patronage, part of the bundle of short-run inducements involving "corrupt" influence at the enforcement stage that serve as the organizational cement for the typical machine party. Where machine pressures are evident in new nations, the development programs pursued are often more amenable to the kind of political analysis indicated here than to an analysis that focuses merely on the economic or nationalistic requirements customarily used to justify such ventures. The choice among development projects and the manner of their execution may depend, for the ruling party, less on criteria of growth or productivity than on the level and quality of inducements they place at the party's disposal. If economic success and political payoffs coincide, so much the better, but a project judged a failure by economic planners may nonetheless have achieved precisely the political effects expected of it.[52]

V. THE MACHINE AS A CONSERVATIVE RESPONSE TO CHANGE

Thriving in periods of rapid social change, machine politics nevertheless represents a conservative response to its dynamic environment.[53] Machine politics is conservative in at least these four respects: a) it represents an alternative to violence in managing conflict, b) it increases the legitimacy of the regime for transitional populations, c) it emphasizes short-run gains at the expense of encouraging long-run transformations, d) it avoids class issues and fosters inter-class collaboration.

It is crucial to specify here that these effects, while they may flow in part from corrupt practices, are attributable to machine politics and not to corruption *per se*.[54] The central fact about a political "machine" is that it aims at the "political consolidation of the beneficiaries" of the patronage and graft system.[55] Whereas non-machine corruption often has a random and sporadic character or aims only at the consolidation of narrow elites who control wealth or armed force, the machine must remain popular to survive and must consequently meet the demands of a broad stratum. Not all corruption is machine politics and not all machine politics is corrupt.

Avoiding Violence: The social setting of the machine is ordinarily one where ties to the community as a whole are weak and where the potential for violence is great. The capacity of the machine to organize and provide material inducements (often corruptly) operates as a means of solving, for the time being at least, conflicts of interest that might otherwise generate violence. In order to remain popular, the machine must continually make a place for new, ambitious leaders who could potentially threaten its control. By coopting new leaders the machine can be responsive to new demands while simultaneously protecting its own future.[56]

The 1946 presidential election in the Philippines is a striking example of the use of machine inducements to defuse temporarily an economic situation highly charged with a potential for violence. Faced with a "rising tide of peasant labor discontent," the main sugar, banking, and commercial interests contributed enor-

[52] Road-building in Malaysia illustrates this pattern. The present road system is adequate for some time to come and planners have urged a lower priority for this activity. The almost unique capacity of a large road construction program to reach many rural areas with pork-barrel and patronage benefits, however, has led the ruling party to continue to pave its way throughout rural Malaysia at much the same rate as in the past. Similar calculations lie behind road programs in other new nations—and in portions of the U.S. as well.

[53] Some readers may object to the use of the term "conservative" to cover the effects described. If so, each of the separate effects may be considered separately as the term itself is not central to the argument.

[54] Both Joseph Nye, *op. cit.*, and David H. Bayley, "The Effects of Corruption in a Developing Nation," *Western Political Quarterly*, XIX (December, 1966), 719–732 appear occasionally to treat much corruption as if it were "machine corruption" without specifying the distinction.

[55] Key, *The Techniques of Political Graft in the United States, op. cit.* p. 394.

[56] For an imaginative effort to deal quantitatively with the Congress Party's cooptation of former opponents in Orissā as its majority was threatened, see Baily, *op. cit.*, Ch. 9. The process of cooptation of conservative leaders changed the social base of the party and forced abandonment of much of its original legislative program.

mous funds to the Liberals, whose electoral campaign virtually smothered the revolutionary fires beneath a soft carpet of green.[57]

The Philippine case illustrates the limits of machine conciliation as well. Depending as they do on particularistic, material incentives, machines are only as effective as their inducements. For either the modern sector where broader loyalties and civic sentiments have taken root, or the traditional sector where deference and symbolic goals are common, machine blandishments are likely to fall on barren soil. Machines can, thus, manage conflict best among "transitional" populations and may be unable to alleviate strife—or may actually exacerbate it—in other social contexts.

Creating Legitimacy: Urban American machines have long been credited with wedding the immigrant to the political system by protecting him, meeting his immediate needs, and offering *personal* (particularistic) service. If the new arrival was to be tied to the political system at all, it would have to be by personal, material inducements, and the machine responded since each immigrant represented a vote which it needed. The effectiveness of machine inducements during the Boss Tweed era in New York was underscored by what happened in the short reform period that followed his rule. Within one year, Mandelbaum states, reform mayor Havemeyer "did not have the support of a single mass based political organization."[58] Reform governments, determined to avoid the corrupt practices of the machine, soon discovered that the cost of "clean" government was a marked loss of support.

In India, where ethnic groups are geographically concentrated and thus compound the problem of alienation with that of secession, Weiner claims that only machine style bargaining and benefits can hold the state together.[59] The Congress Party has, in fact, increasingly distributed both particularistic rewards and pork-barrel projects whenever calls for regional autonomy reached threatening proportions, but the success of such efforts has been spotty. Ironically, the desire for regional autonomy was, in part, prompted by a recognition that more civil service posts, more scholarships, and so forth would be available to the local party if regional autonomy were granted than if it were not.

The latent function of building legitimacy served by machine politics is subject to the same qualification that was made for its capacity to settle conflict peacefully. Machine practices may engender support among those for whom material incentives are effective, but may, on the other hand, increase the alienation of the new middle class, military officers, students, and the very traditional. Once again, the *impact of the inducement hinges on the social context.* The support generated by machine rewards is, moreover, based rather tenuously on the continuing distributive capacity of the regime. Lacking either ideological or charismatic foundations, the regime may find its support has evaporated once it can no longer deliver the concrete inducements that serve as the party's social adhesive.

Short-run Goals: The machine must, in a sense, buy its popularity. To the extent that it faces competition, the cost of popularity is raised and the public till may not be sufficient to the demands it must meet. The effect of this "squeeze" in urban America has often forced the machine party not only to raise the city's debt[60] but also to rely increasingly on assistance from businesses with interests in the city. This latter strategy was not without its penalty as licenses and franchises were given to traction and power interests for negligible amounts, as the city accepted sub-standard equipment and materials, and so forth. *Frequently, a three-cornered relationship developed in which the machine politician could be viewed as a broker who, in return for financial assistance from wealthy elites, promoted their policy interests when in office, while passing along a portion of the gain to a particularistic electorate from whom he "rented" his authority.* The substantial long-run costs to the community as a whole were seldom appreciated since the machine controlled an electorate with little sense of community interests and a preference for immediate, personal inducements.

Machines in the new nations have often been impelled to follow a similar course. Although the private sector was occasionally a less significant resource base than in urban America, the machine frequently developed close and occasionally secretive ties with wealthy elites—especially in Southeast Asia and East Africa where minority groups dominate the private sector. Meeting reciprocal obligations to these groups and distributing material incentives for popular support has made the pursuit of longer-run development objectives more dif-

[57] Virginia F. Baterina, *op. cit.*, p. 81.

[58] Seymour Mandelbaum, *op. cit.*, p. 106.

[59] Myron Weiner, *op. cit.*, pp. 71–72.

[60] Boss Tweed, in four years, raised New York City's indebtedness by a multiple of three while leaving both the tax rate and assessments untouched. Mandelbaum, *op. cit.*, p. 77.

ficult.[61] Elsewhere, a dangerous running down of foreign exchange reserves or steeply inflationary deficits have provided fuel for the machine[62]—characteristically followed by a military government presiding over an austerity program.

d) *Non-class Focus:* The fact that the machine stresses family and parochial loyalties to the virtual exclusion of ideology and class politics requires little elaboration, as it is embedded in the definition of a machine.

So long as the public's interest continues to be centered on the scramble for particular benefits for individuals, overt rivalry will tend to be intra-class rather then interclass.[63]

While older elites have typically looked askance at the possibilities for personal mobility provided to those of humble origins by the machine, class issues of the collective nature beyond vague populism have been rare. Machines, by the nature of rewards they offer and personal ties they build into their organization, may well impede the growth of the class and occupational bonds implied by economic change and thus prolong the period during which family and/or ethnic ties are decisive.

VI. THE FAILURE OF THE MACHINE IN NEW NATIONS

Looking at politics in the new nations a scant five years ago, the machine model would have seemed an increasingly practical tool of analysis. The symbolic ties of the nationalist struggle were steadily losing their strength and yet electoral procedures were still enough in evidence to reinforce the efforts of ruling parties to remain genuinely popular. Politicization of the colonial bureaucracy was often underway and many parties were becoming adept at building support by distributing patronage and pork-barrel projects. In spite of these harbingers of machine development, relatively few machine parties actually materialized and those that did were generally short-lived.[64] The task, therefore, is to explain why machines failed to develop as they did in urban America. The simplest answer is, of course, that embryonic machines in new nations were generally thrown out by military coups. Beyond this truth, however, there are additional reasons why machines failed to flourish that are based on the social context of new nations and the dynamics of machine politics itself.

The decline of machine politics in America is of only limited use in accounting for what happened to rudimentary machines in the new states. How, after all, does one compare the demise of two machines, one of which (U.S.) appears to die a more or less "natural" death with a machine that is struck down by military force?

Samuel Hays ascribes the atrophy and disappearance of the American urban machine to certain rather obvious but momentous changes in American life. In the first place, a continually increasing majority of the active American electorate has moved above the poverty line. Most of this electorate is no longer bound to party through the time-honored links of patronage and the machine. Indeed, for a large number of people, politics appears to have the character of an item of luxury consumption. . . .[65]

The services that tied the client to the machine were either no longer necessary or were performed by other agencies than the machine. In a full-employment economy with rising wages, patronage was insufficient cement with which to organize and control a party. With aid to dependent children and old age assistance becoming the formal responsibility of government agencies, "the precinct captain's hod of coal was a joke."[66] The protective and defensive

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[61] Where business secures freedom from restrictions via its ties with the machine, economic growth may be speeded but it appears, in most cases, that businesses seek favored, or monopolistic positions free from competitive rigors. See, for example, the *Santhanam Report*, *op. cit.*, *passim*.

[62] Non-machine rulers often make use of similar income-producing tactics (e.g., Thailand) but, in the case of the machine, its needs are typically greater and its resources are usually destined for wider distribution as well as personal gain of office holders.

[63] Landé, *op. cit.*, p. 48.

[64] Among those new nations where ruling parties possess notable machine characteristics, one might include India, Malaysia, the Philippines, Ivory Coast, Liberia, and especially Lebanon. In the Ivory Coast and India the ruling parties still retain some of the "mass movement" ideological features that marked their earlier history.

[65] In William Nisbet Chambers, *op. cit.*, p. 305. To my knowledge, no actual empirical tests of hypotheses advanced for the rise or decline of machine politics have been attempted. It would be instructive, for example, to plot the increases and decreases of machine-style politics over time in a number of American cities against possible explanatory variables such as rates of in-migration, changes in per-capita income, changes in income distribution, changes in welfare measures, rates of education, and so forth. I am grateful to Garry Brewer for suggesting this general line of inquiry.

[66] *Ibid.*, p. 121.

function of the machine had simply ceased to be important political incentives.

Viewed from another angle, the machine simply destroyed its own social base. It had flourished among those who were, for one reason or another, "civic incompetents"; so when immigration slackened, when the new citizens gained a secure economic foothold, and when they developed wider loyalties, the central prop of machine politics was destroyed. Here and there, individual politicians managed to adapt to the new style and incentives, but the machine itself disappeared along with its social context.

The failure of machines in the new nations not only differed from the American pattern, but varied somewhat from case to case due to the special circumstances of each nation. Nevertheless, it is possible to discern a number of important factors that appear to have been significant in the demise of many such embryonic machines.

In the first place, the full development of a machine depends on its evolving capacity to create and maintain a large popular following with particularistic inducements. Typically, this capacity has developed best in the context of electoral pressures. Elections in American cities were virtually guaranteed by the fact that the city was a unit in a larger political system which sanctioned elections; machines perfected their techniques in the knowledge that they would always face periodic electoral opposition. Ruling parties in new nations, however, often began with a considerable store of popularity generated in the nationalist period. As this support deteriorated, the dominant party did not necessarily have to fall back on material incentives to retain its wide support; it could alternatively abrogate elections and escape the usual machine pressures. A good many nationalist leaders—having goals of transformation in mind—were increasingly discouraged at the growth of particularistic demands from all quarters that liberal democratic forms seemed to foist on them. Not having the heart for mediating between a host of what they considered short-sighted parochial demands made of machine bosses, many concluded that liberal democracy stood in the way of long-run national goals. Both Nkrumah and Sukarno spoke feelingly in this regard, and both consciously chose to eschew machine politics for more grandiose, symbolic goals.[67]

Another factor that basically altered the character of machines in some cases was the predominant position of a single ethnic group. In urban America it was virtually never possible for a machine to rule without being obliged to knit together a broad ethnic coalition. Where machines could be based on one dominant ethnic group in new nations—e.g. pre-Ne Win Burma and, to a smaller extent, Nigeria—the excluded ethnic groups, which were often geographically concentrated, demanded at the very least more regional autonomy and launched secessionist revolts in some areas.[68] The resulting threat to the territorial integrity of the state was commonly the occasion for military takeovers.

Looking at those nations in which machines have developed with some vigor, the importance of elections and ethnic balance (or homogeneity) is manifest. Lebanon, Malaysia, and India, for example, are balanced ethnically so as to require some form of collaborative rule while the Philippines is relatively homogeneous ethnically; all four have retained electoral forms. Beyond these two factors, however, are two broad obstacles to machine politics relating to the nature of a machine's clientele and its resource base.

Machines in American cities tended to live beyond their means and the evidence suggests that machines in new nations behave similarly.[69] As a form of rule machines are particularly subject to what Zolberg terms "an inflationary process of demand-formation"[70] and naturally thrive best in a buoyant economy that provides them with a continually ex-

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[67] In an otherwise perceptive article, Edward Feit characterizes, I think mistakenly, Nkrumah's CPP before the military coup as a political machine. He distinguishes between a political party which "aggregates demands and converts them into legislative policy" and a political machine which "exists almost exclusively to stay in power." The problem, of course, is that many regimes are motivated almost solely to stay in power—e.g., the Thai military, Haiti's Duvalier—but the term machine should be reserved for civilian regimes which rest on a popular base. The CPP, until about 1960, might profitably be seen as a machine party, but thereafter coercion and symbolic goals dominated. "Military Coups and Political Development: Some Lessons from Ghana and Nigeria," *World Politics*, XX (January, 1968), 179–193.

[68] For an excellent discussion of ethnic configurations and their political implications, see Clifford Geertz (ed.), *Old Societies and New States* (New York: Free Press, 1963).

[69] This fact may indicate that machine politics is not a stable form of rule.

[70] Aristide R. Zolberg, *op. cit.*, p. 149.

panding store of material incentives to distribute. It is perhaps no coincidence that the high-water mark of machine politics in the new nations occurred in the mid-1950's when Korean War boom prices for primary exports underwrote high rates of growth. In addition, there were a large number of "one-time-only" rewards available to ruling parties after independence. Foreign businesses could be nationalized, new franchises and licenses could be let, older civil servants could be replaced by loyal party workers, but the supply of such material incentives was soon exhausted in the absence of economic expansion. Of the later cabinets before martial law was imposed in Indonesia, Feith claims,

> . . . perhaps most fundamentally, the weakness of these later cabinets stemmed from their shortage of disposable rewards . . . Moreover, the number of material rewards and prestige roles which government was expected to provide did not decrease . . . In sum, then, those cabinets were almost as poorly equipped to reward as to punish.[71]

It is reasonable to suppose that the Indonesian case is not unique. The material rewards were, finally, not sufficient to the task and, amidst the ruling party's loss of support, the military —which, if it could not reward, could at least restrain and punish—stepped in.

The line of reasoning developed above suggests that perhaps the machine flourishes best at the sub-national level where it was confined in the U.S. That is, *the durability of this political form is maximized where there is an external guarantor of the electoral process, where the machine is a part of a larger growing economy that can afford its expensive habits, and where its bosses do not have a monopoly of coercive authority*. A large measure of the instability of machines in developing nations may derive from their national rather than local character.[72]

Finally, in those nations where demand-inflation was not the key factor, there was some question of how well suited the social context was to machine politics. On the one hand, the machine faced opposition from a small but strategically-placed middle class of civil servants, professionals, students, and, above all, army officers, which was less amenable to material incentives and was, like its American counterparts, profoundly alienated by machine corruption and patronage. On the other hand, the machines, particularly in Africa, faced large numbers of quite traditional folk for whom religious and cultural issues were still important[73] and whose leaders realized that the machine threatened the ascriptive basis of their power. These populations by and large remained outside the scope of the machine and represented, at a minimum, a latent challenge to the machine's authority. Bastions of tradition were often found in areas of "indirect rule" where social and political change had been less severe, while the machine won support especially among urban migrants and in areas (often "directly" ruled) where folk ways had been decisively uprooted by colonialism. The *transitional* population on which the machine relied was, in these cases, simply not large enough to sustain this form of government when it was menaced by widespread traditional recalcitrance and a powerful middle class with military allies. Macines require not only an economy that performs tolerably well but a social context that corresponds to the inducements it can give; only where both conditions have been satisfied have machines managed to survive and grow.

[71] Herbert Feith, *op. cit.*, p. 572.

[72] I am indebted to Professor Henry Hart for suggesting this.

[73] To stretch a point, one might link them with the forces in American politics that felt strongest about Sunday laws, prohibition, and so forth.

SOME ECONOMETRICS OF THE HUK REBELLION*

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Shortly after World War II a Communist guerrilla army, the Hukbong Magpalayang Bayan (HMB), or People's Liberation Army, became a serious threat to the new Philippine Republic. The Huks, as they are commonly known, controlled large parts of the sugar cane and rice growing areas of Central Luzon and carried out military and political operations in other parts of the islands. Like their Communist counterparts in Vietnam and Malaya, the Huks began as an anti-Japanese guerrilla army. In fact, Huk originally referred to Hukbalahap, a contraction of a phrase meaning People's Army Against the Japanese. As the military arm of the Communist Party, however, their ambitions always exceeded mere anti-Japanese activities.

After the Japanese defeat, successive Philippine governments wrestled with the problem of eliminating the Huks. Policies of coercion failed because the Philippine Army and Constabulary were not up to the task. Policies of conciliation failed because the demands of the Huks were regarded as unreasonable. By 1949 it became clear that the issues dividing the Huks and the government would have to be settled by force. Following the allegedly fraudulent election of President Quirino in 1949 the Huks gained steadily. By 1950 large unit raids were common and a full-scale attack on Manila was envisioned for 1951. Before that time arrived, however, the tide had begun to turn. Under the new Defense Secretary, Ramon Magsaysay, the Constabulary and Army were reorganized, and by the early 1950s the Huks were on the run, one of the most dramatic events being the surrender of former Huk Supremo Luis Taruc in May 1954.[1]

Despite the decimation of Huk units in Northern and Southern Luzon, Panay, and other regions, the movement survived and persists to this day especially in the part of Central Luzon known as Huklandia—the provinces of Bulacan, Nueva Ecija, Pampanga, and Tarlac. Although this region comprises only a small part of the Philippines (see Fig. 1), its size understates its importance. With close proximity to Manila and a leading position in rice production, it has always played a key role in Philippine history.

Huklandia is the only region in which the Huks flourished.[2] During the period 1948–1953 Huk expansionary forces had moved up and down the Sierra Madre cordillera, north to the Cagayan valley and the Ilocos region, south from Laguna to the Bikol provinces, and across the sea to Negros and Panay. The striking feature of this expansion, however, was its failure. Huk outposts were readily smashed, usually with the aid of defectors and informants from the local population.[3] A resurgence of the Huks over the past six or seven years has also been accompanied by the growth of a significant following outside Huklandia, especially in Southern Luzon. But again it does not appear that the Huks have been able to gain the kind of effective domination achieved in Central Luzon.

Figure 2, based upon data obtained from the Constabulary, shows the location of Central Luzon barrios[4] supporting the HMB in 1968. What are the peculiar features of these dissident barrios? Why should the Huks succeed there but not elsewhere? An operational theory of the Huk rebellion should be able to tell what special characteristics are to be found. For example, some have argued that the desire for land by the peasantry is a prime motivating force in the rebellion. We should expect to find, then, that barrios in which few farmers own their own land tend, other things being equal, to support the Huks. To carry out tests of this

* This article was written while the author was at the RAND Corporation, Santa Monica, and the Institute for Advanced Study, Princeton. The views presented are his own, and do not necessarily reflect those of the institutions with which he has been affiliated.

[1] A brief history of the Huk movement is contained in the first few chapters of A. Scaff, *Philippine Answer to Communism* (Stanford: Stanford University Press, 1955).

[2] N. Valeriano and C. Bohannan, *Counter-guerrilla Operations: The Philippine Experience* (New York: Praeger, 1962), p. 33.

[3] W. J. Pomeroy, *The Forest* (New York: International Publishers, 1963), esp. pp. 38–43, 156, 166–168; Valeriano and Bohannan, *op. cit.*, p. 32. See also V. Lieberman, "Why the Hukbalahap Movement Failed," *Solidarity* (October–December 1966), pp. 22–30.

[4] A barrio usually contains 1000–2000 people; a municipality is generally composed of 15–40 barrios.

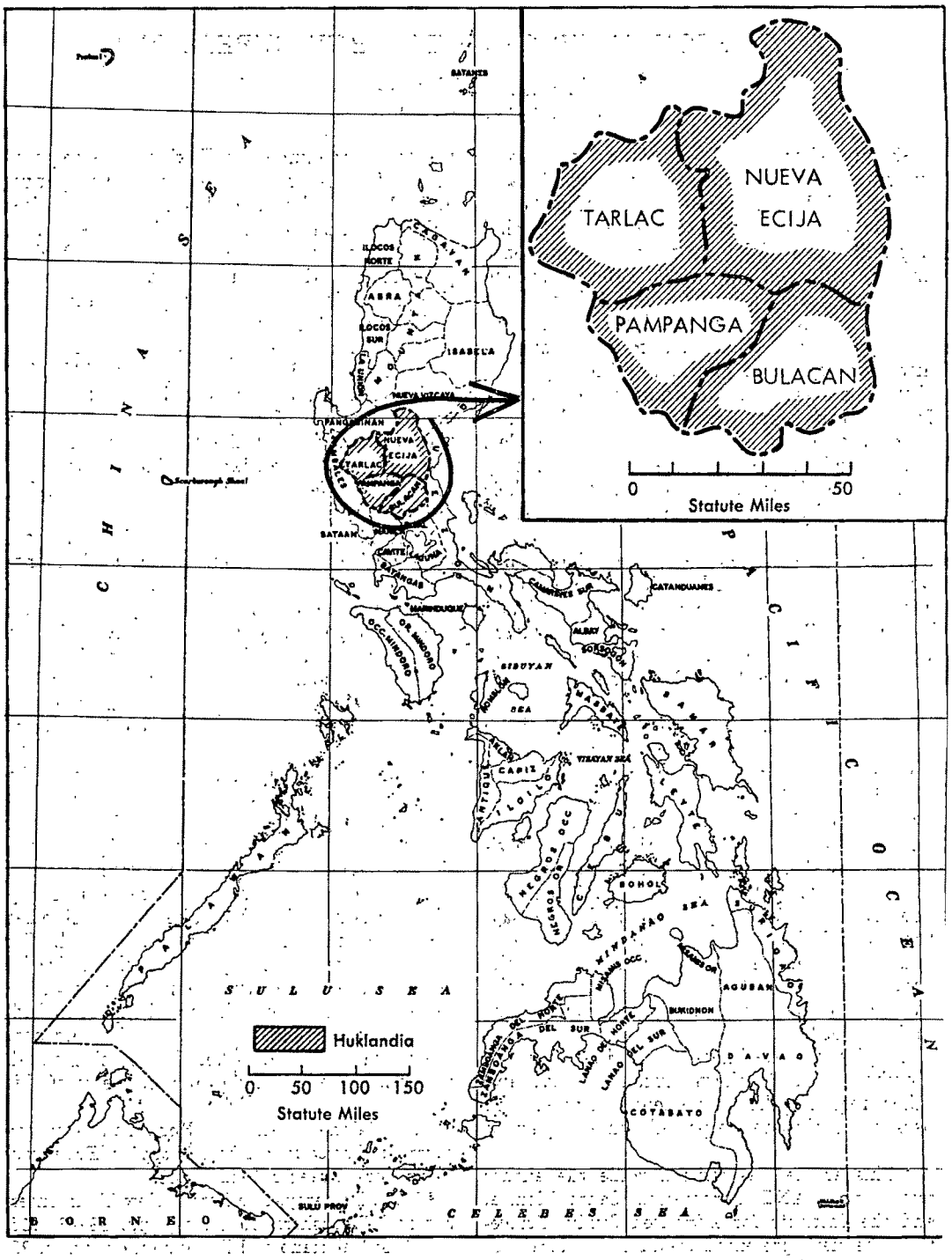


FIG. 1. Map of the Philippines.

and other theories requires an explicit framework or model. Application of the econometric approach will show that the successes and failures of the Huk movement can be explained well by a few cultural, economic, and geographical variables.

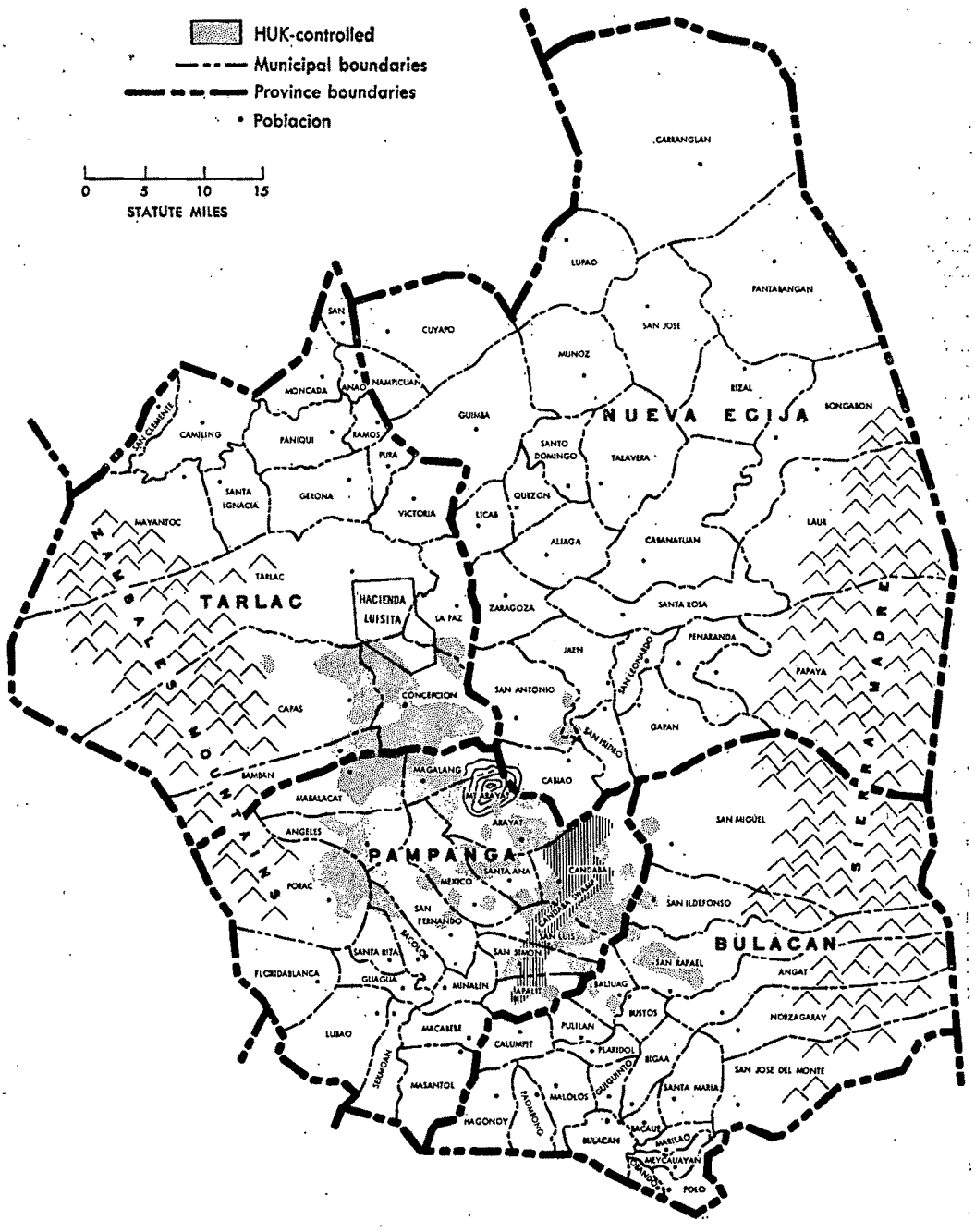


FIG. 2. Huklandia.

I. A MODEL OF REBELLION

For some time economists have represented the economic system by a set of equations—behavioral, institutional, technological, and definitional. The variables in these equations are those in which the economist is interested—prices, wages, employment—plus those non-economic variables that must be taken into account—family size, weather, technological conditions. A representative equation in an economic system would be the demand equation, relating the quantity of a good purchased to its price, the prices of other goods, income, family size, age, and so forth. Each decision

made by "economic man" can be represented by a behavioral equation. The equation enables us to determine what an individual or group will choose to do, given the objective costs and benefits associated with the various choices and given the tastes or attitudes of those making the choices. In the demand equation the price of the good is an objective cost; the age of the consumer is a variable characterizing the consumer himself and therefore his likely tastes and attitudes.

There is no reason why the political system cannot be thought of in the same way. Whether a village is government- or rebel-controlled is a consequence of decisions made by people, individually or collectively, in that village. Their decision will be a function of the objective costs and benefits attached to alternative choices, and of the basic attitudes of these people toward the rebels and toward the existing order. There is, then, an equation relating the degree of government or rebel control in an area to the "prices" of various actions in the area, and to those characteristics that determine the basic attitudes of the population.

The characteristics of the population might be represented in the equation by variables such as language, religion, occupation, and literacy. Determined over long periods of history they are usually not affected by the events of the rebellion (with the significant exception of its outcome). They may be regarded therefore as determined for the most part outside the rebellion model. On the other hand, the costs and benefits associated with different peasant actions are very much under the influence of government and rebel policy. It is obviously desirable for each side to attach high rewards and severe penalties to favorable and hostile acts. But the business of setting these prices is itself costly. Possessing limited resources, neither side can afford to reward good behavior and punish misbehavior each time they occur. Instead a strategy must be worked out by which resources flow to those places that yield the greatest returns for a given cost.

The cost of influencing peasant behavior in a community seems to depend heavily on the friendliness or hostility of the surrounding area. To determine one's allies and enemies requires communication with the village. To reward allies and punish enemies requires access by military and political personnel. The movement of messages and men is obviously more expensive when they must pass through enemy areas. The rational allocation of resources thus compels each side to concentrate on contiguous areas.

As a consequence, the pattern of incentives in any community depends upon the control situation of its neighbors. There are, of course, additional factors. Guerrillas are thought to be relatively more efficient in areas of low mobility such as mountains and swamps. Thus, villages located near mountains may face exceptional pressure from the rebel side. But this topographical factor is exogenous and may simply be added to the list of exogenous variables. The principal endogenous influence on incentive patterns is contiguity. We may therefore replace the prices in the control equation with an index of Huk control in neighboring areas. The control equation may then be written formally:[5]

$$HC_j = F(HCN_j; X_{1j}, X_{2j}, \cdots, X_{nj}; e_j), \quad j = 1, 2, \cdots, J. \quad (1)$$

HC is a variable measuring the degree of Huk control (or rebel support). The subscript j indicates the municipality; in all there are J. HCN is an index of Huk control in neighboring municipalities. X_1 through X_n are n exogenous variables; e is a random error representing ignored minor variables.

For simplicity, HCN is assumed to be an ordinary weighted average of Huk control in bordering municipalities:

$$HCN_j = \sum_{m=1}^{J} a_{jm} HC_m, \quad (2)$$

$$\sum_{m=1}^{J} a_{jm} \leq 1, \quad j = 1, 2, \cdots, J. \quad (2a)$$

In the statistical work that follows the weights were chosen in proportion to the length of the border between the municipality and each neighboring municipality.[6] If the total border of municipality A is 20 miles and munici-

[5] The pattern of incentives obviously depends upon the control situation within the community itself. In other words, HC_j appears on the right hand side of the equation as well as the left. Equation (1) must therefore be regarded as derived from an original equation, the HC_j on the right hand side having been "solved out." The parameters of equation (1) will as a consequence be somewhat larger than those of the original equation.

[6] Since this study is confined to a particular geographical area, municipalities on the edges of that area will have borders with external municipalities that are not represented in the summation of (2a); hence, the inequality. This is a reasonable way of handling the weights since external municipalities have no critical barrios for primarily exogenous reasons and therefore make no contribution to HCN. The alternative would be to force the weights to add to one by scaling them up, resulting in a somewhat lower estimate of k.

palities A and B share a border of two miles, the $a_{AB}=.10$. (Note that in general $a_{AB} \neq a_{BA}$). Alternative weighting principles, such as distance from the center of one municipality to the center of another, would cause negligible changes in the index.

If the general function F in equation (1) can be approximated by a linear function,

$$HC_j = k\left(\sum_{m=1}^{J} a_{jm}HC_m\right) + \sum_{i=1}^{n} b_i X_{ij} + e_j, \quad j = 1, 2, \cdots, J. \quad (3)$$

If all the variables on the right hand side of equation (3) were exogenous, the parameters (k and the b's) could be estimated by running a simple multiple regression. But obviously HCN is not exogenous. It is a linear combination of HCs and these values are determined within the model, that is they are endogenous. According to equation (3), to know Huk control in one municipality we must know Huk control in neighboring municipalities. But if A is a neighbor of B, B is a neighbor of A. Thus, to know control in A we must know control in B, and to know control in B we must know control in A. Control in all municipalities must therefore be determined simultaneously by the J equations, one for each municipality.

There is considerable econometric literature on the estimation of parameters in simultaneous equation systems.[7] One of the elementary points is that ordinary least squares or multiple regression generally leads to biased and inconsistent estimates when there are two or more endogenous variables in an equation.[8] However, the sophisticated methods devised to yield consistent estimates are not applicable (without modification) to this special case. As it turns out, consistent estimates can be obtained by a very simple procedure.

(For those whose mathematical background does not include matrix algebra, a crude prose translation of the remainder of this section will be given. The objective of these manipulations is to transform equations (3) into a new set of equations that do not include HCN, but only HC and the X's. Equations (3) read: Huk control in a given municipality depends upon Huk control in neighboring municipalities and the environmental conditions (the X's) within the given municipality. This is transformed to: Huk control in a given municipality depends upon the environmental conditions in the given municipality and upon the environmental conditions in the neighboring municipalities. Thus, "environmental conditions in the neighboring municipalities" is substituted for "Huk control in the neighboring municipalities." Needless to say, the mathematical counterpart of this is at once more precise and more complex. In the new equation Huk control depends only on factors that are exogenous, that is, they affect Huk control, but are in turn unaffected by it. Using the new equation estimates of the parameters of equations (3) can be obtained that are far more accurate than direct use of equations (3). In particular, application of multiple regression directly to equations (3) would result in overestimation of the importance of coercion or contiguity, and more important, severe underestimation of the role of other variables.)

Consider the J equations (3) in matrix notation:

$$HC = kAHC + Xb + e, \quad (4)$$

where HC is a $J\times 1$ vector of observations on Huk control; A is a $J\times J$ matrix of weights, a typical element being a_{jm}; X is a $J\times n$ matrix of J observations on n exogenous variables; and e is a vector of random errors.

Manipulating (4) we have:

$$HC - kAHC = Xb + e,$$
$$HC = (I - kA)^{-1}Xb + (I - kA)^{-1}e.$$

Letting $u = (I-kA)^{-1}e$

$$HC = (I - kA)^{-1}Xb + u. \quad (5)$$

In this equation system, HC is expressed entirely as a function of exogenous variables. Consistent estimates of the parameters can be obtained by applying least squares or the equivalent method of maximum likelihood. Unfortunately, this does not lead to a straightforward linear multiple regression because the equations are not linear in the parameters. To minimize the sum of squares of the residuals, or equivalently to maximize the likelihood function under normality, we choose a wide range of values of k, substitute them into $(I-kA)^{-1}$, and run multiple regressions of HC on the transformed Xs. The value of k that minimizes the sum of squares is the least squares and maximum likelihood estimate. The regression coefficients for this value of k then give the estimate of b.

A better understanding of the equation system (5) can be gained by expanding the ma-

[7] The best textbook reference is E. Malinvaud, *Statistical Methods of Econometrics*, North-Holland Publishing Co., Amsterdam, 1966.

[8] An estimate is unbiased if its expected value equals the true value of the parameter. An estimate is consistent if, as the sample size becomes large, the estimate tends in a stochastic sense to the value of the parameter. See M. G. Kendall and A. Stuart, *The Advanced Theory of Statistics*, Griffin, London, 1961, Vol. 2, Ch. 17.

trix $(I-kA)^{-1}$. Under appropriate conditions, $k<1$ and the inequalities (2a) being sufficient;

$$(I - kA)^{-1} = I + kA + k^2A^2 + \cdots .$$

Ignoring the error term,

$$HC = Xb + kAXb + k^2A^2Xb + \cdots . \quad (6)$$

The first term in this series represents the influence of the characteristics of a municipality on Huk control within that municipality. If k were zero this is the only influence that would exist. With k greater than zero neighboring municipalities begin to play a role. In the second term the expression AX represents weighted averages of the characteristics of bordering municipalities. These weighted neighborhood characteristics operate on Huk control in the same manner as the characteristics of the original municipality, except that they are deflated by k. If k is large the characteristics of the neighbors will be important; if k is small they will not.

The third term represents the effects of characteristics in municipalities bordering on the neighbors of the original municipality. In the expression A^2X the characteristics of these second-order municipalities are weighted according to their indirect influence through the first-order municipalities. Since A^2 is smaller than A and k^2 is less than k these second-order effects will be less than the first-order.[9]

Pursuing the higher order terms it is clear that every municipality will affect every other municipality, but that the impact will lessen with the distance between them.[10]

It should be stressed that the value of k depends not only on the validity of the allocation theory, but also on the extent to which coercion is used. In the absence of coercion k must be zero. On the other hand, with great and efficient exercise of threat and intimidation it can reach one, in which case the theory implies almost complete control by one side or the other. The estimate of k obtained at any point in time refers of course only to the use of coercive resources at that point. With changing capabilities either side can effect large shifts in the level of control and accompanying changes in the value of k.

[9] Smaller in the sense that the average element of A^2 is smaller than the average element of A. For particular elements this need not be true.

[10] The analogy of this discussion to input-output systems should be obvious to economists. The matrix A is an input-output matrix with the special condition that each industry must sell to the same industries from which it buys.

II. THE HUK EQUATION

Some barrios of Huklandia are so dominated by the Huks that they are classified as critical by the Philippine Constabulary. The specific criteria are: first, the Huks must exercise considerable authority over local officials; and second, the residents must actively support the Huks through monthly contributions and other assistance. No distinction is made whether this support is obtained voluntarily or by force, and in practice this would probably be impossible to ascertain. As the model suggests, Huk support will generally depend on both popularity and pressure.

The variable HC (Huk Control) is defined to be the percentage of critical barrios in a municipality in 1967. All references to Huk "control," "support," or like terms refer to this measure. Statistics recently released by the Constabulary indicate Huk activities outside Central Luzon, principally in Southern Luzon. At the time my data were collected (1967–1968) I was assured that there were no critical barrios in that region. The new data appear to contradict this assurance. On the other hand, the very small number of Constabulary assigned to the area seems to belie the new data. Unfortunately, I have no firsthand acquaintance with them. If there are in fact critical barrios in Southern Luzon, a separate econometric analysis for that region should be conducted. Simple extrapolation of the Central Luzon results would be inappropriate.

The exogenous variables were constructed from data in the 1939 and 1960 Philippine Censuses of Population and Agriculture. These included economic variables (tenancy, average farm size, gross peasant farm income, value of farm output, type of agriculture, farm population); cultural variables (language, religion, education, literacy); demographic variables (population growth, population density); and geographical variables (mountains, swamps). For two reasons it was felt that the 1939 data would be most appropriate. First, to be truly exogenous some of these, particularly the economic variables, should antedate the formation of the Huks in 1942. Second, some potentially important variables can be constructed only from the more comprehensive 1939 Censuses. As it turns out the 1939 variables actually have larger coefficients and greater statistical significance than their counterparts from 1960. (The reason for this will be discussed later in this section.)

To study the influence of changes in the environment between 1939 and 1960 rate of change variables were constructed where feas-

ible. In fact, most exogenous variables changed little during this period, the notable exceptions being population and type of agriculture, and, to a lesser extent, tenancy, farm population, and farm size. No change variables were found to be statistically significant.

In all, 57 Central Luzon municipalities have been included in the study. These range from Malolos in the south to Paniqui in the north. The only reason for limiting the analysis to this area is to reduce the computational work. The 57 municipalities cover all the Huk area, plus the surrounding areas in every direction. There is nothing to be gained from extending the boundaries. As it turns out, the equation arrived at predicts Huk control outside of the sample with great accuracy and inclusion of additional municipalities would only confirm what was already well established.

Estimating the parameters of the model from the reduced form we arrive at the first equation (A) of Table 1. This equation includes only those variables that possess statistical significance by standard statistical criteria. (The data from which this equation is derived are given in Table 2.) Three principal conclusions may be drawn. First, k is large and hence our theory of coercion is well supported. Second, the presence of Pampangos, an ethnic and linguistic minority, is a crucial condition for Huk success. Third, Huk control will be greater where most men are farmers and most farmers are tenants.

To appreciate the importance of k being as large as .8, consider the case of a municipality surrounded by thoroughly Huk-controlled territory (that is, HCN = 100). Let us suppose that this surrounded municipality possesses none of the characteristics favorable to Huk success. There are no Pampangos, few tenant farmers, and so on. According to equation (A), this unreceptive place would find 80 percent of its barrios critical.[11]

Examples as extreme as this do not occur in the data, but there are some illustrative cases. San Rafael in Bulacan has few Pampangos and on the basis of its cultural and economic features should be relatively Huk-free. Yet one in every six barrios is critical. Figure 2 shows that San Rafael is bordered on the west by a solidly Huk region around the Candaba swamp. To the east the Sierra Madre offers excellent refuge to Huk regulars. To the north and south neighbors San Ildefonso and Baliuag are in similar positions. Thus, for apparently geographical reasons an otherwise peaceful spot finds itself with a serious Huk problem.[12]

To return to a point made earlier, there is nothing inherently stable about the parameter k. Its value as computed in the cross-sectional analysis reflects the importance of the hypothesized coercion mechanism at a specific point in time. With more effective use of coercion by the Huks k would rise, indicating an expansion of Huk control outside of the shaded areas of Figure 2. A decline in k would, of course, result in a contraction of area under control. The Huk expansion of the immediate post-war period and the contraction of the 1950's should be thought of primarily in these terms. The reorganization and revitalization of an ineffective Constabulary by Magsaysay into a formidable anti-guerrilla force drastically reduced the importance of coercion in the Huk campaign. Accordingly, a reduction of k to a very small magnitude results, when plugged into equation (A), in a predicted number of barrios under Huk control that is but a meager fraction of the actual 1968 figures. Although detailed statistics are not available to the author this prediction would probably not correspond too badly to actual conditions in the late 1950s.

Among exogenous variables the proportion of Pampangos in the population dominates. In the absence of Pampangos the impact of other exogenous variables is nil. Apparently the Huks can control non-Pampango areas only through external pressure. In this sense the presence of Pampangos is a necessary but not sufficient condition for Huk success. Outside of Pampanga and the Pampangan-speaking areas of Tarlac, Nueva Ecija, and Bulacan, the equation predicts accurately the absence of any critical barrios.

The predominance of the Pampango variable is due to the particular way that P enters into equation (A). Suppose instead we assumed that the presence of Pampangos is not a necessary condition, but just one among sev-

[11] It should be mentioned that there are factors other than coercion that would be contained in the contiguity effect. For example, the strong kinship tie among Filipinos would imply sympathy for the cause of relatives in neighboring areas. But the quantitative importance of this and similar qualifications is likely to be small.

[12] The use of coercion does not imply a high level of violence. An efficient coercive organization does not have to carry out its threats if they are truly credible. An occasional penalty swiftly administered may be all that is necessary. The correlation between effective coercion and observed incidents of violence is probably quite weak.

TABLE 1. HUK CONTROL EQUATIONS

(57 Municipalities)

Equation		$\bar{R}^2$ (Reduced Form)	F (Reduced Form)
(A)	$HC = -1.53 + .8\ HCN + P[2.28\ FMP - 1.69\ OWN + .33\ SGR + 23.0\ MNT + 11.6\ SWP]$ $(.76)^a\quad (.46)\quad (.26)\quad (.09)\quad (3.5)\quad (4.2)$	.86	60.5
(B) [k=0]	$HC = .94 + P[4.47\ FMP - 2.10\ OWN + .42\ SGR + 40.3\ MNT + 16.0\ SWP]$ $(2.60)\quad (.86)\quad (.47)\quad (.16)\quad (6.7)\quad (8.6)$	.73	27.8
(C) [P linear]	$HC = 3.00 + .9\ HCN - .03\ P + 2.15\ FMP - .89\ OWN + .30\ SGR + 14.5\ MNT + 16.5\ SWP$ $(1.19)\quad (.03)\quad (2.15)\quad (.19)\quad (.10)\quad (3.2)\quad (4.4)$	.68	17.3
(D) [k=0, P linear]	$HC = -6.13 + .19\ P + .88\ FMP - .65\ OWN + .44\ SGR + 20.9\ MNT + 18.5\ SWP$ $(9.70)\quad (.09)\quad (.54)\quad (.29)\quad (.18)\quad (5.4)\quad (10.7)$	.50	8.3

Definitions:

HC = the percentage of barrios in a municipality under Huk control (1967–1968).[b]
P = the proportion of the population that speak the Kapampangan dialect (1939).[c]
FMP = farmers as a percentage of the population (1939).[c]
OWN = owners as a percentage of all farmers (1939).[c]
SGR = the percentage of cultivated land planted to sugar cane (1939).[c]
MNT = 1 if mountains are in or immediately adjacent to the municipality; 0 if not.
SWP = 1 if a swamp is in or immediately adjacent to the municipality; 0 if not.
HCN = a weighted average of HCs in bordering municipalities.

Notes:

[a] The figures in parentheses are standard errors.

[b] The percentage of variation in HC explained by equation (A) or (C) *given HCN* is substantially higher than the $\bar{R}^2$ for the reduced form. This latter figure is based only on the explanatory power of the exogenous variables.

[c] All 1939 data were obtained from the 1939 Census of Population or the 1939 Census of Agriculture.

eral exogenous factors. What would happen if P entered the equation additively rather than multiplicatively? The answer is given by equation (C). Taken as a whole, this is much poorer than equation (A). Furthermore, the coefficient of P is not even statistically significant! The explanation is that Huk control is confined for the most part to the Pampangan-speaking area; but not all of this area is Huk-controlled. Within the area, and only within the area, the remaining exogenous factors operate.

Strictly speaking, this is the way we must interpret equation (A). Having limited ourselves to only the simplest models, however, the supposed "necessity" of Pampangos is an exaggeration. More complex functional forms would almost certainly show that factors such as tenancy operate among non-Pampangos, but to a much smaller degree. We should expect to find many non-Pampangos freely embracing the HMB, and we do. This does not detract, of course, from the strong general tendency that the equation reveals.[13]

Two questions arise in any attempt to ascertain the behavioral interpretation of the Pampangan effect: Why would a serious insurgency be most likely to develop among Pampangos? And, why would other ethnic groups shun a Pampangan movement? The latter question is the easier and will be handled first. Based upon the history of rebellions in Central and Southern Luzon there does appear to be a strong element of mistrust between Pampangos and their neighbors, the Tagalogs and Ilokanos. During the first phase of the Philippine Revolution against Spain (1882–1896), the militantly nationalist Katipunan movement was overwhelmingly Tagalog and received little support from the Pampangans.[14] Even after hostilities broke out between the Tagalogs and the Spaniards (August 1896) Pampangans for the most part remained loyal to Spain and many served faithfully on the Spanish side. It was not until the defeat of the Spanish at Manila Bay by the Americans (May 1898) that the Pampangans began to favor the revolution. After joining the Republic in August 1898, Pampanga, although one of the wealthiest provinces, ranked a poor fifteenth in financial contributions.[15]

A similar lack of Pampangan enthusiasm for a predominantly Tagalog cause was displayed during the Sakdalista episode of the 1930s. The Sakdalista party was a radical agrarian movement in Central and Southern Luzon and ran successful candidates for public office. During the 1934 elections they received a significant percentage of the vote in every province of the region—except Pampanga.[16]

Thus, the forsaken Pampangos of the past two decades have been undergoing an experience familiar to the Tagalogs. In historical perspective, the Huk phenomenon is seen as a simple turnabout of the usual situation, but with this exception: the success of a Tagalog cause is little affected by Pampangan support or the absence of it. Tagalogs outnumber Pampangos by more than 6 to 1. Given the Huk performance with the potential backing of little more than 5 percent of the Philippine population, the Tagalog abandonment must rate as a decisive factor of the rebellion.[17]

To the north, especially in northern Tarlac, the principal neighbors of the Pampangos are Ilokanos. Here the historical evidence is more limited. However, the impression one obtains is that the cleavage between Ilokano and Pampango is more thorough than between Tagalog and Pampango. The Ilokano is noted for his clannishness and, although willing to follow his own leaders, he is suspicious of strangers. To the Huks' misfortune, he is one of the more extreme examples of Filipino sectionalism.[18]

We must now consider why this particular

[13] Since there are virtually no Pampangos in Southern Luzon success of the movement there would imply that either the language barrier has been hurdled, considerable coercion has been employed, or the movement there has a different basis.

[14] Pampangans are residents of Pampanga province; Pampangos are people who speak the Kapampangan dialect. Almost all Pampangans are Pampangos, but the reverse is not quite true. In this historical discussion the distinction, although necessary for literal accuracy, is of no great consequence.

[15] J. A. Larkin, *The Evolution of Pampangan Society: A Case Study of Social and Economic Change in the Rural Philippines* (Doctoral Dissertation in History, New York University, 1966). There are a number of Filipino historians who dispute the importance of sectionalism in the Revolution. See for example, T. Kalow, *The Philippine Revolution* (Manila, 1925). Among American writers it is often referred to as the Tagalog Rebellion.

[16] Carl H. Landé, *Leaders, Factions and Parties: The Structure of Philippine Politics* (New Haven: Yale University Press, 1965).

[17] From private conversations with Luis Taruc I gather that the Huk leadership was unaware of this, although in recent years Taruc has reflected on its importance.

[18] Marcelo Tangco, "The Christian Peoples of the Philippines," *Natural and Applied Science Bulletin*, XI, 9–114.

TABLE 2. DATA ON HUK CONTROL, LANGUAGE, TOPOGRAPHY, AND ECONOMIC CONDITIONS

(57 Philippine municipalities)

Municipality	Province	Percentage of Barrios Huk Controlled 1968 (HC)	Proportion of Population Able to Speak Pampangan Dialect (P)	Farms as Percentage of Population 1939 (FMP)	Percentage of Farms Operated by Owners 1939 (OWN)	Area Planted to Sugarcane as a Percent of all Cultivated Land 1939 (SGR)	Dummy Variable-Contiguous to Mountains (MNT)
Angeles	Pampanga	46.7	.88	2.6	14.6	53.9	1
Apalit	Pampanga	7.4	.99	6.7	9.4	11.4	0
Arayat	Pampanga	68.4	.98	8.2	12.4	22.8	1
Bacolor	Pampanga	29.4	.99	6.2	12.5	33.3	0
Candaba	Pampanga	48.1	.92	11.3	4.6	2.5	0
Floridablanca	Pampanga	0.0	.91	7.5	21.0	65.3	0
Guagua	Pampanga	0.0	.98	3.0	18.8	23.4	0
Lubao	Pampanga	2.5	.99	8.4	17.9	35.8	0
Mabalacat	Pampanga	73.7	.96	4.9	10.3	58.5	1
Macabebe	Pampanga	0.0	.99	4.3	14.0	6.6	0
Magalang	Pampanga	80.0	.97	7.8	9.5	49.0	1
Masantol	Pampanga	0.0	1.00	6.3	8.0	1.0	0
Mexico	Pampanga	25.0	.93	9.8	8.1	36.9	0
Minalin	Pampanga	0.0	.99	5.5	4.6	0.9	0
Porac	Pampanga	43.5	.95	7.9	8.4	61.6	1
San Fernanco	Pampanga	11.8	.95	3.4	12.2	46.4	0
San Luis	Pampanga	43.8	.96	6.5	11.3	4.8	0
San Simon	Pampanga	35.7	.97	10.5	18.5	7.1	0
Santa Ana	Pampanga	50.0	.99	8.2	11.5	21.8	0
Santa Rita	Pampanga	0.0	1.00	5.7	16.9	35.2	0
Sexmoan	Pampanga	0.0	.96	0.0	0.0	0.0	0
Bamban	Tarlac	88.9	.92	6.1	7.1	35.8	1
Capas	Tarlac	62.5	.93	9.4	15.4	19.0	1
Concepcion	Tarlac	78.6	.94	9.1	6.7	39.1	0
Gerona	Tarlac	0.0	.26	12.7	33.3	30.5	0
La Paz	Tarlac	9.5	.54	13.4	10.0	20.6	0
Paniqui	Tarlac	0.0	.44	12.1	30.8	14.6	0
Pura	Tarlac	0.0	.54	12.0	34.3	31.7	0
Ramos	Tarlac	0.0	.57	13.2	17.0	27.5	0
Santa Ignacia	Tarlac	0.0	.47	15.0	40.5	4.0	0
Tarlac	Tarlac	0.0	.81	6.7	23.7	25.3	1
Victoria	Tarlac	0.0	.28	14.3	31.3	17.6	0
Aliaga	Nueva Ecija	0.0	.03	23.4	15.6	0.2	0
Cabanatuan	Nueva Ecija	0.0	.03	13.0	19.0	0.5	1
Cabiao	Nueva Ecija	21.4	.65	11.8	14.3	0.1	0
Gapan	Nueva Ecija	0.0	.23	10.3	11.5	0.1	1
Jaen	Nueva Ecija	0.0	.05	26.3	11.7	0.5	0
Licab	Nueva Ecija	0.0	.10	21.3	19.8	0.7	0
Peñaranda	Nueva Ecija	0.0	.00	21.5	40.5	0.2	1
Quezon	Nueva Ecija	0.0	.02	21.2	23.2	0.3	0
San Antonio	Nueva Ecija	20.0	.12	19.8	20.4	0.6	0
San Isidro	Nueva Ecija	0.0	.02	17.3	18.5	0.3	0
San Leonardo	Nueva Ecija	0.0	.16	20.3	17.7	0.8	0
Santa Rosa	Nueva Ecija	0.0	.02	19.3	17.8	0.3	1
Zaragoza	Nueva Ecija	0.0	.04	25.7	24.8	0.5	0
Angat	Bulacan	0.0	.00	18.2	43.1	1.2	1
Baliuag	Bulacan	16.7	.03	7.7	9.3	0.3	0
Bustos	Bulacan	0.0	.00	16.8	16.1	0.7	0

TABLE 2—(*Continued*)

Municipality	Province	Percentage of Barrios Huk Controlled 1968 (HC)	Proportion of Population Able to Speak Pampangan Dialect (P)	Farms as Percentage of Population 1939 (FMP)	Percentage of Farms Operated by Owners 1939 (OWN)	Area Planted to Sugarcane as a Percent of all Cultivated Land 1939 (SGR)	Dummy Variable-Contiguous to Mount (MNT)
Calumpit	Bulacan	0.0	.23	9.0	16.9	20.6	0
Haganoy	Bulacan	0.0	.03	3.4	9.0	7.7	0
Malolos	Bulacan	0.0	.01	4.8	15.6	0.2	0
Paombong	Bulacan	0.0	.00	4.4	7.4	10.5	0
Plaridel	Bulacan	0.0	.00	11.0	14.3	0.4	0
Pulilan	Bulacan	0.0	.05	12.7	22.7	2.2	0
San Ildefonso	Bulacan	9.7	.02	14.9	0.0	0.1	1
San Miguel	Bulacan	5.0	.08	11.3	11.8	0.2	1
San Rafael	Bulacan	16.7	.01	14.9	42.8	0.3	1

Note: The SWP variable has a value of 1 for the municipalities of Apalit, Candaba, San Luis, and San Simon; for all other municipalities it is 0.

Communist movement began among Pampangos in the first place. One answer, and there is no one that can be held with any certainty, involves the general environment of Pampanga and the immediately surrounding parts of Central Luzon. This area has been at the forefront of the general drift toward tenancy and away from peasant ownership that has characterized the Philippines most of this century. As a consequence it became the focus of the radical agrarian politics of the 1930's, with the Socialist and Communist parties centering their activities around the capital city of San Fernando. Feeding on peasant discontent over land distribution and tenure conditions these movements took hold and there ensued peasant strikes, sabotage of crops, and violent clashes between socialist unions and landlord-backed organizations. The important point is that the Communist movement and the subsequent Hukbalahap became predominantly Pampangan movements. Although they faced similar agrarian conditions in their attempts at expansion outside the Pampangan speaking zone, they did not face other Pampangans. The rebellion could be exported therefore only by a heavy emphasis on coercion. As a result the most fragile part of the rebellion has been that outside the relatively reliable home base in Pampanga.

On this interpretation we arrive at the paradoxical conclusion that the rebellion arose out of a dissatisfaction with land tenure conditions, but could not expand to other localities where conditions were as bad or worse.[19] This interrelationship between land tenure and language is demonstrated by the manner in which the tenancy variable enters into equation (A). If P is one, land tenure exerts a strong influence on control. If P is zero, its effect is nil.

At first glance the positive contribution of tenancy to Huk success does not appear to be a remarkable finding. The literature on agrarian radical movements in Southeast Asia,[20] and on the Philippines in particular,[21] stresses the role of unequal land distribution and the resulting frustration of peasant aspirations. Yet certain aspects of this result are interesting. First of all, it is in sharp contrast to the author's research on the Vietnamese war, where it was found that high tenancy rates and several other measures of inequality contributed positively to the government, not the rebel, cause.[22] Without attempting a thorough explanation here, we can distinguish between the effects of tenancy and inequality on peasant aspirations, and their effects on economic, social and political organi-

[19] Note that Pampanga as a province has the highest overall rate of tenancy, but that there are many municipalities outside of Pampanga that have higher rates than specific Huk-dominated municipalities within Pampanga.

[20] E. Jacoby, *Agrarian Unrest in Southeast Asia* (Bombay: Asia Publishing Co., 1961).

[21] F. L. Starner, *Magsaysay and the Philippine Peasantry* (Berkeley and Los Angeles: University of California Press, 1961).

[22] Edward J. Mitchell, "Inequality and Insurgency: A Statistical Study of South Vietnam," *World Politics*, xx (April 1968), "The Significance of Land Tenure in the Vietnamese Insurgency," *Asian Survey*, August 1967.

zation of the village. Even if peasant frustrations are greatest in areas of inequality, and it is not clear that they are,[23] there exist offsetting elements in the way that unequal communities are organized. It can be argued that an external threat to the community in the form of a disciplined guerrilla force can best be handled when the structure of authority is simple. Landlords, with clear interests in defending the village, may be able to efficiently organize their tenants and debtors into a resistance. A more democratic community of small independent farmers may prove indecisive and incapable of organizing a defense. Thus we find in Vietnam relatively larger and presumably more effective local militia and paramilitary forces in areas of strong landlord influence. (Incidentally, if this thesis is correct we would probably have to characterize the Huk rebellion as the more spontaneous, rising out of peasant aspirations, and the Vietnamese as more induced, depending on the use of coercion by external organizations. Emphasizing that this distinction is one of degree and not of kind, the inference does not seem implausible.)

It is also interesting that land tenure should play so large a role where it varies so little. In the great bulk of the Pampangan-speaking municipalities owners represent only 5 to 18 percent of all farmers. Thus, tenancy (including part-ownership) varies from a high 82 percent to a very high 95 percent. Yet the coefficient of the OWN variable is so large (−1.7) that this represents a difference of 22 percentage points in Huk control. One would have thought that by the time tenants and part-owners reached 75 to 80 percent their influence would be so great as to yield diminishing returns to further increments in their number. Apparently not. This suggests that at moderate rates (25–75 percent) tenancy is not a crucial factor contributing to rebellion and becomes so only upon reaching the highest levels. It further suggests that owner-farmers, whose variability is much greater relative to their average numbers, may play a conservative role for exceeding in importance their proportion of the population.

Unlike tenancy rates, occupational composition does vary widely, both within Pampanga and without. In 1939 there were but 7 small farms in Sexmoan, Pampanga, a municipality of more than 10,000 people; whereas in Jaen, Nueva Ecija, there was one farm for every four persons. Assuming a family size of at least four, the farm population varied practically from 0 to 100 percent. The coefficient of the FMP variable indicates that Huk support among non-farmers (fishermen, merchants, laborers) was weak. Examples are found in Sexmoan, a fishing community, with no critical barrios, and San Fernando, location of the provincial capital, with but 11.8 percent. Candaba, on the other hand, with a farm population of over 50 percent, has 13 of its 27 barrios on the critical list.

To see how tenancy and farm population fail to operate as powerful forces outside the Pampangan-speaking area consider one extreme case. (There are many less dramatic examples.) Nampicuan in Nueva Ecija had 1451 farms in 1939. Four of these were operated by owners. By 1960 the figure had soared to 12. Over these 1451 farms were spread 5023 people, or about 3.5 persons per farm. Surely there were few individuals engaged in anything but agriculture. By 1960 the farm population had declined to 78 percent, still very high. The FMP and OWN variables possess values here more favorable to the Huks than in any municipality included in Table 2. Yet, because only 74 residents spoke Pampangan, and, interestingly enough, almost all the remainder were Ilokanos, there were no critical barrios.

The presence of nearby mountains raises Huk control by about 23 percentage points. This is easily explained by the comparative advantage possessed by guerrillas in poor terrain. In the same way, nearby swamps raise Huk control by 11 points. These results strongly reinforce the position that coercion plays a dominant role.

Turning to the sugar cane variable we must be a bit more speculative. Two interpretations may be advanced. First, there is usually a high proportion of farm laborers and migrant workers on sugar cane estates, and these groups may well be the most radical. (Unfortunately, no explicit variable measuring laborers or migrant workers is available on a municipality basis.) Second, the sugar cane landlord is probably more a profit-oriented and business-like and hence less paternalistic than the traditional Philippine landowner. His tenants would then be regarded as employees with no claims on his time and resources other than their share of the cash value of the crop. Since the traditional acceptance of the landlord's high status is predicted on a far more extensive set of roles than mere owner of the land, tenants may have little respect for their landlord's rights and the institutions that preserve them.

Thus far we've been satisfied with explaining a large part of the variation in Huk control across municipalities in terms of a few system-

[23] See Mitchell, *op. cit.*; and *Land Tenure and Rebellion*, The RAND Corporation, RM-5181 (Abridged) (Santa Monica, June 1967).

atic factors. It is often rewarding, however, to try to explain some of the deviant cases where actual Huk control differs substantially from that predicted by the equation. In two cases this seems worthwhile.

The municipality of Tarlac has no critical barrios according to the Constabulary. Yet it possesses some ideal attributes from the Huk point of view. Over 80 percent of the population are Pampangos, a fair amount of sugar cane is grown, the Zambales Mountains are nearby, and its neighbors to the south are under great Huk influence. The equation in fact predicts over 21 percent of the barrios as critical. A glance at Figure 2 gives some indication of how this deviation might be accounted for. The Hacienda Luisita appears to act as a blocking force against Huk expansion northward.[24] The Huks are very strong right up to the borders of the Hacienda but control no barrios beyond that. The Hacienda, a 15,000 acre sugar plantation, has had its own private army for many years and apparently has been able to control the local situation. It is interesting to discover that the new owners of the plantation, both of whom are Filipino politicians, have begun a more conciliatory approach to the Huks and have permitted them to organize peasants on the estate. The Constabulary believes that some of the plantation barrios may soon be on the critical list.

To the east of Tarlac in La Paz the Hacienda seems to serve the same function. From the equation one would predict about 20 percent Huk control compared with an actual 9.5 percent, an error of almost 11 percent. The map shows clearly that the only critical barrios in La Paz are in the southeast not far from some of the Huk barrios of Concepcion. In the west the Hacienda appears to halt the Huk thrust and all territory to the north of it is fairly secure.

The FMP, OWN, and SGR variables were calculated from 1960 as well as 1939 data. (The MNT and SWP variables have no time dimension, while P could be measured only for 1939.) Besides the *a priori* grounds for preferring 1939, these variables also turned out to have much larger and more significant coefficients. The 1939 OWN variable had more than twice as large a coefficient and much greater variance than its 1960 counterpart. The 1960 SGR coefficient was not even statistically significant. When 1960 data are used the burden of explanation is transferred to the MTN and SWP variables as well as to the 1960 FMP variable, which in a relative sense seems not to have changed much. Consistent with these findings is the result that a variable measuring changes in tenancy from 1939 to 1960 makes no significant contribution to equation (A).

The relative importance of pre-war conditions may be the consequence of a common political phenomenon. It often happens that during periods of great crisis, such as the Japanese invasion and the initiation of guerrilla activities, attitudes are formed that become hardened over time and inherited by succeeding generations. This view would also account for the persistent location of the movement in Central Luzon, especially Pampanga, despite a changing environment and twenty-six years of ups and downs in Huk fortunes. An almost identical explanation would seem appropriate in South Vietnam, where recent Viet Cong strongholds correlate highly with old Viet Minh areas.

A further cause of geographical stability in insurgent support is the relatively slow pace at which the important cultural and economic forces change. Some variables, such as farm incomes, tend to be unstable, but there is no indication that these variables play an important function in determining support. The regional patterns of the crucial factors—language, land tenure, occupation and terrain—are relatively constant over periods as long as a generation or more. Thus, we find the locations of highest tenancy in the Philippines (and in Vietnam) remaining roughly the same over the last thirty or forty years.

For this reason it is doubtful whether studies focusing on socioeconomic variables can explain short-run dynamic aspects of the rebellion. Since the War the Huk movement has risen to a threatening position, fallen to near insignificance, and risen again as a serious force. There does not seem to be any broad environmental factor whose path corresponds to this cycle.

The dynamic aspects of the rebellion would seem to be better studied from the point of view of competing organizations. I would suggest that it is the changing objectives, strategies, and resources of the Huks and the several Philippine governments that largely account for temporal shifts in the level of influence. (The reorganization of defense forces by Magsaysay has already been cited.) Given the relatively "soft" kind of data available for such a study, it is better handled as a speculative enterprise, and suited to a more conventional political analysis.

[24] The configuration representing the Hacienda in Fig. 2 is a gross approximation. For our purposes it is only necessary that its general location be correct.

ENVIRONMENT, PROCESS AND POLICY: A RECONSIDERATION*

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In recent years a growing body of research has used multivariate statistical techniques to examine the relationship between aggregate environmental characteristics and the public policies of state and local governments. This research has been concerned primarily with isolating or demonstrating the social, economic, and political correlates of either public policies (e.g., expenditures, revenues and referenda issues) or governmental structures (viz., form of government, size of election districts and type of ballot).[1]

One advantage of the aggregate approach, beyond the relative accessibility of data, is that it permits a systematic, comparative study of states or cities. On the local level this comparative approach provides a convenient supplement to the earlier case study approach which was concerned with the political processes and issues of particular cities.

A number of hypotheses have been suggested by studies employing either the case study or aggregate approaches. In those observations dealing with government structure, attention is usually directed to the council-manager plan as an example of progressive government. That is, city governments which are reform-oriented are likely to be found in more affluent, better educated, homogeneous, middle-class cities. The notion is that the middle class prefers a more efficient, professional city administration. Conversely, the mayor-council plan is usually associated with older, machine-type politics which allegedly reflects the preferences of the less affluent, less-educated, working class and ethnic minorities who are most concerned about political representation.[2]

The question of how to categorize commission government has presented a problem. Despite the fact that it was one of the earliest manifestations of the reform movement, its popularity has declined steadily, and it is rapidly fading into obscurity throughout most of the country.[3] Curiously enough, in spite of its decline in the last four decades and its rather marked politicization in those cities in which it does remain, scholars continue to consider both commission and council-manager governments analytically as reform governments. Indications of the weaknesses in such a combination are apparent in some of the anomalous relationships revealed and recognized in a recent study by Lineberry and Fowler.[4]

I. GENERAL STATEMENT OF PROBLEM

Two ideas emerge from the case and aggregate studies of public policies in cities. The results of the case studies suggest that political variables are important determinants of public policy. More specifically, political decision-making and interaction processes appear to be the key variables which must be studied in order to understand policy outcomes.[5] Conversely, the results of aggregate comparative studies on the state and local levels indicate

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[1] A good discussion of this literature can be found in Herbert Jacob and Michael Lipsky, "Outputs, Structure, and Power: An Assessment of Changes in the Study of State and Local Politics," *Journal of Politics*, 30 (May, 1968), 510–538.

[2] Perhaps the best known statement of this view is found in Edward C. Banfield and James Q. Wilson, *City Politics* (Cambridge: Harvard University Press and Massachusetts Institute of Technology Press, 1963); and their article, "Public-Regardingness as a Value Premise in Voting Behavior," this REVIEW, 58 (December, 1964), 876–887.

[3] Charles R. Adrian, *Governing Urban America* (2nd ed., New York: McGraw-Hill Book Company, Inc., 1961), pp. 214, 217–218.

[4] For example, they found that commission governments did not fulfill the objectives of institutional reform, i.e., this form of government seems to reflect rather than reduce the impact of social cleavages. Robert L. Lineberry and Edmund P. Fowler, "Reformism and Public Policies in American Cities," this REVIEW, 61 (September, 1967), 701–716.

[5] See, for example, Robert J. Mowitz and Deil S. Wright, *Profile of a Metropolis* (Detroit: Wayne State University Press, 1962); and Robert Dahl, *Who Governs? Democracy and Power in an American City* (New Haven: Yale University Press, 1961).

that policy outcomes are more closely associated with the social and economic characteristics of the units of analysis. These studies suggest that environmental rather than political variables appear to be much more important as policy determinants.[6]

There are several possible explanations for these somewhat contradictory conclusions. First, it may be that some variables are ignored in either approach, thus exaggerating the importance of those variables which are considered. For example, most case studies ignore or at least do not treat systematically the social and economic variables employed in the aggregate approach. The opposite emphasis is present in the aggregate studies, i.e., decision-making or process variables tend to be ignored. Second, and this applies mainly to the aggregate studies, it may be that the indicators which are used to measure process variables are simply too crude or insensitive to reflect the dimensions being considered. Attempts to quantify what are essentially behavioral variables frequently encounter this problem. Third, comparative studies of cities have been unable to control adequately for differences in local policies which may reflect state and Federal restrictions rather than local prerogatives. Thus state laws limiting the discretion of city governments can only confound attempts to explain the policies of these governments if this factor is not considered. Closely related to this problem is the fact that controls for time are rarely employed. Particularly in studies of reformism, cities are often classified as reformist or not regardless of when or how the particular charter was adopted. It is unlikely that events such as these, many of which occurred around the turn of the century, can be considered the same as events occurring at mid-century.[7] Finally, it may be that environmental factors are important determinants of policies, such as revenue and expenditure policies, which are substantially dependent on the economic potential of communities. But are these same environmental variables equally important in influencing policies which reflect more closely the manifest political values of a community (e.g., elections for public office and certain referenda decisions)?

Lineberry and Fowler, in their study of reformism in American cities, suggest that:

> The implicit, or at times explicit, causal model in much of the research on municipal reformism has been a simple one: socio-economic cleavages cause the adoption of particular political forms. A more sophisticated model would include political institutions as one of the factors which produce a given output structure in city politics. We hypothesize that a causal model would include four classes of variables: socio-economic cleavages, political variables (including party registration, structure of party systems, patterns of aggregation, strength of interest groups, voter turnout, etc.), political institutions (form of government, type of elections and types of constituencies), and political output.[8]

This study represents an effort to examine, with one modification, such a model of reformism. That modification is that the single policy output, or dependent variable, in this study involves change or innovation in a political institution form of government.

II. SETTING

Forty-three third-class cities in Pennsylvania have been selected for study. In 1957, the Optional Third-Class City Charter Law was signed providing all third-class cities with the option to adopt new municipal charters—either a mayor-council or council-manager plan.[9] If the local electorate chose to keep its existing commission form of government, which until 1957 had been required by state law, it could do so.[10] Under these conditions,

[6] See, for example, Richard E. Dawson and James A. Robinson, "Interparty Competition, Economic Variables and Welfare Politics in the American States," *Journal of Politics*, 25 (May, 1963), 265–298; Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW 60 (March, 1966), 73–82; and Thomas R. Dye, "Governmental Structure, Urban Environment, and Educational Policy," *Midwest Journal of Political Science*, 11 (August, 1967), 353–380. For an exception to this general finding, see Ira Sharkansky, "Economic and Political Correlates of State Government Expenditures: General Tendencies and Deviant Cases," *Midwest Journal of Political Science*, 11 (May, 1967), 173–192. See also the discussion of this question in Jacob and Lipsky, *op. cit.*, pp. 511–517.

[7] On this point, see, Jacob and Lipsky, *op. cit.*, p. 518.

[8] Lineberry and Fowler, *op. cit.*, p. 714.

[9] Hereafter referred to as the Optional Charter Law.

[10] Under the law, a city could place a proposal to elect a charter study commission before the voters either by ordinance or petition. If a commission were elected by popular vote, it could, after a study period, recommend either a mayor-council or council-manager charter, or it could

the state laws of Pennsylvania provide a common legal framework for these cities. That is, in 1957, all forty-three cities were governed by commission governments, and all were provided with the same opportunity to adopt new charters under the law. This, perhaps more than anything else, directs attention to the characteristics of individual communities in seeking some explanation for their action or inaction on the charter question.[11]

The time period considered is the years from 1957, when the Optional Charter Law was adopted, through 1966. During this period, nine cities voted to adopt new charters. Six cities adopted the mayor-council plan. Three adopted the council-manager plan. Twelve other cities attempted unsuccessfully to adopt new charters.[12] There was no new charter activity in the remaining twenty-two cities.

Broadly defined, the dependent variable to be examined is municipal reform or change. As indicated earlier, the usual and inappropriate distinction between reform and non-reform government is not used. Thus, both the new options, council-manager or mayor council, are considered innovative or representative of political change, while the commission plan only represents the status quo.

The setting of this study approaches a quasi-experimental situation. All forty-three cities function within the constraints imposed by Pennsylvania law. Each city was provided with the same option, at the same time, to consider new charters. Furthermore, the controlled time period during which the cities were examined minimizes the effects of demographic or other environmental fluctuations. Control of these factors permits a more accurate assessment of the environmental and political influences on local public policy.

III. RESEARCH DESIGN

The purpose of this study is to examine those variables which are related to the consideration and adoption of new political forms. In a broader perspective, the analysis deals with the correlates of political change on the local level. The general hypothesis is that variations in the social, economic and political characteristics of communities are associated with the decision to alter or retain existing forms of government. The forty-three cities selected for the study are treated as a statistical population of third-class cities in Pennsylvania.[13]

The study attempts to answer two questions: (1) What are the correlates of charter-reform activity in the forty-three cities? (2) Among those cities which were active, what variables are associated with the outcomes of this activity? Twelve independent variables were selected for analysis because of their prominence in the local and state policy literature (see Table 1).[14] Aside from the familiar variables (1–8), variables 9–11 require some further clarification below:

TABLE 1.—INDEPENDENT VARIABLES

1. Population, 1960 (10^3).
2. Median gross rentals, 1960.
3. Real estate values, 1960 (per capita).
4. Median income, 1960 (10^2).
5. Median school years completed by the adult population, 1960.
6. Percentage white collar, 1960.
7. Percentage nonwhite, 1960.
8. Percentage of the native population of foreign or mixed parentage, 1960.
9. Metropolitan classification (1960).
10. Employment-residence ratios (1960).
11. Percentage absentee-owned industry (1965).
12. Local interparty competition (1955–1966).

Metropolitan Classification. The cities are classified as central, independent, or suburban cities. A central city is the largest city of 50,000 or more, located within a Standard Metropolitan Statistical Area (SMSA). Suburbs are cities above 10,000 located within SMSA's. Independent cities are cities above 10,000 located outside SMSA's.[15] This classification is treated as a dichotomous variable

recommend retention of the existing commission plan. A recommendation for a new charter was then placed before the people in a referendum.

[11] Other structural variables usually considered in studies such as this were not relevant. Pennsylvania law requires partisan, at-large elections in all third-class cities.

[12] One of these cities, at the time of this writing had not had the opportunity to vote. It was coded as an "active" rather than a new charter city.

[13] The 1960 census populations of these cities range from 10,667 to 138,440. Five cities were excluded from the study because they have dropped below the legal population minimum of 10,000 required for third-class cities.

[14] Variables 1 through 10 were taken from *The Municipal Yearbook 1963* (Chicago: The International City Manager's Association, 1963) and *The County and City Data Book 1962* (Washington: U. S. Department of Commerce, Bureau of Census, 1962). All data are taken from the 1960 census.

[15] *The Municipal Yearbook 1963*, p. 97.

comparing central and independent cities to suburbs.

Employment-Residence Ratios. This is the ratio of total employment to resident employment. It measures the extent to which a city is dependent upon a nonresident labor force.[16]

Percentage Absentee-Owned Industry. The degree of absentee-owned industry is measured by the percentage of the total local work force employed by absentee-owned firms. The location of the firm's home office was used to determine whether the firm is absentee or locally owned, i.e., if the home office is located outside the city, it is considered to be an absentee-owned firm.[17]

Local Interparty Competition. This variable is defined as the relative percentage of city elective offices held over the twelve year time period by the minority party. This was determined by counsulting *The Pennsylvania Manual* for the years included in the study and recording for each two year period the number of offices held by both parties.[18] These numbers were then totaled for the twelve year period and the percentage of local offices held by the minority party was calculated for each city. The index ranges from 0 (least competitive) to 50 percent (most competitive) where control was equally divided between the parties.

The dependent variable—charter reform activity—is treated as a dichotomous variable, i.e., reform activity is dichotomized on the basis of its presence or absence in each city. The minimal condition required for a city to be classified as "active" was that it had, either by petition or ordinance, placed the question of whether or not to elect a charter study commission before the local electorate.[19]

Two methods were used to explore the second question. First, results were classified in terms of success, i.e., either adoption or rejection of the charter proposals, and operationalized as a dichotomous variable.[20] Second, the percentage of the popular vote approving the proposal was used as the dependent variable in the twenty-two cases where the public did have an opportunity to vote on the issue.[21]

Four political process variables—type of proposal, city hall response, fear of the higher costs of reform government and voter turnout—were included in the analysis. These variables are commonly considered in the case study literature on municipal reform and metropolitan reorganization,[22] but they are not often included systematically in comparative studies. They are treated in this analysis as dichotomous variables.[23]

As stated above, the twenty-two charter referenda occurred over the years from 1957 to 1966. Because of this extended time period, newspaper accounts seemed to offer the most reliable and comprehensive information concerning the campaigns. Local newspapers, in each of the cities in which the referenda occurred, were examined for the time period beginning with the ordinance or petition leading to the election of a charter study commission through to the date the referendum was held.[24]

The four process variables were defined as follows:

1. *Type of proposal.* This was defined and

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[16] *Ibid.*, p. 92.

[17] The employment information and home office location were taken from the *Pennsylvania Industrial Directory 1965* (Harrisburg, Pennsylvania: Department of Internal Affairs, 1965).

[18] "Lists of Public Officials" (Harrisburg, Pennsylvania: Department of Property and Supplies), Vols. 92–97.

[19] Twenty-one of the forty-three cities were defined as active.

[20] Rejection was defined to include, in addition to electoral defeat, the failure of a charter study commission either to submit a recommendation or to recommend retention of the commission plan. One of these conditions was met in twenty-five cases involving twenty cities. Five cities experienced two campaigns. In the analysis, the second effort in these five cities was treated as a separate event.

[21] The twenty-two referenda occurred in nineteen cities. Again, the second referendum in three of the cities was treated as a separate event with conditional changes treated as new information.

[22] For example, see David A. Booth, *Metropolitics: The Nashville Consolidation* (East Lansing, Michigan: Michigan State University, Institute for Community Development, 1963), pp. 37–56; and Brett Hawkins, "Public Opinion and Metropolitan Reorganization in Nashville," *Journal of Politics*, 28 (May, 1966), 408–418.

[23] The risks involved in dichotomizing such complex variables are recognized and accepted in view of the new information which may be revealed about the interrelationship of these variables and the standard environmental indicators in determining policy outcomes.

[24] The duration of reform activity usually ranged from a year to eighteen months. Additional information on some campaigns was found in published and unpublished documents made available by the Bureau of Research and Information of the Pennsylvania Department of Community Affairs, Harrisburg, Pennsylvania.

coded simply as either a mayor-council (1) or council-manager (0) plan.

2. *City hall response.* This variable was dichotomized as opposition (0) or lack of opposition (1) to the proposal. Opposition was defined subjectively as any number of councilmen or city employees who, actively and overtly, opposed the charter reform proposal to a degree, sufficient in itself, to keep the issue consistently before the public. In those cities where city hall was divided on the issue, the dominant response, in terms of publicity, was coded.
3. *Cost issue.* This variable was defined as either the presence (0) or absence (1) of a fear of higher costs under a reform government, as reflected in newspaper accounts and political advertisements.
4. *Voter turnout.* This variable was included in the analysis in a more conventional manner.[25] Voter turnout was defined as the percentage of the total registered voters in each community who voted on the local charter referendum.

Simple, partial and multiple correlations are used to describe the relationships. In addition, cumulative coefficients of determination are reported to indicate the relative variation explained by each independent variable.

IV. REFORM AND SOCIO-ECONOMIC INDICATORS: SOME PROPOSITIONS

There is a substantial body of literature which discusses the environmental correlates of municipal reform.[26] No attempt will be made here to discuss that literature. Rather, only the major conclusions will be summarized in the form of propositions:[27]

1. *Population size.* Larger cities are more likely to consider and adopt new charters than smaller cities (Alford and Scoble; Kessel).
2. *Social Class.* Cities with larger middle-class populations—as measured by occupation, education and income—are more likely to consider and adopt new charters (Banfield and Wilson; Schnore; Alford and Scoble; Kessel).
3. *Social Heterogeneity.* Cities with larger nonwhite and ethnic populations are less likely to consider and adopt new charters (Banfield and Wilson; Kessel; Schnore and Alford; and Alford and Scoble).
4. *Economic Potential.* Cities with greater economic potential—as measured by per capita market value of real estate, median gross rentals and larger employment residence ratios—are more likely to consider and adopt new charters (Williams and Adrian; Sherbenou).
5. *Absentee-owned industries.* Cities with greater proportions of the work force employed

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[25] For an analysis of the correlates of voter turnout, see, Robert R. Alford and Eugene C. Lee, "Voting Turnout in American Cities," this REVIEW, 62 (September, 1968), 796–813.

[26] Examples of such studies are: Edgar L. Sherbenou, "Class Participation, and the Council-Manager Plan," *Public Administration Review*, 21 (Summer, 1961), 131–135; John H. Kessel, "Government Structure and Environment: A Statistical Note about American Cities," this REVIEW, 56 (September, 1962), 615–620; Oliver P. Williams and Charles R. Adrian, *Four Cities: A Study in Comparative Policy Making* (Philadelphia: University of Pennsylvania Press, 1963), pp. 287–288; Leo F. Schnore, *The Urban Scene: Human Ecology and Demography* (New York: The Free Press, 1965) pp. 184–199; Robert R. Alford and Harry M. Scoble, "Political and Socioeconomic Characteristics of American Cities," *The Municipal Yearbook 1965* (Chicago: The International City Manager's Association), pp. 82–97; Charles S. Liebman, "Functional Differentiation and Political Characteristics of Suburbs," *Americal Journal of Sociology*, 66 (March 1961), 485–490; Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," this REVIEW 60 (June, 1966), 306–326; Thomas R. Dye, "Urban Political Integration: Conditions Associated with Annexation in American Cities," *Midwest Journal of Political Science*, 8 (November, 1964), 430–446; Thomas R. Dye, Charles S. Liebman, Oliver P. Williams and Harold Herman, "Differentiation and Cooperation in a Metropolitan Area," *Midwest Journal of Political Science*, 7 (May, 1963), 145–155; and Amos H. Hawley, "Community Power and Urban Renewal Success," *American Journal of Sociology*, 68 (January, 1963) 422–431; and Leo F. Schnore and Robert R. Alford, "Forms of Government and Socio-Economic Characteristics of Suburbs," *Administrative Science Quarterly*, 8 (June, 1963), 1–17.

[27] The propositions listed are logically derived from the studies indicated in footnote 26, *supra*. These propositions are not empirically derived because most of this literature deals specifically with government structure rather than political change. Thus, the results of the examination of these propositions do not bear directly on the findings from which they were derived.

in absentee-owned firms are less likely to consider and adopt new charters[28] (Williams and Adrian, Kessel).

6. *Metropolitan classification.* Central and independent cities are more likely to consider and adopt new charters than suburban cities. This proposition is drawn from Schnore's finding that the commission plan is more common in "older, less modernized" suburbs where many problems are not recognized or acknowledged.[29] Third-class suburban cities in Pennsylvania tend to fit this description. It may be also that the status quo or "caretaker" orientation of some suburbs is a result of the suburban dependence on central city resources and services.[30]
7. *Interparty competition.* Cities with more competitive party structures are more likely to engage in reform activity but less likely to adopt new charters. There is a scarcity of research on local party competition from which to draw hypotheses. Proposition 7 is based on the suspicion that challenges to the controlling party, such as structural changes, are more likely in competitive cities because of the minority party's efforts to exploit any opportunity to weaken the control of the majority party. At the same time, it is doubtful that a weak minority party could mobilize sufficient support to carry a proposal which was opposed by the majority.

V. REFORM ACTIVITY AND ENVIRONMENTAL INDICATORS: SOME FINDINGS

A step-wise correlation program was used to analyze these data.[31] In Table 2, the independent variables are ranked in terms of the relative variance produced in the dependent variable—reform *activity*. It is clear that metropolitan class and population size are the most important environmental correlates of reform activity. The remaining ten variables, in combination, contribute only an additional 22 percent to the explained variance in the dependent variable. It appears that the effects of social class and heterogeneity, economic potential, and absentee ownership are marginal in determining the presence or absence of charter reform activity.

Although the total variation explained by the environment is not high (55 percent), metropolitan classification and population size

TABLE 2.—CUMULATIVE MULTIPLE CORRELATIONS BETWEEN SELECTED ENVIRONMENTAL VARIABLES AND REFORM ACTIVITY IN FORTY-THREE PENNSYLVANIA CITIES

Environmental Variables	Activity Index		
	Multiple R	Multiple R^2	Increase in R^2
Metropolitan Class	.48	.23	.23
Population Size	.58	.33	.10
All Variables*	.74	.55	.22

Note: In this program, variables are ranked in terms of their overall explanatory power (i.e., the variance produced in the dependent variable). If a variable is unrelated to the dependent variable, it is excluded from the equation. When a number of independent variables are not truly independent, i.e., when they are highly intercorrelated, the importance of one or more of these variables may be minimized. For example, if variables A, B, and C are highly intercorrelated and their effects on variable D are being analyzed, the computer may rank variable A highly and as a result, deemphasize the importance of variables B and C. This would result because B and C did not contribute a sufficient additional increment to the variance explained by A. For a more detailed explanation, see Hubert M. Blalock, *Social Statistics* (New York: McGraw-Hill, 1960), pp. 346–351.

* Variables which independently account for less than a 7 percent increase in R^2.

[28] See also, Ernst A. Barth, "Community Influence Systems: Structure and Change," *Social Forces*, 40 (October, 1961), "Absentee-Owned Corporations and Community Power Structure," *American Journal of Sociology*, 61 (March, 1956), 413–419; Robert O. Schulze, "The Role of Economic Dominants in Community Power Structure," *American Sociology Review*, 23 (February, 1958), 3–9.

[29] Schnore, *op. cit.*, p. 190.

[30] See for example, Amos Hawley, "Metropolitan Populations and Municipal Expenditures in Central Cities," *Journal of Social Issues*, 7 (1951), 100–108. The reference to "caretaker" government is taken from the Williams and Adrian typology, *loc. cit.*

[31] For a brief explanation, see Note, Table 2. A complete description of the program is available in W. J. Dixon (ed.), *Biomedical Computer Programs* (Los Angeles: Health Sciences Computing Facility, Department of Preventive Medicine and Public Health, School of Medicine, University of California, 1964), pp. 49–59.

TABLE 3.—CUMULATIVE MULTIPLE CORRELATIONS BETWEEN SELECTED ENVIRONMENTAL VARIABLES AND REFORM OUTCOMES IN TWENTY-FIVE ATTEMPTS*

Environmental Variables	Outcomes (Passage-Defeat)		
	Multiple R	Multiple R^2	Increase in R^2
Rentals	.47	.22	.22
Metropolitan class	.63	.40	.18
Absentee-Ownership	.73	.53	.13
Real estate values	.78	.60	.07
All Variables**	.86	.74	.14

* See footnote 20.

** Variables which independently account for less than a 7 percent increase in R^2.

together account for 33 percent of the total variation. This finding provides some support for Propositions 1 and 6, which, when combined, might be termed a stress-dependence hypothesis. That is, larger cities may generate greater demands for services which are not easily handled in the rather decentralized administrative structure inherent in commission governments. The resulting stress may provide at least the inclination to consider structural changes. Further, as indicated above, the dependence of suburban cities on central city resources may result in a reduction in demands placed on suburban governments and a corresponding status quo orientation.[32]

VI. ENVIRONMENT INDICATORS AND REFORM OUTCOMES: SOME FINDINGS

Attention is now directed to this question: once political change is formally considered, as reflected in the activity index, what are the environmental correlates of the results of this consideration (i.e., the variables associated with passage or defeat)? The results reported in Table 3 reveal that the relationship between outcomes and metropolitan class is consistent with the earlier findings, but the explanatory importance of population size has diminished. A possible explanation is that this is a consequence of the fact that larger cities tended to be active while smaller cities remained inactive (i.e., larger cities were more likely to attempt to exercise their new charter options than smaller cities). In this measure, where only active cities are considered, the variation in population size is not as great.

The effects of absentee-ownership require some explanation. The relationship is negative.[33] It appears that there is a tendency to reject new charter proposals in cities where a larger proportion of the local economy is controlled by absentee-owned firms.[34] This finding is consistent with Proposition 5. Further, there is evidence of support for Proposition 4. Economic potential, as reflected in rentals and real estate values, appears to be a factor of some importance. New charter proposals tend to have a higher probability of success in cities with greater economic potential.

The most important conclusion which can be drawn at this point is that central and independent cities with greater economic potential and lower levels of absentee-ownership are more likely to adopt new charters. It is interesting to note that neither the social indicators nor interparty competition (Propositions 2, 3 and 7) is associated with reformism as it is measured in two separate indices.

VII. ENVIRONMENTAL AND POLITICAL INDICATORS AND REFERENDA OUTCOMES: SOME PROPOSITIONS AND FINDINGS

Thus far, interparty competition was the only explicitly political variable included in the analysis.[35] Now the four additional political process variables are introduced in the analysis of twenty-two charter referenda: (1) type of pro-

[32] A further justification for this explanation is that population size is not strongly associated with any other environmental variable, and is only marginally associated with metropolitan classification ($r = .33$). It is possible that ethnicity ($r = -.49$) and race (percentage nonwhite, $r = -.33$) are exerting an indirect influence through metropolitan classification. These correlations indicate that there is a tendency for the suburban cities to have larger ethnic and nonwhite populations. But, the correlations between both these variables and the activity index are low, although directionally consistent ($r_e = .29$) ($r_{nw} = .25$), and they proved to be rather inconsequential in the step-wise analysis.

[33] The partial correlation coefficient between absentee-ownership and activity outcomes is $-.45$.

[34] Recall that rejection here does not necessarily mean electoral defeat, but any of the means of rejection indicated earlier, *supra*, footnote 20.

[35] Incomplete newspaper coverage is the reason other political variables were not included. Newspaper coverage improved greatly after it was established that a referendum would be held.

posal, (2) city hall response, (3) fear of excessive costs, and (4) voter turnout.[36] The dependent variable in this case is the percentage of the popular vote approving the new charter proposal.

The textbook literature on urban politics provides an abundant supply of insights into the politics of municipal reform.[37] In brief, it is suggested that council-manager proposals are usually opposed by incumbent city administrations and local politicians. This opposition often has its basis in the understandable reluctance to sacrifice or relinquish political influence (e.g., patronage) to a professional manager. Perhaps the most common criticism of the manager plan is that it costs too much. Opponents of manager proposals often exploit the public's fear of excessive costs during reform campaigns. If this fear becomes a salient issue for the electorate, it is probable that it will be reflected in a higher voter turnout comprised of a disproportionately large number of negative voters.[38]

The following propositions are drawn from this literature:

8. City hall opposition is associated with council-manager proposals.
9. The fear of higher costs of reform government is associated with council-manager proposals and city hall opposition.
10. Higher voter turnouts are associated with council-manager proposals, city hall opposition, and the fear of higher costs.
11. Referendum defeat is associated with council-manager proposals, city hall opposition, fear of excessive costs and larger voter turnouts.

Beyond examining these specific propositions, the overriding question raised here involves the relative importance of political process variables when they are evaluated simultaneously with the environmental set.

TABLE 4.—SIMPLE CORRELATIONS AMONG POLITICAL VARIABLES IN TWENTY-TWO CAMPAIGNS

	City Hall Response	Type of Proposal	Cost Issue	Percent Voter Turnout
City Hall Response (1 =support, 0 =opposition)	—	.72	.83	−.53
Type of Proposal (1 =mayor, 0 =manager)		—	.76	−.56
Cost Issue (1 =No, 0 =Yes)				−.57

Propositions 8, 9 and 10 are examined in Table 4. The substantial association between these variables tends to confirm the hypothesized relationships, if one agrees with a logical sequence of events. That is, when council-manager proposals were offered, in most cases city hall responded negatively. Usually this opposition was mobilized in counter-campaigns to dramatize the alleged excessive costs of council-manager government. Alerted to the alleged financial burden council-manager government would create, taxpayers expressed their fears by turning out in larger numbers to vote against the proposal. It appears that if local public officials oppose a charter proposal and subsequently provide a nucleus for resistance, opposition in the community at large is more likely to develop.[39]

With reference to Proposition 10, larger voter turnouts are clearly associated with council-manager proposals, city hall opposition and the fear of excessive costs of reform government. Conversely, smaller voter turnouts are associated with mayor-council proposals, city hall support, or at least lack of opposition, and the absence of the cost issue.

Proposition 11 is considered in the final phase of the analysis, when both environmental and political variables are combined to assess their relative importance as correlates of referenda outcomes. The results reported in Table 5 provide evidence of the rather striking relative im-

[36] Recall again that "reform outcomes" (passage defeat) refer to the twenty-five cases in which decisions were made either by the charter commission, (e.g., recommendation to retain the commission plan) or in a referendum. At this point, *only* referenda decisions (defined as the percentage of the vote for the proposal) are considered.

[37] See, for example, Adrian, *op. cit.*, ch. 8; and Edward C. Banfield and James Q. Wilson, *City Politics, op. cit.*, ch. 13.

[38] A study of fluoridation referenda indicates that higher voter turnout is associated with the defeat of these referenda. See Maurice Pinard, "Structural Attachments and Political Support in Urban Politics: The Case of Fluoridation Referendums," *American Journal of Sociology*, 68 (March, 1963), 513–526.

[39] This is consistent with the findings that the position of the mayor is closely associated with the outcomes of fluoridation referenda. See David B. Rosenthal and Robert L. Crain, "Executive Leadership and Community Innovation: The Fluoridation Experience," *Urban Affairs Quarterly*, 1 (March, 1966), 39–57.

TABLE 5.—CUMULATIVE MULTIPLE CORRELATIONS BETWEEN SELECTED ENVIRONMENTAL AND POLITICAL VARIABLES AND THE PERCENTAGE POSITIVE VOTE IN TWENTY-TWO REFERENDA

Environmental and Political Variables	Percentage Positive Vote		
	Multiple R	Multiple R^2	Increase in R^2
Cost Issue	.69	.47	.47
Population Size	.76	.57	.10
Type of Proposal	.82	.68	.11*
Voter Turnout	.87	.75	.08
All Variables**	.98	.96	.21

* The apparent non-sequential ordering is due to rounding. The difference in R^2 between the second and third variables is .0033.

** Variables which independently account for less than a 7 percent increase in R^2.

TABLE 6.—SIMPLE CORRELATIONS BETWEEN ENVIRONMENTAL AND POLITICAL VARIABLES IN TWENTY-TWO REFERENDA

Environment	Type of Proposal	City Hall Response	Cost Issue	Voter Turnout
Rentals	.27	.42	.41	−.53
Income	.31	.20	.18	−.52
Real estate values	.35	.14	.33	−.43
Employment/residence ratios	.07	.33	.49	−.39
Nonwhite	.48	.43	.29	−.32
Party competition	−.03	−.19	−.20	.20
Metro class	.17	.18	.22	.17
Absentee-ownership	.19	−.11	−.14	−.16
Education	−.13	.06	.11	−.13
Ethnicity	−.17	−.25	−.02	.07
White collar	−.28	.00	−.03	−.07
Population	.29	−.01	.11	−.03

portance of political process variables. Four of the sixteen variables included in the analysis, three political process variables and population size, account for 75 percent of the variation in the percentage positive vote, i.e., the percentage of votes cast *for* the adoption of new charters.

Several conclusions can be drawn from these results. Of greatest significance, is the relative strength of the political process variables. With the exception of population size (a positive relationship), the explanatory capacity of the environmental set of variables appears to be quite limited. Again, the weakness of association between the dependent variable and indicators of social class, social heterogeneity and interparty competition is noteworthy in view of the importance accorded these variables in the literature. Beyond this, it appears safe to conclude that there is some empirical support for Proposition 11.

With reference to Proposition 11, perhaps some further clarification is due. A larger positive vote on the charter question is associated with mayor-council proposals, absence of the cost issue, and lower voter turnout.[40] The reduced importance of city hall response as an independent variable is a function of the statistical fact that this variable is highly correlated with two of the other process variables, viz., type of proposal and the cost issue (see Table 4). In the case of three such variables in a multiple correlation equation, the explanatory overlap is considerable (see Note, Table 2).

VIII. ENVIRONMENTAL AND POLITICAL VARIABLES: A RELATIVE ASSESSMENT

Thus far, an attempt has been made to determine the correlates of variously defined dependent variables which measure different dimensions of structural change or reformism. The analysis has shown that, in general, the relative importance of environmental variables declined when political process variables were considered simultaneously in the analysis of referenda outcomes. Population size was the single exception.

At the conclusion of their study, Lineberry and Fowler suggest that "... political institutions 'filter' the process of converting [socio-economic] inputs into [political] outputs."[41] They hypothesize that political variables perform a similar "filter" or linkage function. If the Lineberry-Fowler model is valid, one would expect to find a convincing relationship between the environmental set and the political process variables. Table 6 shows that there is such a relationship between a subset of the environmental set and the political variables. More specifically, indicators of economic potential (rentals, income, real estate values, and employment-residence ratios) and race (percentage nonwhite) are most closely associated with the political variables. Cities with greater economic potential and larger nonwhite populations are more likely to receive and vote on mayor-council proposals. Consequently, city officials are less likely to oppose these proposals and the cost issue remains dormant. This combination of events results in

[40] The simple correlation between voter turnout and the percentage positive vote is negative, $r = -.53$.

[41] *Op. cit.*, p. 715.

lower voter turnouts and greater acceptance potentials for reform proposals.

There is some evidence, reported earlier, that reform activity is less likely to occur in more racially diverse cities, viz., suburbs.[42] However, this evidence must now be qualified by adding that when reform activity does occur, the size of the nonwhite minority is positively associated with mayor-council proposals which have a greater probability of acceptance. It may be that economically viable and racially diverse cities find mayor-council government more compatible with a need to provide an arena for the arbitration of potentially conflicting interests.

Population size and metropolitan class, which were shown earlier to have some determinant effect on reform activity and outcomes (See Tables 2, 3 and 5), are not related in any meaningful way to the political variables. A plausible interpretation of this, noted earlier, is that population size and metropolitan class exert an independent influence on reformism.[43] Larger central and independent cities are more likely to consider and adopt reform policies simply as a result of a sheer physical need for administrative innovation. In short, their size, relative proximity to parasitic suburbs, or as is the case with independent cities, their relative geographical isolation may provide sufficient pressures for structural change or administrative innovation regardless of other social, economic, and political variables. The remaining environmental indicators appear to be only tenuously related to the political variables.

IX. SUMMARY AND CONCLUSIONS

An important feature of this study is its quasi-experimental design. Both the time sequence and legal constraints are uniform for each city included in the analysis. Thus, any distortion, which is quite probable when these factors are not controlled, has been eliminated.

The findings reported are generally consistent with the linkage model hypothesized by Lineberry and Fowler.[44] The analysis has shown that some of the political variables recognized in the Lineberry and Fowler model —viz., structure of party systems (interparty competition), patterns of aggregation (type of proposal and cost issue), strength of interest groups (city hall response) and voter turnout— were, with the exception of interparty competition, important determinants of referenda outcomes. Beyond this, the relative importance of environmental variables declined when these political variables were considered simultaneously in the analysis. There is some evidence that these political variables function as intervening variables linking a subset of the environmental indicators to referenda outcomes. Finally, some standard social indicators, viz., education, ethnicity, and occupation, were shown to be conspicuously weak in their association with reformism throughout the analysis.

These findings suggest the importance of political process variables in comparative urban research. On the surface, it may seem that this finding is contrary to those of a number of studies which have indicated the limited influence of political variables on policy outputs at both the state and local levels.[45] A possible explanation for these somewhat divergent findings is that previous policy output research has been limited almost exclusively to analyses of revenue and expenditure policies which, quite plausibly, would be associated with variables reflecting the economic potential or the tax base of states and localities. However, the relationships which exist between these socio-economic variables and policies which represent explicitly political values (e.g., candidate preferences or essentially noneconomic issues such as fluoridation and municipal reform) may not be as direct or important as they are in those policies which must be made within the parameters set by the economic potential of the state. It is probable, and certainly worthy of further research, that the explanatory importance of socio-economic and political process variables will vary with the type of policy being considered. When noneconomic policies are considered, a stronger association is revealed between political process variables and, in this case, referenda outcomes. These process variables reflect the attitudinal and behavioral dimensions of city politics to a greater degree than the socio-economic variables.

[42] See footnote 32, *supra*.

[43] *Supra*, p. 1177–78.

[44] *Loc. cit.*

[45] See, for example, Herbert Jacob, "The Consequence of Malapportionment: A Note of Caution," *Social Forces*, 43 (December, 1964), 256–261; Thomas R. Dye, "Malapportionment and Public Policy in the States," *Journal of Politics*, 27 (August, 1965), 586–601 and *Politics, Economics and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966); Richard I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," this REVIEW, 60 (March, 1966), 73–82; and Jacob and Lipsky, *op. cit.*, 511–517.

Another explanation for these differences is that perhaps too much consideration is being given to the relative availability of political data rather than the theoretical relevance of these data to the problem being examined. The result is that political variables are usually defined operationally in structural rather than behavioral or interactional terms. To this extent, the behavioral dimension of politics is being ignored, not assessed, in the policy output studies and the results may be simply a product of the methodology.

What this analysis has demonstrated is not that environmental variables are unimportant, but rather that their importance must be assessed in combination with relevant and meaningful political variables; that is, political process variables which are often recognized but rarely included in comparative urban research. Despite the reservations one may have about the validity of dichotomous variables in defining complex political phenomena, the costs of excluding such variables may far exceed the satisfaction which may be derived from more rigorously defined, more readily accessible but inconsequential indicators. In short, an effort must be made to include those variables, which have been shown in the case study literature to have theoretical and political relevance, into aggregate comparative studies. The theoretical limitations of case study analysis are well-known. More attention should now be directed to the explanatory limitations of simple input-output analysis in which no attempt is made to determine the independent and intervening effects of political process variables.

"ECOLOGICAL" INFERENCE: THE USE OF AGGREGATE DATA TO STUDY INDIVIDUALS*

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Because they are inexpensive and easy to obtain, because they may be available under circumstances in which survey data are unavailable, and because they eliminate many of the measurement problems of survey research, data on geographic units such as counties or census tracts are often used by political scientists to measure individual behavior. This has involved us in the long-standing problem of inferring individual-level relationships from aggregate data, which was first raised by W. S. Robinson in the early nineteen fifties.[1]

In this paper, I shall first discuss the problem raised by Robinson. I shall then review three partial solutions to the problem—the Duncan-Davis method of setting limits, Blalock's version of ecological regression, and Goodman's version of ecological regression.[2] Finally, I shall propose some ways in which Goodman's method may be used so as to reduce the problem of bias in its estimates, and make it a more reasonable tool for reserch.

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[1] W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, 15 (June, 1950), 351–357.

[2] O. Dudley Duncan and Beverly Davis, "An Alternative to Ecological Correlation," *American Sociological Review*, 18 (December, 1953), 665–666; Hubert M. Blalock, *Causal Inference in Non-Experimental Research* (Chapel Hill: University of North Carolina Press, 1965), ch. 4; Leo A. Goodman, "Some Alternatives to Ecological Correlation," *American Journal of Sociology*, 64 (May, 1959), 610–625. See also Duncan, Cozzart, and Duncan, *Statistical Georgaphy* (Glencoe, Ill.: The Free Press, 1961), pp. 60–80; Gudmund R. Iversen, *Estimation of Cell Entries in Contingency Tables When Only Margins Are Observed* (Ph.D. Dissertation, Department of Statistics, Harvard University, 1969), and Donald E. Stokes, "Ecological Regression as a Game With Nature" (unpublished ms.).

I. THE PROBLEM OF ECOLOGICAL INFERENCE

Our difficulty, as Robinson showed, is that we cannot necessarily infer the correlation between variables, taking people as the unit of analysis, on the basis of correlations between the same variables based on groups of people as units. For example, the "ecological" correlation between per cent black and per cent illiterate is +0.946, whereas the correlation between color and illiteracy among individuals is only +0.203. Similarly, the "ecological" correlation between per cent of the population foreign-born and per cent illiterate is −0.618, although the individual-level correlation between these variables is +0.118!

As several writers pointed out in response to Robinson, not all correlations between variables measured at the community level are intended to lead to inferences about individuals.[3] Often communities themselves are the units which we wish to study; and when this is the case, we will wish to find relationships between community-level variables. Many recent studies using data measured at the aggregate level have dealt with true community-level variables, but problematic "ecological" correlations continue to be used.[4]

[3] Among others, Herbert Menzel, "Comment on Robinson's 'Ecological Correlations and the Behavior of Individuals,' " *American Sociological Review*, 15 (October, 1950), 674–675, and Austin Ranney, "The Utility and Limitations of Aggregate Data in the Study of Electoral Behavior," in Austin Ranney (ed.), *Essays on the Behavioral Study of Politics* (Urbana, Ill.: University of Illinois Press, 1962), pp. 91–102.

[4] Studies using community-level variables, but not requiring ecological inferences, are too numerous to list. Two examples are Donald R. Matthews and James W. Prothro, *Negroes and the New Southern Politics*, (New York: Harcourt, Brace, & World, 1966), pp. 101–175; and Robert L. Crain and Donald B. Rosenthal, "Community Status as a Dimension of Local Decision-Making," *American Sociological Review*, 32 (December, 1967), 970–984. Some recent studies which use aggregate correlations to infer individual-level relationships are Duncan MacRae, Jr., and James A. Meldrum, "Critical Elections in Illinois: 1888-1958," this REVIEW, 54 (September,

Before we discuss the use of aggregate data to get at individual-level variables, we should clarify just how we know that a variable is an "individual" one or a "community" one. The percentage of votes for the Democrats in a county may be an individual-level variable measured at the aggregate level, if we relate it to percentage of manual workers in order to find out how manual workers vote Democratic. But it may also be a community-level variable, if we relate the percentage of the vote Democratic in districts to Congressmen's voting records in the house. The distinction lies in our intent in using the variable. In the first example we wish to use "per cent Democratic" to get at a characteristic of individuals; in the second, we wish to use it to get at a characteristic of the district.

I stress this point only because there has been some confusion as to whether, and under what circumstances, voting data (the most frequently used "ecological" variable) are a community variable or an individual one. Austin Ranney, in his discussion of the utility of aggregate voting statistics, suggests that it is valid to correlate voting results of counties, states, and so on, if we do not consider ourselves to be studying individual voting patterns, but rather the behavior of electorates.[5] We can study such phenomena as critical elections, voting change, and the social basis of the vote by treating the number of votes for a party or the number of working-class as community-level variables.

Accordingly, if aggregate data studies carefully and thoroughly identify and describe recurring patterns of preference and turnout characteristic of particular electorates over time and, by ecological correlations, relate those patterns to other traits of the electorates and their environments, they can be valuable allies to the sample surveys in the investigation of electoral behavior.[6]

It seems to me that this approach begs the question. The county or ward "electorate" is not a meaningful entity whose characteristics we normally wish to study. It is an accidental aggregation of voters whose boundaries are of no theoretical interest to us in studies such as Ranney describes. A student of presidential or state elections can use county or ward data interchangeably. In fact, it would not affect the basis of his work if the aggregations, instead of wards or counties, consisted of all adults in the county whose last name began with Aba . . . , Abe . . . , Abo . . . , and so on through the alphabet. He could still take these aggregations of people with similar names and correlate the proportion of working class in the group with the proportion of Democratic voters in the group, or do any of the usual "ecological" correlations for which voting data are frequently used. If it were truly the ward "electorates" that he wanted to study, he could not use the other kinds of data at all.

I am not arguing here that voting data can never be a true community-level variable; I used it above as an example of one. But unless the theory with which we are working conceives of the aggregations we are using as real entities, *for which no other type of aggregation can readily be substituted*, we mislead ourselves if we use ecological correlations.

This brings us back to the original problem: How can we study relationships at the individual level when the data available to us are based on aggregations of individuals?

II. ECOLOGICAL INFERENCE: THE DUNCAN-DAVIS TECHNIQUE

Duncan and Davis point out that if we calculate the maximum strength of a relationship at the individual level which would be consistent with the aggregate marginals, and also calculate the minimum strength, then we have limits within which the true relationship must lie. Without making any assumptions about individuals in the aggregation, we set unimpeachable limits around the true value of the individual-level relationship.

The method works in the following way. Suppose we wish to find the relationship between religion and social class (both dichotomized) in a city. In ward A, we find that the population includes 10,000 working-class and 1,000 middle-class; 8,000 Catholics and 3,000 non-Catholics. Since there are only 1,000 middle-class in the ward, at least 7,000 of the Catholics must be working-class. Thus a minimum of 7,000 and a maximum of 8,000 Catholics are working-class. Similarly, a minimum of 2,000 non-Catholics, and a maximum of 3,000 are working-class.

Repeating this analysis for each ward in the

1960), 669–684; James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," this REVIEW, 58 (December, 1964), 876–888; Gerald Pomper, "Classification of Presidential Elections," *Journal of Politics*, 29 (August, 1967), 535–567; and Karl O'Lessker, "Who Voted For Hitler?", *American Journal of Sociology*, 74 (July, 1968), 63–69.

[5] Ranney, *op. cit.*, 99–101. Pomper, for example, uses this argument to help justify his use of ecological correlations. Pomper, *op cit.*, 556.

[6] Ranney, *op. cit.*, 100.

city, we can then add all the minima together, and all the maxima. The results might read like this:

	minimum	*maximum*
Catholic working-class	110,000	140,000
Catholic middle-class	20,000	50,000
Non-Catholic working-class	70,000	100,000
Non-Catholic middle-class	110,000	140,000

This means the relationship lies somewhere between those shown in A and B:

A. (Minimum relationship)

	Catholic	*Non-Catholic*
Working-class	110,000	100,000
Middle-class	50,000	110,000

B. (Maximum relationship)

	Catholic	*Non-Catholic*
Working-class	140,000	70,000
Middle-class	20,000	140,000

These are absolute limits, not estimates; it is impossible for the individual-level relationship to be higher or lower than these limits.

The great advantage of the Duncan-Davis method is that it requires no assumption about the distribution of individuals in aggregations. The main difficulty with it is that frequently the limits which it can specify are so far apart that we are not much better off than we were before we began the analysis.

We achieve the narrowest limits when the variables we examine are irregularly distributed among the aggregate units. If in the example above the proportion of Catholics does not vary considerably among the wards, we cannot delimit the individual relationship greatly. To take the two extremes, where all wards have the same proportion of Catholics and of working-class, looking at the wards does not give us narrower limits than those we would get by looking at the whole city as a unit. But if all wards are either entirely Catholic or entirely non-Catholic (that is, proportion Catholic is at its maximum variablity) the Duncan-Davis method will in fact give us a single value, which is the true relationship; in this situation the limits simply close together. Many variables of interest to social scientists, such as sex and age, tend to be rather evenly distributed geographically. Others, such as race, tend to be sharply clustered, and will be more amenable to the Duncan-Davis method.

A few examples illustrate the promise, and limitations, of the method. Using census tracts in Chicago, Duncan and Davis are able to show (a) that the correlation among females between race and being a domestic servant falls between +0.165 and +0.317, (b) that the correlation between race and not owning one's home falls between +0.058 and +0.138, and (c) that the correlation between age of buildings and the number of occupants per room in the building is between −0.246 and +0.111. An example used by Goodman illustrates how wide the bounds may be. Taking Robinson's example of the relationship between race and illiteracy, Goodman calculates the bounds of the individual-level correlation as −0.07 and +0.58. These are too wide to be very useful.

The value of the Duncan-Davis method lies in the fact that it does not require any assumptions about internal relationships; but precisely because we do not bring any assumptions to bear, the method is rather inefficient.

III. ECOLOGICAL INFERENCE: REGRESSION TECHNIQUES

Hubert M. Blalock and Leo A. Goodman have in rather similar ways proposed the use of ecological regression coefficients, rather than ecological correlations, as a solution to Robinson's problem. Their methods are more efficient than the Duncan-Davis method of setting limits, but at the same time, they require some difficult assumptions.

Since the assumptions of the two methods are related to the way in which individuals are grouped into aggregates, I shall first discuss in general terms the process of aggregating individuals.

We can regard aggregate units as samples of individuals drawn from the total population in one way or another. The aggregate samplings with which we normally work are not random, but are drawn on the basis of geographic proximity. They could just as well, however, be drawn on some other basis. (I used the example above of aggregations drawn on the basis of individuals' last names).

What is important to ecological regression is the relationship between an individual's scores on X and Y and the probability that he will fall into a given aggregation. There are three ways by which individuals can be grouped ("sampled") into aggregations:

(1) They can be grouped in some way which is unrelated to their values on X and Y. In this case, knowledge of an individual's score on X or Y would not help us predict the aggregation in which he will be found

(2) They can be grouped more or less according to their scores on X, independently of their scores on Y, such that knowledge of an individual's X-score would help us predict which aggregation he will be in.

(3) They can be grouped more or less according to their scores on Y, independently of

their scores on X, such that knowledge of an individual's Y-score would help us predict his aggregation.

Grouping by geographic proximity, or by any other means, will approximate situation 1, situation 2, situation 3, or a combination of situations 2 and 3. Which of these situations has occurred constitutes the major assumption required by ecological regression.

Blalock's and Goodman's versions are presented in rather different ways, but we shall see that Blalock discusses the use of ecological regression when variables are measured intervally at the individual level, whereas Goodman mainly discusses the situation in which variables are measured nominally or ordinally at the individual level. The two forms of ecological regression, as we might expect, will require similar assumptions.

Blalock's Ecological Regression.

Blalock shows that for two "ecological" variables, X and Y, if we use as our measure of relationship the slope of the regression line rather than the correlation coefficient (which measures the spread of the data around the regression line), then we need make fewer assumptions to infer the individual-level relationship from the aggregate-level relationship.

He shows that b_A (the regression coefficient for $\overline{X}$ and $\overline{Y}$ among the aggregates) tends to equal b_I (the regression coefficient for X and Y among individuals), when the individuals are "grouped" into aggregates in a way which is related to their scores on X, that is, when they are grouped by the independent variable. This might be the case, for example, if we were using income as an independent variable. People with high income would tend to live in the same ward as other high income people, people with low incomes would tend to live together, etc. But if the individuals have been grouped by the *dependent* variable, b_A will tend not to equal b_I.

Thus we must meet a single general assumption if we are to infer individual-level relationships from aggregated data through ecological regression. This assumption is that *individuals have been grouped in such a way that their scores on the dependent variable are unrelated to the aggregation in which they fall, except indirectly through their scores in the independent variable.* That is, any relationship between an individual's score on the dependent variable and the aggregation into which he is placed must be indirect—a spurious relationship caused by grouping on the independent variable, and by the individual-level relationship between the independent and dependent variables.

This is simply to say that we cannot validly use the "ecological" regression coefficient b_{YX} if $\overline{Y}$ causes $\overline{X}$. This makes sense, since the regression coefficient is an asymmetric measure of relationship, with no clear interpretation if the dependent variable causes the independent variable.

To give a concrete example, suppose we wanted to find out how strongly a person's education affects his income, from data on the mean educational level and mean income of wards in a city. To use b_A as an estimate of the (individual-level) relationship, we would have to assume that a person's income (the dependent variable) is unrelated to the ward he lives in, when controlling for his level of education (the independent variable). In this case, the assumption would pretty clearly be unjustifiable, and we would be unable to use ecological regression to get what we are looking for.

Goodman's Ecological Regression.

Although Blalock does not discuss the level of measurement of the variables at the individual level, it is clear that his method is appropriate only when the variables are of interval measurement. There is no such thing as a slope or regression coefficient between ordinal or nominal variables, except in the case of dichotomies, so to show that b_A will tend to equal b_I is not useful when the variables are not measured intervally.[7]

Many of the variables with which social scientists are concerned in studying individuals—religion, social class, vote, and so on—are of nominal or ordinal measurement. When we deal with them, we shall have to use something other than the method Blalock suggests.

Leo A. Goodman presents a form of ecological regression which is appropriate for variables which are measured nominally or ordinally.[8] His articles appeared a few years after Robinson's, as an attempt to define those circumstances in which one could surmount the difficulty which Robinson had posed.

Briefly, his method is as follows: Let us assume that we have two dichotomized variables, such as "vote in election I" and "vote in election II," which we wish to relate at the individual level. The data we have in hand, by county, are % Democratic in election I, % Republican in election I, % Democratic in election II, and % Republican in election II.[9]

[7] The examples which Blalock uses in his chapter on aggregation are all of interval measure for the units which he groups together.

[8] Goodman, *op. cit.*

[9] For the sake of simplicity, I shall present Goodman's method as applied to two dichoto-

Then for each county we could construct the following matrix, where X_1, X_2, Y_1, and Y_2 are the data we have for the county, and P (per cent of election I Democrats voting Democratic in election II), Q (per cent of election I Democrats voting Republican in election II), R (per cent of election I Republicans voting Democratic in election II), and S (per cent of election I Republicans voting Republican in election II) are the data about the individuals which we wish to learn.

Election II	Election I		
	Democratic	Republican	
Democratic	$P \cdot X_1$	$R \cdot X_2$	Y_1
Republican	$Q \cdot X_1$	$S \cdot X_2$	Y_2
	X_1	X_2	

$P+Q=100\%$ $\quad X_1+X_2=100\%$

$R+S=100\%$ $\quad Y_1+Y_2=100\%$

If we could assume that P, Q, R, and S had exactly the same value in each county, we could easily solve for them by a regression equation derived as follows.

We know that $Y_1=P \cdot X_1+R \cdot X_2$. (Per cent of the vote Democratic in election II equals per cent of the vote Democratic in election I times the per cent of election I Democrats voting Democratic in election II, plus per cent of the vote Republican in election I times the per cent of election I Republicans who switch to the Democrats in Election II.)

Since $Y_1=P \cdot X_1+R \cdot X_2$, it follows that

$Y_1=P \cdot X_1+R(1-X_1)$, since

$X_1+X_2=1.0$

$=P \cdot X_1+R-R \cdot X_1$

Hence, $Y_1=R+(P-R)X_1$. Thus we end up with an equation equivalent to that for the linear regression of Y_1 on X_1, $Y_1=a_{YX}+b_{YX}X_1$. If we perform this regression on the county data for Y_1 and X_1, we solve for a_{YX}, which equals R, and b_{YX}, which equals P-R.[10]

The technique proposed by Goodman, then, is as follows. Pick one of the categories of the independent individual-level variable, and one of the categories of the dependent individual-level variable. From the aggregate data, plot the slope and the intercept for the relationship between per cent of the aggregate's population which is of the independent category (in our example, percentage of the vote Democratic in election I) and the per cent which is of the dependent category (percentage of the vote Democratic in election II.) The intercept for this relationship equals the percentage of individuals who are in the independent category which we leave out of the regression analysis, but who are in the dependent category we use in the analysis. That is, the intercept of the relationship between per cent Democratic in election I and election II equals R, the percentage of Republicans who switched to the Democrats in the second election. The slope equals the percentage of Democrats who stayed with the Democrats, minus the percentage of Republicans who switched. By calculating the slope and intercept of the relationship between per cent Democratic in election I and per cent Democratic in election II in this way, we can fill in the individual-level table relating vote in election I to vote in election II. R will equal a_{YX}, S will equal $1-R$, P will equal $a_{YX}+b_{YX}$, and Q will equal $1-P$.

This might seem like too much hocus-pocus and be suspect for that reason. But it makes sense when we look at what the slope and intercept in a regression equation mean. The intercept is the expected value of the dependent variable, when the value of the independent variable is set at zero. The slope measures the amount of increase in the dependent variable which results from an increase of one unit in the independent variable.

Looking back at the example, it makes sense that the intercept of the regression line should equal the proportion of election I Republicans who voted Democratic in election II. Where per cent Democratic in election I equals zero, all the voters in the district in election I will have been Republican. The percentage of the vote Democratic in such districts in election II will equal the percentage of Republicans switching to the Democrats.

Starting from this intercept, the slope measures the increase in percentage Democratic in election II which will result from adding a given percentage Democratic in election I to the district. If the make-up of the district is

mous variables. Since dichotomies can be treated as interval variables, Blalock's methods could also be used for two such variables, though the results would not be as simple conceptually as those from Goodman's method. But Goodman can be easily extended to non-dichotomous variables, which is not true of Blalock. To avoid creating the impression that there is a mysterious difference between Goodman's and Blalock's forms of ecological regression, I should point out that Goodman's version is equivalent to using Blalock's version with dummy variables, then transforming the resulting slopes back into proportions.

[10] Goodman, *op. cit.*, 612.

switched from 0% Democratic in election I to 10% Democratic in election I, how much is per cent Democratic in election II likely to change? When per cent Democratic in election I increases by 10%, per cent Republican in election I will decrease by the same amount. Thus, the change in per cent Democratic in election II should equal 10% times the proportion of election I Democrats who vote Democratic in election II, minus 10% times the proportion of election I Republicans who switched to the Democrats in election II. In other words, the slope $b_{YX}=P-R$, as Goodman says.

So far, we have assumed that the internal values were constant for all our counties. If P, Q, R, and S were constant among the counties, the regression equation would give an exact solution for each of them. But they will rarely be constant. Therefore Goodman shows that if the *expected* values of P, Q, R, and S are constant at all values of X_1 then b_{YX} will be an unbiased estimate of $P-R$.[11] That is, if for all different values of X_1, the *average* value of P and R (for Q and S are determined by the other two) is constant, Goodman's form of ecological regression is valid. This is consistent with common sense. If the expected value of R is the same at all values of X_1, then a_{YX} (the expected value of R when $X_1=0.0$) is a satisfactory estimate of R in general. But if the expected value of R is different at different values of X_1, a_{YX} will not be typical of R in general, and will be a biased estimate of R.

Goodman did not specify under what conditions this assumption is met, but we shall see that they are exactly the conditions in which the use of Blalock's method is valid. If individuals are placed into aggregate units randomly, or on the basis of their value on X, the assumption is met; if they are aggregated according to their Y-score, the assumption is not met and Goodman's method produces biased estimates.

In drawing up aggregations according to individual's scores on X, what we do in effect is set arbitrary marginal frequencies of X for the aggregate units. Given a dichotomous variable X, the effect of drawing people into aggregations according to whether they are + or − on X will be to determine the number of aggregations having a given percentage of their members + on X. We might set aggregations at either all + or all − on X. Or we might use a system whereby we draw fifty +'s and then draw individuals randomly, until we have a hundred individuals. Whatever we choose, we arbitrarily regulate the marginal X_1 values of the aggregations. In the first case, we set the value of X_1 at either 0% or 100% with no districts having any other value. In the second, we guarantee that the value of X_1 will be at least 50% in each district.

It is my contention that when we group by X in this way, the assumption of Goodman's method (that the expected values of P and R are constant at all values of X) is met. When we group by Y, the assumption is violated.

We wish to estimate P, the proportion of X_1's that are Y_1's. But there is an inverse proportion, P^1 (the proportion of Y_1's that are X_1's), which must be taken into account. If one of these proportions is held constant from aggregate to aggregate, but the marginals Y_1 and X_1 are varied, then the value of the other proportion (the inverse of the proportion which is held constant) must vary with the changes in the marginals.

Consider the two counties below:

County A

	X_1	X_2	
Y_1			150
Y_2			250
	100	300	

County B

	X_1	X_2	
Y_1			250
Y_2			150
	300	100	

If P is the same in the two counties then P' cannot have the same value in both. For example, if $P=.5$, the two counties will look like this:

County A

	X_1	X_2	
Y_1	50	100	150
Y_2	50	200	250
	100	300	

[11] *Ibid.*, 612.

County B

	X_1	X_2	
Y_1	150	100	250
Y_2	150	0	150
	300	100	

In this case, P' for County A is .333 and P' for County B is .600. Similarly, if P' had the same value for both counties, P would vary systematically with changes in the marginal values of X_1 and Y_1.

When we form aggregations by grouping along one of the two variables, what we do is hold the proportion based on that variable roughly constant (subject only to random error), while we vary the marginals. This forces the inverse of the proportion to vary systematically with the marginals. For example, if we group by Y, we draw a certain number of Y_1's from the total population, and then a certain number of Y_2's. Since we have drawn the Y_1's randomly from all the Y_1's in the population, we can expect that in all the aggregates we build this way, about the same percentage of Y_1's will be X_1's. In other words, P' is held roughly constant. At the same time, the marginals are varied. Thus P must vary systematically with the marginals, and analysis of the marginals by ecological regression will give us a biased estimate of P.

As an example of this, consider simulated data which are grouped, first by X and then by Y. Given a population whose individuals are distributed as shown in Table 1:

TABLE 1. THE POPULATION

	X_1	X_2	Total
Y_1	20,000	60,000	80,000
Y_2	80,000	40,000	120,000
Total	100,000	100,000	

These individuals are grouped into 100 aggregations of 1,000 individuals each. We know the per cent of each aggregation that are X_1, the per cent X_2, the per cent Y_1, and the per cent Y_2. What we do not know, but wish to learn through analysis of the aggregate data, are the values of P, Q, R, and S in the percentage table. If the assumption required by Goodman's

	X_1	X_2	
Y_1	$P \cdot X_1$	$R \cdot X_2$	P = %-age X_1's that are Y_1 Q = %-age X_1's that are Y_2 R = %-age X_2's that are Y_1
Y_2	$Q \cdot X_1$	$S \cdot X_2$	S = %-age X_2's that are Y

method is met, we can estimate the table from the aggregate data. How well is the assumption (that the expected values of P and R are constant at all values of X_1) met when we group by X? When we group by Y?

As I pointed out above, placing individuals into aggregations on the basis of their X value is equivalent to arbitrarily setting the frequencies of X_1 and X_2 for the areas.[12] In this example, individuals are drawn to form aggregations in such a way that the per cent X_1 in each district must be either 20%, 40%, or 60%. In the first type of aggregation there are 200 X_1's, in the second, 400 X_1's, and in the third, 600 X_1's. What are the expected values of P, Q, R, and S at these different values of X_1?

We see from Table 1 that the probability of an X_1 being a Y_1 is 20,000/100,000 = 0.20; the probability of an X_1 being a Y_1 is 0.80; the probability of an X_2 being Y_1 is 0.60; and the probability of an X_2 being Y_2 is 0.40. If we draw 800 X_2's, for example, the expected number of Y_1's is 0.60×800 or 480.

In this way we can calculate the expected number of X_1Y_1-individuals, X_1Y_2-individuals, X_2Y_1-individuals, and X_2Y_2-individuals for each type of aggregation. These expected values are shown in Table 2.

In grouping by X, the expected values of P, Q, R, and S remain constant at all values of X.

The results of grouping in a similar way by Y are different. Returning to Table 1, we see that the probability that a Y_1 will be an X_1 is 20,000/80,000, or 0.25, the probability that a Y_1 will be an X_2 is 0.75, the probability that a Y_2 will be an X_1 is 0.667, and the probability that a Y_2 will be X_2 is 0.333. The results of this grouping are shown in Table 3. Note that the expected value of P varies systematically with the value of the marginals.

Finally (though this is not illustrated), when we group in a way that is unrelated to either X or Y the expected values of P and Q are constant for all values of X. In this case, we in

[12] Under a probabilistic grouping of individuals, the district frequencies would not be fixed precisely, but their average, or expected, frequencies would be. The example I present uses precise fixing of frequencies for convenience.

TABLE 2. EFFECTS OF GROUPING BY X

$X_1 = 20\%$

	X_1	X_2	Total
Y_1	40	480	520
Y_2	160	320	480
Total	200	800	

$E(P) = 40/200 = 0.20$

$X_1 = 40\%$

	X_1	X_2	Total
Y_1	80	360	440
Y_2	320	240	560
Total	400	600	

$E(P) = 80/400 = 0.20$

$X_1 = 60\%$

	X_1	X_2	Total
Y_1	120	240	360
Y_2	480	160	640
Total	600	400	

$E(P) = 120/600 = 0.20$

effect draw individuals into the aggregations randomly. Again using the data in our example, the probability of drawing an individual who is X_1 and Y_1 is $20{,}000/200{,}000 = 0.1$; the probability of an X_1Y_2 is $80{,}000/200{,}000 = 0.4$. There will tend to be four times as many X_1Y_2's as there are X_1Y_1's in each aggregation, one-fifth of the X_1's in each aggregation will tend to be Y_1, and thus the expected value of P is constant for all values of X_1.

IV. REDUCING BIAS IN GOODMAN'S REGRESSION[13]

We have seen that Goodman's analysis requires exactly the same assumption as Blalock's.

[13] In this section I shall deal only with ways to reduce bias in Goodman's ecological regression. The suggestions I make should apply equally to Blalock's version, but I have not developed them along those lines here, since we only rarely use aggregate data to get at interval individual-level variables.

TABLE 3. EFFECTS OF GROUPING BY Y

$Y_1 = 20\%$

	X_1	X_2	Total
Y_1	50	150	200
Y_2	530	270	800
Total	580	420	

$E(P) = 50/580 = 0.09$

$Y_1 = 40\%$

	X_1	X_2	Total
Y_1	100	300	400
Y_2	400	200	600
Total	500	500	

$E(P) = 100/500 = 0.20$

$Y_1 = 60\%$

	X_1	X_2	Total
Y_1	150	450	600
Y_2	270	130	400
Total	420	580	

$E(P) = 150/420 = 0.36$

This limits its applicability more than had previously been realized. Actually, the difficulty of using Goodman's regression has been recognized in practice. In the decade since his method was presented, it has remained virtually unused in published research. This has probably been due to its propensity to produce badly biased estimates when its basic assumption is violated. These are particularly demoralizing since they often appear as percentages which are greater than one hundred or less than zero [14]

[14] The problem of estimates which are higher than one hundred per cent or less than zero per cent has been dealt with elsewhere by Telser: L. G. Telser, "Least-Squares Estimation of Transition Probabilities," in C. F. Christ, *et. al.*, *Measurement in Economics* (Stanford, Calif.: Stanford University Press, 1963.) He is concerned with the problem when it is due to predictable

In the preceding section, I have specified the assumption which must be met if we want unbiased estimation. This might be expected to make Goodman's method more usable. But if this is to be the case, we must be able to tell when we are safe in making the assumption. As it happens, we are often unable to tell whether or not Y causes X (which is another way of saying that data are grouped by the dependent variable.) It is not enough to know that Y does not cause X at the individual level. Even if this is so, the grouping process may in some way be based on individuals' Y-scores. In this case, %-Y will be a cause of %-X.

For example, we might want to relate education to occupation by calculating the slope of %-manual workers on %-high school graduates. The direction of causality at the individual level is clear enough: a person's education causes his occupation, but (except in a very weak way) occupation does not cause education. But individuals are very likely grouped into geographic aggregations largely on the basis of their occupations, so that %-manual workers would help to determine the %-high school graduates in the district.

Even when, as in this example, we can hazard a good guess that a given variable is grouped geographically, it is often difficult to tell the extent to which this is due to the other variable in the analysis, or to independent grouping on the variable. Thus it is often difficult to decide *a priori* whether we are justified in making the assumption that the data are not grouped along the dependent variable other than indirectly, through the impact of the independent variable.

But if we cannot be sure that the assumption is valid, there are two precautions we can take which will help to reduce the bias below what we would get using Goodman's regression in the ordinary manner. These may help reduce the bias to magnitudes we are willing to live with.

(1) *A Condition in Which Bias Is Low.*

First of all, the strength of the relationship at the individual level determines how much bias is caused by violations of the assumption. *When the variables with which we are concerned are strongly related at the individual level, grouping by the dependent variable does not cause as great bias in the ecological estimate as when they are weakly related.* This fact may allow us to use ecological regression with relative confidence, even when we are unsure of the assumption about grouping. If we have good reason to assume that the individual-level relationship is one which will generally be rather strong, then we will not be so concerned with the assumption about grouping.

When there is a strong relationship between the variables X and Y, this means that only a few X_1's fall into one of the categories of Y, while the vast majority fall into the other. As a result, changing the frequency of Y_1 cannot greatly change the incidence of X_1Y_1-types relative to that of X_1Y_2-types, though it will affect the incidence of X_1's as a whole very greatly. If there are practically no X_1Y_1's in the population, then an increase in the number of Y_1's in an aggregate unit does not add many X_1Y_1's. In the extreme case of a perfect relationship between X and Y at the individual level, in which all X_1's are Y_2's, there would be no variation at all in P, regardless of the aggregate units' Y_1 frequencies; each of the units would have zero X_1Y_1's, and P would always equal zero

To illustrate this, let us simulate further populations like that shown in Table 1. Keeping the total numbers of X_1, X_2, Y_1, and Y_2 constant, we shall adjust the number of X_1Y_1's, X_1Y_2's etc., so that the strength of the true individual-level relationship varies, and then calculate the expected frequencies of P as X varies, for aggregations formed by grouping along Y. In other words, we shall perform the same analysis as was done above in Table 3, but for populations which involve varying individual-level relationships between X and Y. Table 4 shows the results of such an analysis for four different populations, having the marginals given in Table 1, but varying in their internal distributions. Population B has the same internal distribution as that given in Table 1; population A has a distribution in which X and Y are more weakly related than in Table 1; populations C and D have distributions in which X and Y are more strongly related than

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estimation error in estimating parameters which fall near the extremes. His solution is to change the estimation procedure so that it cannot produce results of greater than one hundred or less than zero. This strikes me as a rather trivial problem, since an error of ten percentage points in estimating a parameter is just as harmful if it stays within the "respectable" 0–100 bounds as it is if it happens to make the researcher look silly by straying outside those bounds. I suspect, however, that the frequency with which one encounters these unnatural results in using Goodman's technique is due to a more serious problem than that of estimation error—violation of the assumption that data have not been grouped by the dependent variable.

TABLE 4. RELATIONSHIP BETWEEN %-X_1 AND E(P) IN VARYING POPULATIONS

Population	True ϕ^2	E(P), $X_1 = 45\%$	E(P), $X_1 = 55\%$	Difference
A	.042	.540	.107	.433
B	.167	.298	.130	.168
C	.375	.142	.078	.064
D	.602	.028	.013	.015

in Table 1. The point in Table 4 is that the expected value of P for aggregate units varies more strongly with the units' X_1 frequencies when the units are drawn out of a population in which X and Y have a relatively weak relationship. Since the bias involved in grouping along Y is due to the fact that such grouping causes $E(P)$ to vary with the value of X_1, this bias will be less serious when X and Y are strongly related in the population from which the aggregations are drawn

If we are willing to assume that the individual-level relationship is relatively strong, then we may use ecological regression with less fear of bias. In one sense, this is a strange kind of assumption to make. The analysis is intended to measure the individual-level relationship, yet we are asked to start by assuming the strength of that relationship! Still, we often have good reason to assume that a relationship will be in a certain vague range, although we want to know where it falls within that range.

Electoral stability and change studies are an example of the sort of situation in which we might use this characteristic of ecological regression to avoid analyses which are particularly prone to bias. In such a study of two British elections, for instance, we might be interested in estimating the percentages to complete the following table:

1959	1955			
	Conservative	Labour	Liberal	Non-voter
Conservative	...%	...%	...%	...%
Labour	...%	...%	...%	...%
Liberal	...%	...%	...%	...%
Non-voter	...%	...%	...%	...%
Total	100%	100%	100%	100%

The assumption that the dependent variable is unrelated to the aggregate units, except indirectly through the independent variable, must be met for each pair of independent and dependent variables needed to fill out the cells of the table. But it is unlikely that all of the 1959 variables are unrelated to geographic districts, so we cannot confidently make the basic assumption.

However, we should be able to assume that 1955-Conservative and 1959-Conservative are strongly related at the individual level, and likewise that the variable pairs 1955-Labour, 1959-Labour; 1955-Liberal, 1959-Liberal; and 1955-Non-voter, 1959-Non-voter are strongly related. For these analyses the problem of bias should be less serious, though of course it is not eliminated. On the other hand, for the rest of the analyses needed to complete the table (1955-Conservative, 1959-Liberal for instance) it is likely that the individual-level relationship is rather weak.

One possible strategy in this situation would be to limit our study to the four "stability" percentages which we can estimate with relatively little bias, and forget about measuring specific crossovers.

(2) *A Technique for Reducing Bias.*

A second way to reduce bias in ecological regression is based on the fact (for which a proof is offered in Appendix I) that whenever data are grouped by Y, if b_{YX} is positive, the bias in estimating P is positive. That is, the estimate of P based on b_{YX} and a_{YX} is greater than the true P in the population. At the same time, when b_{YX} is positive, b_{XY} is also positive, and P' (the proportion of Y_1's that are X_1) can be estimated from b_{XY} and a_{XY} with no bias or (if the data have been grouped by X as well as by Y) with a positive bias. Similarly, if b_{YX} is negative, then P is underestimated by b_{YX} and a_{YX}, and P' is either unbiased or underestimated by b_{XY} and a_{XY}.

This may allow us to reduce materially the amount of bias in estimating P. We can estimate P from two different regression equations, *and we can choose the estimate which probably incorporates less bias* (or, we hope, no bias at all). We have the estimate of P derived from b_{YX} and a_{YX}; to the extent that data are grouped by Y, this estimate of P is biased. Secondly, we can use the estimate of P' (biased to the extent that the data are grouped by X) to estimate P, by the equation: [15]

[15] P' times the total number of Y_1 individuals in the population tells us how many individuals in the population are X_1 and Y_1. If we divide this figure by the total number of X_1 individuals in the population, we have the proportion of X_1's that are Y_1

$$P = \frac{P' \cdot \text{Total number of } Y_1 \text{ individuals in the population}}{\text{Total number of } X_1 \text{ individuals in the population.}}.$$

If b_{YX} and b_{XY} are positive, then to the extent that the data are grouped by Y the direct estimate of P has a positive bias. And to the extent that the data are grouped by X, the indirect estimate of P has a positive bias (since the estimate of P' has a positive bias). Thus we have two separate estimates of P, for both of which any bias which exists is positive. In order to pick the one which involves less bias, we choose the lower of the two. If b_{YX} and b_{XY} are negative, by the same argument we pick the higher estimate of P, since any bias in estimates from a negative slope is negative.[16] In this way we can choose that estimate of P which is based on the regression coefficient which involves less grouping on the dependent variable.

There is another, perhaps more important, advantage to this obliging characteristic of ecological regression. In addition to reducing bias in the estimates, we can also specify the direction of whatever bias there is.

If b_{YX} and b_{XY} are positive, then, since any bias that remains after we have picked the lower estimate is positive, we know that the true value of P assuredly does not lie *above* our estimate (subject, of course, to sampling error). In this way we can estimate an upper, or lower, limit for the value of P.

We might choose to combine such an unbiased upper or lower limit with one of the Duncan-Davis limits, in order to produce a narrower range of possible values. Combined limits of this sort would involve one absolute limit (from the Duncan-Davis analysis), and one unbiased, estimated limit (the Goodman estimate).

An Example.

An example may illustrate the usefulness of this technique for reducing the bias in Goodman's estimates. In an ecological analysis of race and home-ownership, using 1960 census tract data from Mecklenburg County, North Carolina, the true individual-level relationship between the variables, ascertained from statistics for the county as a whole, is relatively weak, $\phi_2 = .116$. Thus the first condition for avoiding bias is not met. But the second technique, using both possible estimates of P, gives us considerably better estimation than we would get using normal procedures.

Letting H represent %-homeowner, and W %-white, the relevant regression equations are:

$$W = .275 + .835\,H$$

and

$$H = .227 + .491\,W$$

Mecklenburg County has 44,755 homeowners, and 60,403 white families.

From b_{WH} and a_{WH}, we estimate that 111.0% of homeowners are white. But estimating indirectly from b_{HW} and a_{HW}, we get a figure of 96.9%. Since the slopes are positive, we know that any bias in the estimates is positive. Therefore we choose the lower of the two. The true value, from census data, is 90.1%.

Similarly, estimating the percentage of whites that are homeowners, we get figures of 71.8% by direct estimation, and 82.2% by indirect estimation. Again we choose the lower figure, since the slopes are positive. The true value is 67.0%.

A Computational Aid.

Instead of actually calculating both the direct and indirect estimates of P, one may decide on the basis of the variances and means of the variables which estimate will involve less bias. As is shown in Appendix II, when:

$$\frac{\text{Var}_{Y_1}}{\overline{Y}_1\overline{Y}_2} > \frac{\text{Var}_{X_1}}{\overline{X}_1\overline{X}_2}$$

the direct estimate, $b_{YX}+a_{YX}$ is the more biased of the two estimates. But when this condition does not hold, either $b_{YX}+a_{YX}$ is the less biased of the two estimates, or there is no difference in bias.

Aside from easing our calculations, this condition gives us a good operational definition of the verbalizations "grouping by Y" and "grouping by X." $\overline{Y}_1\overline{Y}_2$, or $\overline{Y}_1\ (1-\overline{Y}_1)$, is the maximum value that the variance of Y_1 can reach, and $\overline{X}_1\overline{X}_2$, or $\overline{X}_1\ (1-\overline{X}_1)$, is the maximum possible value of the variance of X_1. The con-

[16] Note that the direct and indirect estimates of P may differ as a result of sampling error, as well as because of differing magnitudes of bias. This is why I stated above that by comparing the two estimates we can pick the one which *probably* incorporates less bias. In fact, if there were no bias in either estimate of P, and we consistently chose the lower of the two, we would ourselves cause a slight downward bias in the estimation procedure. Accordingly, if there are only slight differences between the estimates, it may be better to take the average of the two estimates, rather than pick the one which (presumably) incorporates less bias.

dition above can be verbalized as "the dispersion of Y_1 scores, relative to the amount of dispersion possible given the value of $\overline{Y}_1$, is greater than the disperion of X_1 scores, relative to the amount of dispersion possible given the value of $\overline{X}_1$." Or, "grouping by Y is greater than grouping by X."

V. CONCLUSION

It has been more than a decade since Robinson pointed out the flaws in ecological correlation, and since ecological regression and the Duncan-Davis limits were proposed as alternatives. The latter techniques require less restrictive assumptions than ecological correlation; yet neither has been adopted by researchers, while ecological correlations are frequently used.

In the case of the Duncan-Davis limits, this is probably due to the fact that researchers prefer a single point-estimate to a range of possible values, and to the fact that the limits are rather wide. The lack of interest in ecological regression is probably due to the fact that errors in estimation are likely to turn up either as negative percentages or as percentages which are greater than one hundred. This is disheartening to the researcher, and is difficult to present to his colleagues. Ecological correlations, on the other hand, are comfortably bounded by -1.0 and $+1.0$ no matter how incorrect the ecological inference is.

I hope that by specifying conditions in which the assumptions required by ecological regression are less critical, by suggesting a rough technique through which bias caused by errors in the assumptions can be reduced, and finally by its review of the Duncan-Davis, Blalock, and Goodman techniques, this paper will encourage greater use of alternatives to ecological correlation.

APPENDIX I

PROOF THAT WHEN THE SLOPES ARE POSITIVE ANY BIAS IN ESTIMATING P IS POSITIVE, AND THAT WHEN THE SLOPES ARE NEGATIVE ANY BIAS IN ESTIMATING P IS NEGATIVE[17]

Given a population of individuals measured on two dichotomous variables, x (consisting of attributes x_1 and x_2), and y (consisting of attributes y_1 and y_2):

Let P_t = the proportion of x_1-individuals who also have attribute y_1. The individuals in the population are grouped into aggregations. For each of these aggregations, let:

X_1 = the proportion of individuals having attribute x_1
X_2 = the proportion of individuals having attribute x_2
Y_1 = the proportion of individuals having attribute y_1
Y_2 = the proportion of individuals having attribute y_2
P = the proportion of x_1-individuals who also have attribute y_1
R = the proportion of x_2-individuals who also have attribute y_1

In estimating P_t by the ecological regression of Y_1 on X_1, there are four possible situations, with different implications for bias in the estimate:

A. DATA ARE NOT GROUPED BY y.

In this case, as was shown in the main body of the paper, the estimate of P_t is unbiased.

B. DATA ARE GROUPED BY y, AND $b_{Y_1X_1}$ IS POSITIVE

If this is the case,

1) *X_1 and P are positively related*

Since $b_{Y_1X_1}$ is positive, as Y_1 increases X_1 increases. But as Y_1 increases, Y_2 of course decreases, since for each aggregation $Y_1+Y_2=1.0$. This means that P will increase as Y_1 increases, since the number of individuals having attributes x_1 and y_1 will increase in the aggregations as Y_1 increases, but the number of individuals having attributes x_1 and y_2 will decrease as Y_2 decreases. Thus the number of x_1 individuals having attribute y_1 will increase relative to the total number of x_1 individuals. Therefore, Y_1 and P are positively related. Since both P and X_1 are positively related to Y_1, X_1 and P are positively related.

2) *Since X_1 and P are positively related, $(a_{Y_1X_1}+b_{Y_1X_1})>P_t$.*

Proof

a) Since X_1 and P are positively related, $E(P)$ will be at its maximum when X_1 is at its maximum, which is 1.0. (It is at this point that the assumption of a monotonic relationship enters; see the Note at the end of the Appendix.)

b) Therefore, when $X_1=1.0$, the expected value of P is greater than P_t, unless the value of X_1 equals 1.0 in every aggregation, in which case regression analysis is impossible anyway.

c) When $X_1=1.0$, $E(Y_1)=E(P)$, since when

[17] Assuming that any relationship between X_1 and P is monotonic. See the Note at the end of this Appendix.

$X_1=1.0$, $X_2=0.0$, and $Y_1=P\cdot X_1+R\cdot X_2=P\cdot 1.0+R\cdot 0.0=P$

d) When $X_1=1.0$, $E(Y_1)=a_{Y_1X_1}+b_{Y_1X_1}\cdot 1.0=a_{Y_1X_1}+b_{Y_1X_1}$

e) Therefore (from steps c and d), $a_{Y_1X_1}+b_{Y_1X_1}=E(P\mid X_1=1.0)$, the expected value of P given that $X_1=1.0$.

f) Since $E(P\mid X=1.0)>P_t$ (from step *b*), $a_{Y_1X_1}+b_{Y_1X_1}>P_t$.

3) *But $a_{Y_1X_1}+b_{Y_1X_1}$ is the estimate of P_t in ecological regression. Therefore, in this case, this estimate has a positive bias*

C. DATA ARE GROUPED BY y, AND $b_{Y_1X_1}$ IS NEGATIVE

This case differs from case B only in that now an increase in Y_1 coincides with a decrease in X_1. But an increase in Y_1 still brings an increase in P, since as above, an increase in Y_1 increases the number of individuals with attributes y_1 and x_1, and is accompanied by a decrease in Y_2 which causes a decrease in the number of individuals having attributes x_1 and y_2. In this situation X_1 and P will be negatively related.

This means, as in the similar proof above, that when $E(P)$ is at its minimum, and therefore is less than or equal to P_t, it is equal to $a_{Y_1X_1}+b_{Y_1X_1}$, which is thus a negatively biased estimate of P_t.

D. DATA ARE GROUPED BY y, AND $b_{Y_1X_1}=0.0$

Here, even though the data are grouped by y, P will be unrelated to X_1. Therefore, $a_{Y_1X_1}+b_{Y_1X_1}$ is an unbiased estimate of P_t. This makes sense if we realize that the only circumstances under which the data could be grouped by y, and X_1 still be unrelated to Y_1, is that for the individuals out of which the aggregate units are drawn, attribute x_1 is unrelated to attribute y_1.

NOTE:

The above proof assumes that there is a nonotonic relationship between P and X_1. If his is not so, it is not necessarily true that P eaches its maximum (or minimum) value vhen X_1 and $E(Y_1)$ are at their maximum, and hus that $E(P\mid X_1=1.0)\geq P_t$ (or $E(P\mid X_1=1.0)\leq P_t$)

If there is interaction between individuals' x-scores and y-scores in the grouping process, hen the P, X_1 relationship can be non-monoonic. Such interaction occurs if the expected roportion of y_1's who are x_1y_1 varies with the alue of Y_1 in grouping by y, or if the expected roportion of x_1's who are x_1y_1 varies with the alue of X_1 in grouping by x. In the simulated example of grouping by y presented earlier in this paper, for instance, there was no such interaction; the probability that an individual had attribute x_1, given that he had attribute y_1, was assumed to be the same in all the aggregations, with differing values of Y_1.

It would take a very specific kind of interaction, however, to produce both a non-monotonic P, X_1 relationship and a linear X_1, Y_1 relationship. Such a process of interaction grouping would have to be:

(1) *A process which produces a non-monotonic P, X_1 relationship.* Many interaction-based groupings would not do so. For example, it might be that as the values of X_1 and Y_1 increase, the value of P increases at an accelerating rate. The P, X_1 relationship would be nonlinear, but still monotonic.

(2) *A process, furthermore, in which the non-monotonic P, X_1 relationship is mirrored by an R, X_2 relationship in such a way that the X_1Y_1 relationship is linear.* If at some point in the value of X_1 there is an unusual shift in the proportion of x_1 individuals who are y_1, there must be a compensating shift in the proportion of x_2 individuals who are y_1, if the predicted value of Y_1 is to fall on the same line as the predicted values of Y_1 at other values of X_1.

Finally, even if there is a non-monotonic relationship between P and X_1, it may still be true, even though it is no longer necessarily true, that $E(P\mid X_1=1.0)\geq P_t$ for positive b_{YX} or $E(P\mid X_1=1.0)\leq P_t$ for negative b_{YX}. Whether it is true or not depends on the distribution of X_1 values. If most of the observations have X_1 values such that they share a region of increase (or decrease) with the region approaching $X_1=1.0$, then it will still be true that $E(P\mid X_1=1.0$ will be greater (or less) than P_t, even though there are some values of X_1 at which the expected value of P is greater (or less) than $E(P\mid X_1=1.0)$.

Since it must be highly unlikely for all of these conditions to be met, it seems to me that if we are confident of the (testable) assumption that X_1 and Y_1 have a linear relationship, we can normally assume that no interaction-based grouping of the type described above has occurred, unless there is some *a priori* reason to expect that it has.

APPENDIX II

PROOF THAT EXAMINATION OF THE MEANS AND VARIANCES OF THE VARIABLES WILL SHOW WHICH ESTIMATE IS MORE BIASED.

If $b_{YX}=0$, then as was shown in Appendix I, neither the direct estimate nor the indirect

estimate of P incorporates any bias. Therefore I will work here only with the situation in which b_{YX} is non-zero.

We must prove:

If $b_{YX} \neq 0$, a necessary and sufficient condition that the direct estimate of P be the more biased estimate is:

$$\frac{\text{Var}_Y}{\overline{Y}(1-\overline{Y})} > \frac{\text{Var}_X}{\overline{X}(1-\overline{X})}$$

Let us call the direct estimate P_D, and the indirect estimate P_I. Observe that:

$$P_D = a_{YX} + b_{YX} = (\overline{Y} - b_{YX}\overline{X}) + b_{YX}$$
$$= \overline{Y} - b_{YX}\overline{X} + b_{YX}$$
$$P_I = \frac{\overline{Y}}{\overline{X}} \cdot (a_{XY} + b_{XY}) = \frac{\overline{Y}}{\overline{X}} [(\overline{X} - b_{XY}\overline{Y}) + b_{XY}]$$
$$= \overline{Y} - b_{XY}\frac{\overline{Y}^2}{\overline{X}} + b_{XY}\frac{\overline{Y}}{\overline{X}}$$

Since

$$b_{XY} = b_{YX} \cdot \frac{\text{Var}_X}{\text{Var}_Y},$$

Denoted

$$b_{YX} \cdot \frac{V_X}{V_Y},$$

$$P_D - P_I = (\overline{Y} - b_{YX}\overline{X} + b_{YX}) - \left(\overline{Y} - b_{YX}\frac{V_X}{V_Y}\frac{\overline{Y}^2}{\overline{X}} + \frac{V_X}{V_Y}b_{YX}\frac{\overline{Y}}{\overline{X}}\right)$$

$$P_D - P_I = b_{YX} \cdot \frac{1}{\overline{X}V_Y}(V_Y\overline{X} - V_Y\overline{X}^2 - V_X\overline{Y} + V_X\overline{Y}^2)$$

$$P_D - P_I = b_{YX} \cdot \frac{1}{\overline{X}V_Y}[V_Y(\overline{X} - \overline{X}^2) - V_X(\overline{Y} - \overline{Y}^2)]$$

$$P_D - P_I = b_{YX} \cdot \frac{1}{\overline{X}V_Y} \cdot (\overline{X} - \overline{X}^2)(\overline{Y} - \overline{Y}^2) \cdot \left[\frac{V_Y}{(\overline{Y} - \overline{Y}^2)} - \frac{V_X}{(\overline{X} - \overline{X}^2)}\right]$$

$$P_D - P_I = b_{YX} \cdot \frac{1}{\overline{X}V_Y} \cdot (\overline{X} - \overline{X}^2)(\overline{Y} - \overline{Y}^2) \cdot \left[\frac{V_Y}{\overline{Y}(1-\overline{Y})} - \frac{V_X}{\overline{X}(1-\overline{X})}\right]$$

You will recall that when $P_D > P_I$, and b_{YX} is positive, or when $P_D < P_I$, and b_{YX} is negative, P_D is the more biased estimate. That is to say, when $P_D - P_I$ has the same sign as b_{YX}, P_D is the more biased estimate. In the final equation above, note that $\overline{X}$, V_Y, $(\overline{X} - \overline{X}^2)$, and $(\overline{Y} - \overline{Y}^2) \geq 0$. Therefore, unless one of them equals zero (in which case ecological regression would be useless anyway, since this would mean that there was no variation in one of the variables), the sign of $P_D - P_I$ is determined by the signs of b_{YX} and the sign of the term in the brackets. If and only if the term in the brackets is positive, $P_D - P_I$ and b_{YX} have the same sign, and thus the direct estimate is the more biased. If and only if the term in the brackets equals zero, $P_D - P_I$ equals zero, and neither estimate is more biased. If and only if the term in the brackets is negative, $P_D - P_I$ and b_{YX} have different signs, and thus the indirect estimate is the more biased.

In order to give an interpretation for the terms

$$\frac{V_Y}{\overline{Y}(1-\overline{Y})} \quad \text{and} \quad \frac{V_X}{\overline{X}(1-\overline{X})},$$

note that $\overline{Y}$ $(1\text{-}\overline{Y})$ is the maximum possible value of V_Y.

A common computational formula is:

$$V_Y = \frac{\Sigma Y^2}{N} - \overline{Y}^2$$

Since $0 \leq Y \leq 1$, V_Y reaches its maximum value when Y equals either zero or one for each aggregate unit. But when this is true, $\overline{Y}$ will be the proportion of units for which $Y = 1$, and therefore

$$\Sigma Y^2 = \overline{Y}N \cdot 1^2 + (1 - \overline{Y})N \cdot 0^2 = \overline{Y}N$$

Then

$$V_Y = \frac{\Sigma Y^2}{N} - \overline{Y}^2 = \frac{\overline{Y}N}{N} - \overline{Y}^2 = \overline{Y}(1 - \overline{Y})$$

Therefore when V_Y is at its maximum, it equals $\overline{Y}$ $(1\text{-}\overline{Y})$; similarly $\overline{X}$ $(1\text{-}\overline{X})$ is the maximum possible value of V_X. Accordingly, the term

$$\frac{V_Y}{\overline{Y}(1-\overline{Y})}$$

is interpreted as the magnitude of the variance of Y, relative to its greatest possible magnitude

VOTING ON THE PROPOSITIONS: BALLOT PATTERNS AND HISTORICAL TRENDS IN CALIFORNIA*

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Entering the polling booth on Tuesday, November 3, 1964, the typical California voter found himself confronted with an immense sheet of finely-printed green paper, a dirty black rubber stamp, a tiny ink pad, and thirty decisions to render. A few minutes later (the legal maximum is 10) he emerged and numbly surrendered to a clerk his ballot, now slightly embellished, like the fingers of his decision hand, with black ink stains.

Most of his decisions were made on a lengthy array of propositions. In these, questions on an assortment of issues were posed, each couched in language tedious and obscure—as only minds trained in the finest law schools could devise.

This study seeks to gain some knowledge about factors which influence the vote on these propositions. It employs a somewhat unorthodox method: an analysis of the voting patterns on actual ballots. The ballots furnish attractive data because they are, after all, completely accurate records of the results of those minutes of decision-rendering in the artificial privacy of that cramped polling booth.

It would be impossible by this method to generate a comprehensive theory about proposition voting behavior—too much information about the individual voter is forever lost by the secrecy incorporated in the balloting procedure. Nevertheless a contribution can be made in several areas of relevant concern, areas currently dominated more by folklore and mythology than fact or theory.

I. DATA

The data come from some 1600 ballots voted in Los Angeles county, most of them in the presidential election of 1964. First, by peering over the shoulders of the ballot counters on election night in 1964—and well into the next morning—it was possible to obtain an accurate record of the ballot patterns of the 209 voters in a reasonably typical middle income precinct. Second, through the cooperation of the county clerk, a modified random sample comprising 1085 of the 94,058 absentee ballots voted in the county in the election was obtained and the ballot patterns noted.[1] Finally, some limited ballot data were gathered for three municipal elections in the spring of 1965.[2]

II. 1964 IN CONTEXT: SOME HISTORICAL COMMENTS ON "MOOD" AND ABSTENTION

Before examining the ballot data directly it may be helpful to put the 1964 election in somewhat broader historical perspective. The method to be employed in this investigation of historical patterns in proposition voting will be multiple regression analysis. Those repelled either by history or regression analysis may wish to skip this section after being informed of two relevant findings: *when looked at in the light of the experience of proposition voting at general elections in California in this century, it is found, first, that the electorate was in a favorable "mood" in 1964 and, second, that abstention rates on the propositions were very low.*

Regression analysis permits one to estimate the independent impact of each of a number of variables as they interact simultaneously in association with a "dependent variable."[3] For the discussion of "mood," the variable to be explained, the dependent variable, will be the average "yes" vote for the "noncontroversial" propositions in each election, while the dependent variable in the abstention discussion

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[1] The absentees represented 3.3 percent of the total county vote.

[2] It might be noted here that the secrecy of the ballot applies to the voting procedure itself. The ballots are secret in the sense that it is impossible to link a particular ballot with an individual voter. After the ballot box has been opened anyone, even social scientists, can by law view the ballots as they are counted.

[3] For a discussion, see Mordecai Ezekiel and Karl A. Fox, *Methods of Correlation and Regression Analysis* (New York: Wiley, 1959); E. Malinvaud, *Statistical Methods of Econometrics* (Chicago: Rand McNally, 1966); N. R. Draper and H. Smith, *Applied Regression Analysis* (New York: Wiley, 1966).

will be the average abstention garnered by these propositions.

"Noncontroversial" propositions are the residue when one excludes from consideration all bond, initiative, and referendum measures. All of the noncontroversial propositions have been placed on the ballot by the state legislature—and by extraordinary majorities, a fact which helps to guarantee their noncontroversial nature. They tend to cover matters which seem legalistic, involved, trivial, or irrelevant to the average voter and, with very few exceptions, they generate little or no campaign debate. These measures, then, should be the most affected by whatever general mood happens to prevail at the time and the least influenced by peculiarities of special information and campaign activity.[4]

The period from 1918 to 1968 is covered in the regression analysis. The years previous have been excluded either because there were too few measures on the ballot or because the Republican party was in utter disarray due to Progressive or Bullmoose inroads, thus making it difficult to assess electoral partisanship in the election. Between 5 and 22 noncontroversial propositions appeared on the ballot in each of the elections considered.

Electoral mood. It is difficult to avoid the conclusion that there exists an underlying mood of favorableness or unfavorableness toward the propositions which forms a background from which specific propositions, due to reasonable or capricious circumstances, may deviate. Besides 1964, the years of 1960, 1952, and 1928 have seen a large measure of success for ballot measures while years like 1958, 1936, and 1918 have seen much failure. The average favorable vote on the noncontroversial propositions for the period has varied widely and erratically from a high of 65 percent to a low of 40 percent. At the same time, however, the standard deviation for the vote on these measures for each year has remained quite stable—at one point, in fact, it was constant for four straight elections. Thus it appears that *there are good years and bad years for noncontroversial propositions. The average success varies from election to election but the dispersion of propositions around that average remains quite steady.*

If there is such a thing as voter mood on the propositions, it would be of interest and importance to discover what determines the mood. Several variables which seemed on *a priori* grounds likely to be associated with the average favorable vote on the noncontroversial propositions were tested with the results as displayed in Table 1.[5]

Only one of the variables, a measure of the partisan nature of the election, proved to be a truly strong predictor. A number of measures of partisanship were experimented with and the one which proved to give the best fit was the percentage of total registration gained by the Republican candidate for Governor or President.[6] This correlates .70 with the average favorable vote on the noncontroversial propositions and proves to be by far the most powerful independent predictor even when a substantial number of other variables are thrown into the regression calculation. An increase of 3 percentage points in the Republican share of the registered vote is associated with an increase of over 2 percentage points in the average favorable vote on the noncontroversial proposition.

Thus it is found that *what is good for the Republicans is good for the noncontroversial propositions and vice versa.* When political, economic, and social forces have worked to the Republicans' benefit in California, they also seem to have encouraged a favorable mood toward the noncontroversial propositions. It

[4] For more content-oriented surveys of historical trends, see Sheldon Radabaugh, "Tendencies in California Direct Legislation," 42 *Southwestern Social Science Quarterly* 66–78 (June 1961); and Winston W. Crouch, *The Initiative and Referendum in California* (Los Angeles: Haynes Foundation, 1950).

[5] The regression equations are displayed vertically in the table. Standard errors are given in parentheses below each partial regression coefficient; to be regarded as statistically significant, a regression coefficient should be, conventionally, at least twice its standard error. The equations as a whole were significant (F test) at the .01 level. The Durbin-Watson statistic for serial correlation was also calculated. It fell in the inconclusive area for each equation.

[6] When the total vote cast is used as the percentage base, the Republicans tend to do "too well" for a proper fit in gubernatorial elections—particularly in the Warren era. This may suggest that Republican candidates for governor have done somewhat better than one would "normally" expect them to—a phenomenon perhaps attributable to the historically weak party structure in the state as well as to the party's ability to field candidates of bipartisan appeal. One way of overcoming this peculiarity was to take the total registration as a base thus smoothing out these disruptions in the lower turnout off-year elections. In 1946 the race for governor was won in the primary and thus the Republican vote for lieutenant governor has been used.

TABLE 1. REGRESSION ANALYSIS OF MOOD AND ABSTENTION, 1918–1968

Dependent variable	*Mood* Average favorable vote on the noncontroversial propositions, in percent (mean = 54.4; s.d. = 6.6)	*Abstention* Average abstention on the noncontroversial propositions, in percent (mean = 29.9; s.d. = 7.9)
Intercept	16.34	650.93
Independent variables		
Percent of the total registration gained by Republican candidate for President or Governor	0.71 (0.18)	−0.42 (0.12)
Turnout, in percent of registered vote	0.01 (0.13)	0.04 (0.11)
Average abstention on the non-controversial propositions, in percent	−0.10 (0.23)	
Number of propositions on the ballot	−0.24 (0.25)	0.02 (0.19)
Favorable vote on the most controversial proposition, in percent	0.12 (0.07)	
Trend; the year of the election	0.01 (0.13)	−0.32 (0.08)
Abstention on the most controversial proposi- in percent		0.63 (0.19)
R^2	.60	.81
Standard error of estimate	4.8	3.9

does not necessarily follow, however, that Republican voters are inordinately favorable to noncontroversial propositions. Indeed, as will be seen below, the ballot data from 1964 suggest the opposite—although it should be borne in mind that the 1964 election was one in which the relationship between Republican and proposition success did not hold up and also that there is something of a middle class bias in the ballots examined.

It is somewhat surprising that none of the other independent variables showed much strength. For example turnout is positively associated with the average favorable vote on the noncontroversial propositions (r = .25) for the period and it could be argued that citizens who vote only in presidential elections are disproportionately people who tend rather passively to acquiesce on the legislative ballot proposals—although this conflicts with the notion of the proponents of alienation that high turnout would be associated with referendum defeat.[7] However, as can be seen from Table 1, the independent influence of the variable in the regression equation is nil.

Similarly the variable denoting the average abstention on the noncontroversial propositions by itself correlates −.45 with the dependent variable—suggesting at that level that ab-

[7] See John E. Horton and Wayne E. Thompson, "Powerlessness and Political Negativism: A Study of Defeated Local Referendums," 67 *American Journal of Sociology* 487–88 (March 1962) and James S. Coleman, *Community Conflict* (New York: Free Press, 1957). Since only general elections are being considered in this analysis, turnout tends to be rather high in all cases. The election with the lowest turnout was 1942 with 59 percent of the registered vote, the highest, 1964 with 88 percent. Local elections frequently generate much lower turnout rates.

stainers, if prodded into rendering an opinion on these proposals, may support them. It too was found to have little independent effect in the regression equation. There was a somewhat stronger tendency for the length of the ballot to be negatively associated with the average favorable vote, but the relationship was not significant.

The vote on the most controversial proposition[8] might be expected to set the "mood" for the election. This variable does seem related to the average favorable vote on the legislative propositions but the relationship, while approaching statistical significance, is weak: the vote in favor of the most controversial proposition must rise over 8 percentage points to be associated with an increase of one percentage point in the dependent variable.

Abstention. It can be seen from Table 1 that, while there seems to have been no trend in electoral mood, *there has been a strong downward trend in abstention on ballot measures in California during this century.* The regression equation suggests a decline in average abstention of about one percentage point every three years. The low abstention rates found in the 1964 election simply place it as a late entry in this process.

A large drop in abstention occurred between the 1910 and 1912 elections (before the period covered by the regression analysis). The change was probably caused by increased interest in the ballot measures encouraged by the adoption in 1911 of initiative and referendum procedures, by the practice which began in 1912 of mailing out voters booklets, by the establishment in office in 1910 of the Progressive party in California with its enthusiastic—perhaps frenetic—preoccupation with "good government," and by the beginning of women's suffrage in 1912 with the disproportionate participation in early elections of those women who were the most intensely interested in politics.[9] After this period there was a general decline in abstention, then something of a leveling off during the twenties and thirties, a major drop in the middle forties, with a continued, but slower, decline since then.

The reasons for this trend are not particularly obvious. Californians may be somewhat more election conscious than they used to be, for turnout (based on registration figures) has slowly increased over the years.[10] However, the cause is not in a changed attitude toward abstention in general or simply in the postwar influx of new residents from the east (who might conceivably be expected to have different attitudes toward abstention) for abstention on the judicial races has remained fairly constant in the last thirty years. Voter knowledge about the judicial races has always been very low and nothing has ever been done to bring these contests to the attention of the voter; thus abstention remains constant (and large).[11] On the propositions, however, voter attention and knowledge may have been increasing in recent years leading to higher participation on these ballot items. Causes for this may be more education, greater and better coverage by the mass media, more effective campaigning on the propositions, and simply higher literacy rates.[12]

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[8] This is assumed to be the proposition with lowest abstention. Propositions which have generated controversy seem invariably to have low abstention rates. It seems then that, as the debate is more loudly and clearly drawn on a proposition and as the voter is made aware of the arguments of both sides and of the issues at stake, he is far more likely to reach a decision than he is to come to a psychological or ideological impasse due to a discovered value conflict or to cross-pressures. In only one of the elections was the proposition with lowest abstention itself "noncontroversial" by the definitions used here.

[9] For a discussion of some of these points, see also V. O. Key, Jr. and Winston W. Crouch, *The Initiative and Referendum in California* (Berkeley: University of California, 1939), pp. 531–532. Incidentally, the correlation between trend and abstention does not affect the interpretation of the "mood" equation: neither variable is significant even when the other is excluded.

[10] For a contrary argument, see Eugene C. Lee, *California Votes 1928–1960* (Berkeley: Institute of Governmental Studies, University of California, 1963), Chapter 1. The correlation of turnout with the year in California in general elections for the 1912–1968 period is .48. The regression coefficient suggests an average increase of turnout of about one percentage point every four years. Regression analysis using the year as an independent variable as well as a dummy variable for on- or off-year elections generates a highly significant equation and a multiple R^2 of .66:

$$\begin{aligned}\text{TURNOUT} = &-408.72 + 0.25(\text{YEAR})\\ &\quad (0.06)\\ &+ 11.35(\text{ON-OFF})\\ &\quad (1.96)\end{aligned}$$

[11] The abstention rate on the vote for state supreme court justices did drop noticeably in 1966 as a result of an unprecedented (and unsuccessful) campaign against the justices for their declaration that 1964's propostion 14 was unconstitutional.

[12] These data are generally consonant with James Rosenau's argument that the size of the

Whatever the reason for the decline in abstention, the trend gives little support to those who argue that material progress, the increasing size and technical complexity of society and the increasing remoteness of political events have led to rising political bewilderment, apathy, and alienation. Or to those who suggest that increasing "other-directedness" in our century has led to increased political apathy (a condition specifically *not* equated with non-voting in these writings).[13] It is surely the case that over the last fifty or sixty years things political and governmental have gotten bigger and more complex. Yet, as the decline of abstention suggests, this has *not* led to bewilderment or apathy on the part of the voter. Quite the contrary: voters now—at least in California—seem to be much more confident of their ability to render valid decisions on the complexities of modern government than they ever were in the past. This is not to say, of course, that they are making better decisions than their predecessors; it is only to assert that psychologically they seem more likely to *feel* their decisions are meaningful.

A search by means of regression analysis for other variables associated with the average abstention rate for the noncontroversial measures for the 1918–1966 period discovers that the percentage of total registration gained by the Republican candidate for Governor or President is again a healthy contributor: good years for Republicans have been associated with low abstention. On the 1964 ballots, however, there was no tendency for abstainers to vote Republican more than non-abstainers.

The abstention rate for the most controversial proposition is also a highly significant contributor to abstention on the noncontroversial propositions: when the abstention on this proposition decreases by two percentage points, the average abstention on the noncontroversial propositions goes down by well over one percentage point. Controversial propositions appear to draw the voters' attention and interest to the proposition part of the ballot. As noted above, this may partly explain the sharp statewide drop in abstention on legislative measures which occurred in 1912 when the initiative and referendum were first used. This phenomenon can also be seen in some of the ballot data. Total abstention on the noncontroversial propositions on the ballots examined from the local elections of 1965 was two to three times higher than it had been on the similar propositions on the November 1964 ballots. The difference undoubtedly rests in the absence in the 1965 elections of any truly controversial ballot measure to catch the voter's attention and interest.

Turnout again is found to be inadequate in the regression analysis. It has no independent effect on abstention, although the simple correlation between the two is −.44. Thus those who vote at every election do not appear to be more (or less) likely to vote on the propositions than those who only appear at elections with high turnout. The length of the ballot, rather surprisingly, also is not related independently to abstention. Voters do not appear particularly to be put off by lengthy ballots.

III. THE PROPOSITIONS IN 1964

Table 2 supplies an indication of the substance of each of the propositions in 1964.[14] The first three measures and the last two were bond issues on which there was no open controversy.[15] Propositions 4 through 12 were placed on the ballot by the legislature. All were accompanied by an utter absence of campaign debate and will be considered the "noncontroversial" propositions for the purpose of the ballot analysis.

While proposition 13 was "noncontroversial" in the sense that there was little or no organized campaigning for or against it, it was closely linked to proposition 16 which proposed to set up a private lottery in the state and was controversial in the extreme. To stifle the lottery proposal the legislature inserted on the ballot as proposition 13 a measure which would outlaw the naming of private corporations in the

attentive public has been increasing. See his "The Attentive Public and Foreign Policy" (Center of International Studies, Princeton University, March, 1968).

[13] See Murray B. Levin, *The Alienated Voter: Politics in Boston* (New York: Holt, 1962), pp. 70–71; Kenneth Keniston, "Alienation and the Decline of Utopia," 29 *American Scholar* 179–180 (Spring 1960); and David Riesman, *The Lonely Crowd: A Study of the Changing American Character* (Garden City, N. Y.: Doubleday-Anchor, 1955), pp. 196–199.

[14] A facsimile of much of the 1964 ballot, including all of the statewide propositions can be be found in Clyde E. Jacobs and John F. Gallagher, *California Government: One Among Fifty* (New York: Macmillan, 1966), p. 53.

[15] The ease with which these bonds passed is typical. Of the 31 bond issues referred by the California legislature to the electorate at general elections since 1910, only three have failed to pass. One of these was the school bond measure on the 1968 ballot, a reaction in large measure to student disorders. The most recent previous bond failure occurred in 1922.

TABLE 2. THE PROPOSITIONS AND THE VOTE

	Proposition	Percent Yes of Votes Cast on the Proposition			Abstention Rate in percent			Newspaper Recommendation	
		County	Precinct	Absentee Sample	County	Precinct	Absentee Sample	Times	Herald-Examiner
1	Beach and park bonds ($150 million)	58	72	63	12	10	11	Y	N
2	College bonds ($380 million)	62	64	70	14	14	11	Y	Y
3	School building bonds ($260 million)	66	70	70	15	17	13	Y	Y
4	Restrict veterans' tax exemption to residents	69	71	65	14	15	12	Y	Y
5	Improve veterans' widows' tax exemption	58	59	69	14	17	11	N	N
6	Establish retaliatory tax on out of state insurers	79	78	70	22	27	21	Y	Y
7	Permit investment of public retirement funds in corporations	44	48	45	20	22	18	N	Y
8	Keep unopposed judicial races off the ballot	67	72	68	24	28	26	Y	Y
9	Apportion all supervisorial districts uniformly	75	75	75	27	33	28	Y	N
10	Permit use of certain school funds for other purposes	53	55	49	20	24	19	Y	N
11	Eliminate vote requirement in city-county contracts	72	76	61	27	32	30	Y	Y
12	Permit property tax relief in disaster areas	73	75	85	17	23	14	Y	N
13	Ban naming of corporations in the Constitution	71	68	67	22	31	25	Y	Y
14	Prohibit state from restricting owner's right to sell	67	70	66	4	3	6	Y	Y
15	Outlaw pay television	62	54	55	6	4	9	N	N
16	Set up privately-run lottery	34	42	44	8	9	8	N	N
17	Change railroad train crew laws	62	66	70	8	8	15	Y	Y
A	County Hospital bonds ($26.7 million)	67	72	73	17	21	16	Y	Y
B	County juvenile detention building bonds ($8.6 million)	65	71	70	17	20	16	Y	Y

California constitution, a measure which would, in effect, make the lottery impossible. Had both propositions passed, the one with higher affirmative vote would have been authoritative.[16]

The last four state propositions, 14 through 17, were all initiatives and generated considerable controversy. The notorious proposition 14 prohibited state or local governments from limiting an owner's right to sell or rent residential property—in effect it repealed acts of the legislature which had been designed to halt racial discrimination in housing. Campaigning on this issue was easily the most intense of any in 1964. Except for a right to work proposition in 1958, fewer California voters abstained on

[16] A crude, but provocative, estimate of the extent of the role sheer caprice can play in the vote on the propositions can be gained by cross-tabulating the votes on propositions 13 and 16 as in the table below. A consistent pro-lottery vote would require a "yes" vote on 16 and a "no" on 13 while consistent anti-lottery people voted against 16 and for 13. Those 14 to 17 percent of the ballots marked favorably for both measures largely defy rational explanation for by approving proposition 13 these voters were moving to nullify their affirmative vote on 16. Most likely, they genuinely favored the well-publicized lottery measure but, uninformed and uninstructed, they also voted for proposition 13 without seeing the logical connection. Another 8 percent of the absentees and 12 percent in the precinct voted for the lottery and abstained on proposition 13. Since the latter measure was designed to nullify the lottery vote, these lottery proponents who let others decide on it can hardly be said to be fully aware of their own best interests in this matter. Together these two groups make up one quarter of the ballots. In addition the rationale for voting negatively on both propositions is a bit feeble. Furthermore, if those who voted affirmatively on both were voting randomly on proposition 13 it is to be expected many citizens voted consistently only because they "guessed right" on the proposition. A similar argument could be made for those who voted against both measures. If the "worst" assumptions are made in each case, the number of ballots marked capriciously for at least one of these measures is about 60 percent. This is an outside limit under these considerations and the argument is a bit strained, but the exercise should serve to suggest orders of magnitude. It would appear that the vote on these measures

proposition 14 than on any ballot measure in the century.[17]

While the lottery promoters sought in proposition 16 to create an industry, the theater owners in proposition 15 sought equally ingeniously to destroy one. Concerned by the threat of competition from a new pay television company, they endeavored to deal frontally with their problem: they organized to get petitions signed so that an initiative could be put on the ballot making (home) pay television forever illegal in the state of California.[18]

Proposition 17 suggested that California's laws about size of railroad crews be replaced with ones which would reduce the minimum requirement. The measure was supported by management and opposed by labor.

IV. AGGREGATE RESULTS FROM THE BALLOT DATA AND THE COUNTY

Table 2 also displays the aggregate voting results on the propositions for the 209 precinct ballots and for the ballots sampled from the absentees. When these are compared to the results prevailing in the county as a whole it can be seen that, despite the rather middle class bias of the ballot data, there are no spectacular variations. In fact had no one been permitted to vote except for these 1294 citizens the decisions on all nineteen propositions would have been precisely the same.[19]

The precinct voters differ rather markedly from those in the county as a whole only on the first proposition—understandable since the proposition proposed funds for a large park near the precinct. The absentees' differences with the county are also generally quite small, the most notable discrepancies occurring in the vote on propositions 11 and 12.[20]

V. ANALYSIS OF THE BALLOT DATA

The data from these ballots can be used to yield evidence about the following possible sources of influence in proposition voting: 1) newspaper recommendations, 2) political party endorsement, 3) campaigning, 4) cohabitation, 5) popular patterns of thinking, 6) order effects, 7) negativism, and 8) yeasaying and naysaying.

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was itself the result of a kind of lottery.

Proposition 13		Proposition 16 Yes %	Abstain %	No %	Total %
		Absentees (N = 1085)			
Yes		17	1	32	50
Abstain		8	8	9	25
No		15	—	10	25
	Total	40	9	51	100
		Precinct (N = 209)			
Yes		14	1	32	47
Abstain		12	8	11	31
No		12	—	10	22
	Total	38	9	53	100

[17] See also Raymond E. Wolfinger and Fred I. Greenstein, "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," this REVIEW, 62 (September 1968), 753–769; and Harlan Hahn, "Northern Referenda on Fair Housing: The Response of White Voters," 21 *Western Political Quarterly*, 483–495 (September 1968.)

[18] Both propositions 14 and 15, passed by the state electorate at this election, were subsequently declared unconstitutional by the courts.

[19] The similarity between precinct and absentees found in the cross-tabulations in note 16 above is quite typical.

[20] A brief analysis of absentee voting from several California elections as well as of the limited literature on the subject suggests that the vote of the absentees does not usually differ greatly from the precinct vote. Absentees tend to be better off economically than the average and, as seen in Table 2, somewhat more likely to favor bond issues. Also, as often noted, absentees are likely to vote a few percentage points more Republican than other voters. The advantage for the Republicans, however, varies from election to election and from race to race and does not appear to depend on whether the Republican candidate is a moderate or a conservative. There is also some tendency for the Republican gain among the absentees to diminish as income increases; in poor areas the absentees are distinctly Republican while in wealthy areas they differ little from other voters. See John E. Mueller, "Reason and Caprice: Ballot Patterns in California," unpublished Ph. D. dissertation, University of California, Los Angeles, 1965, chapter 3. See also James K. Pollock, "Absent Voting with Particular Reference to Ohio's Experience," 15 *National Municipal Review* 282–292 (May 1926); Charles E. Merriam and Harold F. Gosnell, *Non-Voting: Causes and Methods of Control* (Chicago: University of Chicago, 1924), pp. 63–72; Joseph P. Harris, *Election Administration in the United States* (Washington, D. C.: Brookings, 1934) pp. 283–301; Belmont Brice, Jr., "Absentee Voting and the Character of the Electorate," *BGR Observer*, June 1961 (Los Angeles: Bureau of Governmental Research, University of California).

1. *Newspaper recommendations.* The recommendations of local newspapers, it might be expected, are important aids to the voter's decision on the array of propositions. As Edward C. Banfield and James Q. Wilson note, "Where voters must . . . pass on a multitude of referenda, the newspaper acquires added influence."[21] This is presumably very likely to occur in Los Angeles where "the *Times* has long been regarded as the most powerful single political force in the city."[22] The newspaper had a circulation in 1964 of over 800,000 daily and 1,144,000 on Sunday. The circulation of the Hearst *Los Angeles Herald-Examiner*, the other major newspaper in the county, is about three-quarters that of the *Times*.

The newspapers were far from secretive about their wishes. The *Herald Examiner*, published a full statement of its preferences on the editorial pages of both the Sunday and Monday editions before the Tuesday election. The *Times* did even better, printing its recommendations on the front page of the Sunday edition and including them also in the editorial pages on Monday and Tuesday.

To be counted, the absentee was required to mail his ballot so that it was received by the day before the election. Some 9 percent of the absentee ballots were received by the county clerk on Monday and another 7 percent were received in a special mail delivery on Sunday. Since the city's newspapers published their recommendations in full only on Sunday, this means that a maximum of only 16 percent of the absentees could have seen the recommendations in full before voting. This figure errs, of course, very much on the high side. A large majority, if not all, of the ballots received on Sunday and many of those received on Monday were undoubtedly mailed before the Sunday newspapers were printed. Furthermore a tenth of the ballots received on the two days were military ballots, suggesting an early and distant mailing.

Therefore, since the preferences of the absentees do not seem to differ much from those of the voters in the rest of the county who would find the newspapers' recommendations readily accessible (see Table 2), an initial hint that newspapers may not be a major influence on the proposition vote can be registered.

A further hint is given in the following information: *out of the sample of 1,293 ballots, not a single one was marked on the propositions exactly according to the recommendations of either of the county's newspapers.*

In reflecting on this fact, however, it should be noted that, since there were three ways to vote on each proposition (yes, no, abstain) there were 1,162,261,467 different patterns a voter could select on the nineteen propositions. Thus if people marked their ballots randomly one would expect a given pattern rarely to appear. This is important to keep in mind, even though it is not being argued that ballots are marked (purely) by chance. Nevertheless the idea that some voters slavishly duplicate their newspaper's wishes over the entire list of propositions is by no means bizarre. It might be expected that at least a few people methodically clip out the ballot recommendations section of their newspaper and take it to the polls with them. Indeed, in a survey of readers of the *News-Press* in Santa Barbara, California, 12 percent stated that they took the newspaper's recommendations to the polls with them at *every* election, while another 27 percent stated that they did so occasionally.[23] It is true that many of these may deviate here and there, but to expect some of them at least to make no exception whatsoever hardly seems unjustifiable. Furthermore, it is to be expected that some people in the county, especially in middle income areas, wholly without reference to the newspaper would follow the same reasoning as its editorial board—the "fiscally responsible" Republican attitude of the *Times*, for example—and thus by chance add to the group adopting the newspaper's line. Shortly after the November election a number of students of California politics were asked, "Out of 209 voters in a reasonably typical middle class precinct in Los Angeles, how many do you think voted the *Times* line exactly?" Estimates—not all of them carefully considered—generally ranged from 20 to 80 percent and the information that none of the voters had done so brought near-universal surprise (and glee).

Although no voter selected over the nineteen propositions the pattern suggested by either of the city's largest newspapers, it might be argued that to expect *perfect* agreement with newspaper recommendations on the ballot is really too stringent a test. In assessing the power of the press perhaps one should, first,

[21] Edward C. Banfield and James Q. Wilson, *City Politics* (New York: Vintage, 1963), p. 324.
[22] *Loc. cit.*

[23] James E. Gregg, "Newspaper Editorial Endorsement: Their Influence on California Elections," (unpublished Ph.D. dissertation, University of California, Santa Barbara, 1964) p. 266. See also James E. Brinton, Chilton R. Bush and Thomas M. Newell, *The Newspaper and Its Public* (Stanford: Institute for Communication Research, Department of Communication and Journalism, Stanford University, 1959), p. 63.

allow for some "error" from a strict newspaper-line vote and, second, concentrate primarily on the noncontroversial propositions where campaigning is nonexistent, ideological predisposition is minimal, confusion is greatest and thus where the influence of the press is most likely to be exerted.

Table 3 displays the distributions of ballots according to the number of deviations from the pattern suggested by the newspapers. From this it can be seen, for example, that about 8 percent of the voters in the precinct voted the *Times* line with three or fewer deviations. It is doubtful that this percentage represents a significant indication of press influence, since the "error" distributions for the precinct and the presumably largely uninfluenced absentees are very similar. More faith could be put on this observation of course if the precinct data base were larger, but even as it stands the close parallelism of the distributions is impressive.

It might be argued that the data in the table demonstrate that the *Times* was more influential than the *Herald-Examiner* and therefore by implication that the *Times* at least had some impact. A more plausible explanation is that the difference is a function of negativism. As noted in Section II above, 1964 was a very affirmative year in California; only two of the propositions failed to get a majority in the state. Thus the more negative votes a newspaper recommends, the more unlike the consensus it was likely to appear; and the fewer the negative votes recommended, the sooner the newspaper is likely to pick up by this measure anyone inclined to vote affirmatively on all the propositions as a fellow traveler. Because of this the *Times*, which recommended negative votes on four propositions, is likely to appear more influential than the *Herald-Examiner* which recommended seven "no" votes (see Table 2). Indeed a newspaper which urged affirmative votes on *all* the propositions would do considerably better by this measure than either newspaper.

TABLE 3. DISTRIBUTION OF BALLOTS ACCORDING TO THE NUMBER OF "ERRORS" FROM NEWSPAPER RECOMMENDATIONS ON NINETEEN PROPOSITIONS

Number of "Errors"	Los Angeles *Times*		Los Angeles *Herald-Examiner*	
	Absentees %	Precinct %	Absentees %	Precinct %
0	—	—	—	—
1	—	—	—	—
2	2	4	—	—
3	4	4	—	—
4	7	6	1	2
5	9	10	4	4
6	10	10	8	9
7	12	13	9	11
8	10	8	15	15
9	8	8	14	10
10	8	5	9	11
11	6	3	10	8
12	6	4	7	6
13	4	4	6	4
14	3	3	4	4
15	2	5	3	3
16	2	4	2	5
17	2	3	3	5
18	2	3	2	3
19	3	1	3	1
	100	98	100	101
N =	1,085	209	1,085	209

When only the "noncontroversial" propositions, numbered 4 through 12, are considered, the case for the influence of the press remains feeble. Only two voters in the precinct selected the pattern suggested by the *Times* on this shortened list of propositions and only one voter applied the *Herald-Examiner's* formula. Perhaps one should remove the two veterans' propositions, 4 and 5, from consideration on the grounds that some voters may already have firm opinions about such perennial issues and thus would have little need to consult their newspaper. This process leaves as a residue propositions 6 through 12—the most complicated and obscure on the ballot. On these measures the *Herald-Examiner's* line is voted by only two voters in the precinct.

The *Times* does better, amassing seven voters in the precinct—about 3 percent. While this number is hardly spectacular, it becomes even less so on closer examination. On these propositions the *Times* recommended a "yes" vote on all except proposition 7. In Table 4 a comparison is made of the frequencies of ballots with affirmative votes on six of these measures and a negative vote on the seventh. It can be seen that the *Times* pattern is only about as frequent—*both* in the precinct and among the absentees—as would be expected from a knowledge of the *statewide* vote on the measures: proposition 7 did worst of the seven and accordingly the pattern with a single negative vote on that proposition has a high frequency and each frequency falls at about the expected rate. Had the *Times* recommendation exercised a special effect it is fair to expect this pattern to attain an embellished frequency.

It might be anticipated that the recom-

TABLE 4. FREQUENCIES OF PATTERNS ON PROPOSITIONS 6 THROUGH 12 WITH ONE NEGATIVE AND SIX AFFIRMATIVE VOTES

Pattern	Number with Pattern		Statewide "No" Vote on Negative Measure as % of Votes Cast on Measure
	Absentee Sample	Precinct	
NYYYYYY	18	2	21
YNYYYYY[a]	34	7	58
YYNYYYY	16	5	41
YYYNYYY	5	1	24
YYYYNYY	27	8	49
YYYYYNY	15	1	28
YYYYYYN	6	1	34

[a] Los Angeles *Times* recommendation.

mendations of a more local newspaper would seem more relevant to the precinct voters and thus have greater impact. But, in fact, the results seem much the same. Only one of the 209 voters in the precinct voted the local newspaper's line on the 16 propositions for which it made recommendations.

The power of the press might also be tested by looking at the correlations among pairs of propositions, a procedure which gets away from the demand that perfect or near-perfect voting be found on a string of propositions. To suggest a newspaper influence is operating, the correlations should be positive when the newspapers recommended "yes" votes or "no" votes on both and they should be negative when the press suggested a favorable vote on one and a negative note on the other. In fact, however, no such patterns emerge and virtually all of the correlations among the propositions, regardless of the position of the press, are positive. This suggests nothing so much as an overriding influence by fairly persistent yea-sayers and nay-sayers—a phenomenon to be examined below.

As noted above, some limited ballot data were also gathered for three municipal elections in the spring of 1965. Comparable analysis of patterns on the propositions on these ballots found little evidence that newspaper recommendations are more influential on these local elections.

In short, the evidence for believing that newspapers exercise little influence on the vote for the propositions seems quite persuasive.[24] It is true of course that none of the measures used with the ballot data can indicate finally and surely whether the vote of a particular individual on a particular proposition was or was not influenced by the position of the press. But *it seems quite likely that there is no major impact of newspaper recommendation on proposition voting, especially over a series of ballot items.*

2. *Party endorsement.* The Democratic party in California in 1964 strongly advocated a negative vote on proposition 14—the fair housing issue—and also urged its partisans to support the bond issues. At the same time, the official Republicans, split on the issue, smiled and said nothing. The strongest correlations among items on the ballots examined were precisely on these issues with Democratic voters 30 to 50 percent more likely than Republicans to support the bonds and to reject proposition 14.

The sharp polarization of attitude, especially on the fair housing issue, goes well beyond what one would expect on the basis of findings from surveys for the noncampaign situation and suggests that the *parties can be extremely important in shaping opinion on the propositions.*[25] However, this influence is limited (and

[24] Most of the research in the area of newspaper influence on issues and referendum voting suggests that the impact of the press is low at best. See George A. Lundberg, "The Newspaper and Public Opinion," 4 *Social Forces* 709–715 (June, 1926); Harold F. Gosnell and Margaret J. Schmidt, "Relation of the Press to Voting in Chicago," 13 *Journalism Quarterly* 129–147 (June, 1936); James E. Brinton and Norman McKown, "Effects of Newspaper Reading on Knowledge and Attitude," 38 *Journalism Quarterly* 187–195 (Spring, 1961); and Alvin Boskoff and Harmon Zeigler, *Voting Patterns in a Local Election* (Philadelphia: Lippincott, 1964), ch. 4. Impressions on the California case are given by Erwin A. Cottrell, "Twenty-five Years of Direct Legislation in California," 3 *Public Opinion Quarterly* 39–41 (January, 1939). Studies which argue that the newspapers do have influence include Gregg, *op cit.*, his "Newspaper Editorial Endorsements and California Elections, 1948–62," 42 *Journalism Quarterly* 532–538 (Autumn, 1965), and his "Newspaper Endorsements and Local Elections in California," (Institute of Governmental Affairs, University of California, Davis, May 1966). For a critique of Gregg's approach, see Mueller, *op. cit.*, pp. 183–185.

[25] The importance of active party endorsement of referendum issues is also noted in Wolfinger and Greenstein, *op. cit.*, pp. 760–764 and Norman C. Thomas, "The Electorate and State Constitutional Revision: An Analysis of Four Michigan Referenda," 12 *Midwest Journal of Political Science* 115–129 (February 1968). The studies also suggest that class voting differences can be overwhelmed by the influence of party endorsement.

made difficult to examine fully) by the fact that the parties take positions on only a few ballot measures. This is sensible from their point of view, of course, since they do not wish their position on the propositions to harm them in the partisan races—after all, every stand alienates somebody.

3. *Campaigning.* When non-party campaigns are launched on particular propositions they may also make some difference. In the last days of the campaign, labor and management spent some effort in propagandizing for their position on proposition 17 which concerned railroad crew laws. The campaign probably helped to reduce abstention on the issue: as can be seen from Table 2, the absentees, many of whom must have missed the brief, last minute campaign, were notably more likely to leave their ballots blank for the proposition. Also, the preferences of the voters in the precinct were a bit more likely than those of the absentees to be correlated with their votes on other liberal-conservative issues and candidates. (A similar phenomenon is found in the nonpartisan race for district attorney between a liberal and a conservative where last minute campaigns served to stress ideological differences.)

Thus very limited evidence suggests that *campaigning can reduce abstention and align preferences.* However, the overall effect will, again, be limited to those few propositions on which active campaigning is done.[26]

4. *Cohabitation.* Of the ballots examined in the precinct there were eight or nine pairs of ballots marked on the propositions, and usually also on the partisan races, in an identical manner. Each of the patterns is quite individualistic and thus most likely represents a husband and wife pair in total agreement. That out of 209 voters in a residential section so few pairings occurred indicates that absolute household coordination on politics ("I always vote the way my husband tells me") is rather less than the rule.[27]

[26] See also Wolfinger and Greenstein, *op. cit.*, pp. 762–764.

[27] Interviewing in a California city shortly after the 1958 election in which there were eighteen state propositions on the ballot, over 700 married couples were asked, "As far as you remember, did you both vote the same way on all the state propositions?" Fully *72 percent* of those who had voted in the election stated that they had voted the same way. The radically conflicting ballot data from the precinct indicate that one cannot rely on self-report in a survey situation to substitute for the actual examination of ballots.

5. *Popular patterns of thinking.* An extensive search was made of the ballots to check for the existence of groups of voters who, for whatever reason, happen to think alike on a series of propositions. For example, a plausible interpretation of aggregate voting returns might be that there exists a sizeable group of like-minded individuals who represent the overall mood for the year; it is their votes which by and large determine the vote result while the votes of the remainder of the electorate more or less cancel each other out. Or, it might be expected that a significant number of voters would have left their ballots blank except to vote on the most hotly debated proposition, number 14.

In fact, it was found that there were almost as many ballot patterns as there were ballots. Over 90 percent of the voters in the 1964 ballot samples selected patterns on the propositions which were unique. Only one pattern reached a frequency of even 2 percent. This was a flat abstention on all the propositions by twenty-three absentees. There was not a single voter in the precinct who voted exactly in accordance with the majority of the precinct on the propositions. It may also be relevant (and perhaps marginally reassuring) to note that no ballots were marked in a manner indicating an obvious attempt to "make patterns," such as alternating affirmative and negative votes.

Taking subsets of the propositions, the only patterns that emerge distinctly are those noted between and among the bond issues, proposition 14, and the partisan races. Other correlations among ballot items are very low. Thus for the most part the *passage or defeat of any given proposition is due to the combined efforts of a body of voters who are quite unlike in their voting on other ballot measures.*

6. *Order effects.* There seems to be good reason to believe that the amount of abstention an item garners depends in part on its position on the ballot and on the items surrounding it. Noncontroversial propositions which can be expected to have relatively low abstention rates seem to have higher ones if they are placed among propositions with high expected abstention rates, while measures which immediately follow intensely fought propositions seem to have lower abstention rates than would be expected on other grounds. For example, the abstention on propositions 7, 10, and 12 (see

Data derived from Table 5.7 in Richard F. Carter, "Voters and Their Schools," (Technical Report, Institute for Communication Research, Stanford University, Stanford, California, June, 1960), p. 173. Additional information from a personal communication from Mr. Carter.

Table 2) was largely bestowed by voters who skipped over the whole bloc of noncontroversial propositions. Very few of the voters who voted on all but one or two of the propositions selected any of these three measures for their abstention. Therefore had the propositions been positioned, say, among the bonds or immediately after proposition 14 their aggregate abstention probably would have been much lower. Only five ballot measures before 1964 received such low abstention as propositions 15, 16, and 17 and it is doubtful whether either the historical trend in abstention (see Section II) or the subject matter of these propositions fully accounts for these rates. Much of the voting on these measures probably was encouraged by their position immediately after the intensely-fought proposition 14.

Some insight into order effects can be gained by briefly looking at a curious pattern in the votes for three nonpartisan races on the ballot: two contested judicial seats and the race for district attorney. The names of the two candidates in each of these races were rotated from district to district so that each candidate was listed first on about half the ballots in the county and second on the other half. It is a fairly well established fact that on paper ballots the candidate who is listed first in low visibility races has a definite advantage.[28] And, in the present case, this finding is seen to hold, for each candidate received a vote six to eight percentage points higher when he was listed first than when he was listed second.[29] However, examining the ballot data from the absentees (who largely missed the belated DA campaign) it was found that those who voted consistently for the first (or last) candidates in the two judicial races were *not* particularly likely to persist in that idiosyncrasy on the district attorney race which was next on the ballot. In fact, they were rather inclined to *reverse* their pattern of selection: those who had voted for two *first*-listed judges tended slightly to vote for the *last*-listed district attorney candidate and vice versa. The gain for the first position in the district attorney race, curiously enough, is contributed almost entirely by those voters who abstained on the judicial races but voted for a DA candidate.

[28] See Henry M. Bain and Donald S. Hecock, *Ballot Position and Voters Choice* (Detroit: Wayne State University Press, 1957); and Howard White, "Voters Plump for First on List," 39 *National Municipal Review* 110–111 (February, 1950).

[29] The names of the candidates in the presidential contest were also rotated. It is noteworthy that order had *no* effect on this high visibility race.

Thus a significant number of those voters who voted straight for the first or last position on the two judicial races seem to have become "tired" of it by the time they came to the district attorney race and thus were rather inclined to vote the other way, purely, it would seem, out of a desire to mix up their voting pattern. The abstainers, however, were unencumbered by any previous voting patterns and thus were more willing to let the innate preference for the first position affect them. In an array of races of equally low visibility, then, it is to be expected that ballot position would be most important on the first race or two, but would have an increasingly small impact on the races further on. In such a situation the candidates placed last on some of the later races in the array may be at no disadvantage whatever.

One implication of this conclusion is of special interest for the analysis of proposition voting: *on items of low visibility, voters seem to have a tendency to avoid over a long stretch a pattern of behavior which is obviously uniform.* This generalization may help to explain why such a large percentage of the vote on the propositions is idiosyncratic.[30]

The state legislature devoutly believes in the existence of a body of citizens who start out voting affirmatively on bond issues but turn to negativism as they move down the ballot viewing with mounting horror the extent of the proposed expenditures. Part of the reason for placing state bond issues at the top of the ballot is to catch the affirmative votes of these citizens before they turn sour.

Analysis of the ballot data indicates that there is indeed a small but significant number of voters, 7 percent in each ballot group, who begin by voting affirmatively on the bonds but turn negative before they reach the end. The trouble, however, is that there is an equal and opposite number of voters who start out voting

[30] Campbell and Miller have noted that when tickets are split, such splitting usually occurs at the state and local level: "The Motivational Basis of Straight and Split Ticket Voting," this REVIEW, 51 (June 1957), 293–312. At least part of this splitting may be done by voters who become embarrassed, one might say, by the fact that they have voted a straight party line on the national offices. Thus they are inclined to break with the party on the less visible local races simply because of a semi-capricious urge to mix up their vote pattern. Perhaps this somewhat tenuous line of reasoning could be used to explain in part why some cities with Democratic registration majorities elect Republicans to positions of local leadership.

negatively and end up on the favorable side! Important in this consideration of course is the subject matter of the bonds. Things might look entirely different in another election—or even in this one had the bonds appeared in a different order.[31]

7. *Negativism, alienation, and the boob.* One of the more striking discoveries in the examination of the ballots—even though they were largely from presumably middle class voters and even though 1964 was a notably "affirmative" year—was the low amount of negativism. *Only one of the 1,293 ballots was marked negatively for all the propositions.* There were *no* ballots marked negatively for eighteen of the nineteen propositions and only fourteen voters representing barely 1 percent of the combined sample voted against as many as fifteen of the nineteen propositions. Massive disaffection seems to have been missing.

Proponents of the concept of alienation frequently argue that alienation in the vote sometimes will be associated with negativism on ballot measures, although they usually argue that the negative voting will occur only on certain measures—ones in which a hostility to the political order can be clearly expressed. Specifically, students of the problem have suggested that alienation accounts in part for negative voting on bond issues, fluoridation measures, and proposals for metropolitan reorganization.[32] It does seem however, that if many voters really find voting to be "meaningless" and the "electoral process to be a sham,"[33] a few of them even in this sample should express this utter disgust by a blanket negative vote.

There were two groups of propositions on which a negative vote could be expected to express for some people an inner disenchantment with the political system. One comprised the noncontroversial propositions, numbered 4 through 12. As noted above, all of these were constitutional amendments placed on the ballot by the legislature and therefore, by implication at least, approved and endorsed by the political establishment. The other consisted of the bond issues.

On the noncontroversial propositions only *five* voters, comprising less than 0.4 percent of the combined sample, voted negatively on all nine. If the two veterans' issues are excluded (propositions 4 and 5) the amount of total naysaying is still less than 1 percent of the sample. If those who voted negatively on six of the seven measures are included in the group, the number rises to 3 percent.

There are many reasons for voting against all the bonds: a conservative outlook on spending, a resentment against high taxes, a preference for pay-as-you-go financing, caprice, and an ill-humor on election day, as well as alienation. Yet only one out of every ten or eleven ballots was marked negatively on all the bonds.

Negativism on three uncontested judicial races (voters could only approve or disapprove continuance in office) is also worth examination in this connection. Some writers find that a "large portion of the electorate," the "average voter" in fact, feels that voting is meaningless and that politicians are "selfish" and "irresponsible." These feelings are found not to be restricted by time, circumstances, or place: they "go beyond resentment toward the particular candidate in this election; they indicate a widespread disgust and disillusionment with the political process and politicans in general."[34]

If such "widespread disgust" existed it surely could be expected that a strong protest would have been delivered in a vote against the judicial political process. But nothing of the sort materialized. No more than 8 percent of the electorate in the county voted against any one of the judges. Of the absentee ballots examined (judicial voting data were not gathered in the precinct) only 4 percent were marked against all three judges.[35] The small amount of

[31] For other discussions of order effects on proposition voting see Key and Crouch, *op. cit.*, pp. 536–538; Joseph G. LaPalombara, *The Initiative and Referendum in Oregon: 1938–1948* (Corvallis: Oregon State College, Studies in Political Science, No. 1, 1950), pp. 95–97; and James K. Pollock, *The Initiative and Referendum in Michigan* (Ann Arbor: Michigan Governmental Studies No. 6, Bureau of Government, University of Michigan, 1940), pp. 45–49.

[32] Horton and Thompson, *op. cit.*, Wayne E. Thompson and John E. Horton, "Political Alienation as a Force in Political Action," 38 *Social Forces* 190–195 (March, 1960); William A. Gamson, "The Fluoridation Dialogue: Is It an Ideological Conflict?" 25 *Public Opinion Quarterly* 527–537 (Winter, 1961); Arnold Simmel, "A Signpost for Research on Fluoridation Conflicts: The Concept of Relative Deprivation," 17 *Journal of Social Issues* 26–36 (no. 4, 1961); Edward L. McDill and Jeanne C. Ridley, "Status, Anomia, Political Alienation and Political Participation," 63 *American Journal of Sociology* 205–213 (September, 1962); and Gene L. Mason and Dean Jaros, "Alienation and Support for Demogogues," 1 *Polity* 479–500 (Summer 1969).

[33] See Levin, *op. cit.*, p. 58.

[34] *Ibid.*, p. 61.

[35] On the basis of an "unstructured questionnaire" one would expect far more pure negativism

negativism (and even part of that surely is the result of caprice rather than alienation) is far surpassed by an overwhelming vote of confidence in the governing group. The advantage of incumbency in most political races is perhaps a further example of this confidence.

These data suggest therefore that, *while some voters may be alienated, the phenomenon is less than rampant among the electorate at large.* In addition they lend no support to the theory of the "boob" voter. It is frequently argued, especially in debates over direct legislation, that there exists a significant number of voters who respond according to the philosophy, "I always vote no, and I'm usually right."[36] A. J. Pillsbury, a rather bombastic California analyst of these matters has defined the boob as the citizen "who votes 'no' right down the line of measures submitted without shadow of knowledge or reason"; he was, Pillsbury assures us with precision, "abroad in this fair land as far back as 1862 and averaged 15.22 percent of the total vote on the measures throughout the period from 1849 to 1879."[37] The ballot data, even considering the affirmative mood of the electorate in 1964, strongly dispute the existence of such a large group.[38]

Data were also gathered on proposition voting in two local elections in Los Angeles in April and May, 1965. Ballots from two precincts were examined in the April election, 277 in all, and one of the precincts was revisited in May and information from 70 of the 101 ballots voted was gathered.

A most striking finding is that total negativism on noncontroversial propositions, though far from massive, was distinctly higher in the April election than it had been in November and than it was in the May election. Fully 4 percent of the voters in the quite substantial April sample voted negatively on all nine propositions while, as noted above, less than one half of 1 percent of the November ballots were so marked on the nine noncontroversial propositions. Furthermore, only one of the seventy ballots examined in the May election was marked negatively on all the ten propositions and no other ballot had more than seven marked negatively. This phenomenon would be entirely unexpected from an analysis of aggregate data, for all of the April propositions passed handsomely while one in November and two in May failed of passage and one or two others barely slipped through.

It is of cource possible that these negative votes were contributed by an enlarged turnout in April of the alienated, belated groundhogs who emerged for only a day to growl and snap, then to return to their lairs. However, a somewhat different explanation can be proffered. It might be said that voters follow what could be called the "categorical imperative" of voting: they vote on that maxim whereby they can at the same time will that it should become a universal—or at least statewide—law. In other words, they would like to see their vote selections prevail in the election. On several of the items on the 1964 ballot, however, the "categorical imperative" may not fully apply. These were the uncontested judicial races the outcome of which was such a foregone conclusion that one could vote negatively without any real fear that one's "wishes" would carry. Thus one could safely succumb to caprice or express rather harmlessly a protest against the system. The April propositions were also of this

than this. See I. A. Berg and G. M. Rappaport, "Response Bias in an Unstructured Questionnaire," 37 *Journal of Psychology* 475–481 (October, 1954). Historical evidence, incidentally, suggests that the amount of negative voting on these judicial races may be a fairly good indicator of electoral discontent. Much larger, though still far from dominant, portions of the electorate voted against the judges during the depression than have so since that time—except for the campaign of 1966 noted above. (See note 11.)

[36] An antifluoridation voter quoted in William Attwood, "Fluoridation: Why All the Controversy?" *Look* (June 24, 1958), p. 23. See also Dewey Anderson, *California State Government* (Stanford University, 1942), p. 205. For a discussion and some analysis, see Waldo Schumacher, "Thirty Years of People's Rule in Oregon: An Analysis," 47 *Political Science Quarterly* 251 (June 1932); Harold F. Gosnell and Margaret J. Schmidt, "Popular Law Making in the United States, 1924–1936," in *Problems Relating to Legislative Organization and Powers*, Volume VII in a series published by the New York State Constitutional Convention Committee (Albany, 1938), p. 317; Key and Crouch, *op. cit.*, p. 536; and Pollock, *The Initiative . . .*, p. 50.

[37] "Participation of the People in Their Government," 7 *Transactions of the Commonwealth Club of California* 450–451 (No. 11, March 3, 1931).

[38] At least for the middle class. Poorer voters may express greater blanket negativism, but a brief analysis of precinct results suggests that voting on noncontroversial propositions in lower class neighborhoods shows more abstention (as the analysis in Section II above would predict), but no more negativism.

sort. They were without opposition, undiscussed, so trivial in content that the *Herald-Examiner* did not even bother to publish vote recommendations on them. And, almost mechanistically, precisely the same amount of total negativism (4 percent) is found on these measures as was found on the uncontested judicial races in November.

This may in part be alienation to be sure. But it is a rather mild, almost cowardly, utterance of disgust and, at 4 percent, hardly a threat to the system—especially since it finds expression only when it is likely to have no effect.

8. *Yeasaying and naysaying.* While the discussion above suggests that bloc negativism, whether from alienation or "boob" voting, is hardly endemic in the population, it is not to be denied that some people are inclined to be more affirmative and some more negative than others. It was noted above that virtually all correlations among the propositions were positive (if low), suggesting that supporters and opponents of a given proposition are more likely to pick the same option on any other proposition than they are to reverse themselves. Such "yeasaying" and "naysaying" is due to a generalized favorableness or unfavorableness toward the propositions as well as to the psychological phenomenon known as response set—the tendency for some people to select positive or negative options regardless of item content.[39]

In the ballot data, with their middle class bias, people inclined to favor the noncontroversial propositions tended to vote Democratic while the relatively small band of naysayers voted Republican disproportionately.

Yeasaying and naysaying are not found to be equally effective on all issues; they are affected at least in part by the prominence of the issue. Where controversy and interest are minimal yeasayers and naysayers will let their partly capricious biases prevail. Where the debate is noisy and the electorate relatively informed, however, reason, ideology, and psychological identification tend to dominate. For example, those inclined to approve or oppose the continuance in office of the three uncontested judges tended to persist in this voting pattern on the noncontroversial propositions, but not on the controversial ones.[40]

VI. SUMMARY AND CONCLUSIONS

This analysis of ballots has found little evidence to support the view that newspaper recommendations strongly affect the vote on the propositions. Nor can support be found for the suggestion that there exists a large block of voters intent on voting negatively on everything. For the most part voting on the propositions is a strikingly idiosyncratic process, although campaigning, especially if it is by political parties, does seem to structure opinion on those few propositions on which it is conducted and cohabitation has a noticeable, but quite limited, impact. It has also been possible to make some comments on the relevance of order and of yeasaying and naysaying to the proposition vote.[41]

[39] See Arthur Couch and Kenneth Keniston, "Yeasayers and Naysayers: Agreeing Response Set as a Personality Variable," 60 *Journal of Abnormal and Social Psychology* 151–174 (March 1950); Bernard M. Bass, "Development and Evaluation of a Scale for Measuring Social Acquiescence," 53 *Journal of Abnormal and Social Psychology* 296–99 (November 1956); and Lee J. Cronbach, "Further Evidence on Response Sets and Test Design," 10 *Educational and Psychological Measurement* 3–31 (Spring 1950).

[40] This conclusion fits well with Cronbach's observation with respect to educational testing that "Response sets become most influential as items become difficult or ambiguous." *Op. cit.*, p. 3. It was noted above that there were also on the ballot two contested judicial races where the order in which the candidates were listed was rotated from district to district. There was little tendency for yeasayers to select the candidates listed first. Therefore for the most part yeasayers seem to pick the "yes" option not because it is listed first, but rather because they really are favorable in some sense toward the ballot item. A similar finding occurs in a study of response sets where the choice of the "yes" and "agree" responses were not particularly affected by the order in which the response options were presented. Berg and Rappaport, *op. cit.*

[41] These considerations, of course, are by no means exhaustive. Among the many factors left unconsidered, for example, is the effect on the vote of the way the proposition is worded on the ballot. One survey found that attitude on an important bond measure (not just an obscure technical proposition) apparently was sharply altered when confusion about its intent was cleared up. See Dwaine Marvick, "Electoral Opinion in the 59th Assembly District, Los Angeles, June 1962," (Falk Foundation Program of Political Training and Research, University of California, Los Angeles, Department of Political Science, 1962).

It also seems likely that the voters booklet, which is sent out to all voters several weeks before the election, can be quite important in in-

Hopefully, this analysis of a set of ballots, very limited in number and in class origin, will help to suggest the value of such data in voting research. In the last few years the popularity of punch card ballots has greatly increased, which means that millions of ballots, voted by all segments of society, are now becoming available in highly convenient form. An exploitation of this vast source of data could serve to broaden and clarify considerably the conclusions reached here.

fluencing opinion, at least on the noncontroversial propositions. Fragmentary evidence to support this conclusion was found in the present study. At one point twenty-two judges were asked to select from proposition 6 through 12 the measures which they felt would appear to the "average voter" to be most unobjectionable. While their ratings generally reflected the statewide vote on these propositions, on propositions 11 and 12 the fit is quite poor. Proposition 12, which authorized the giving of tax relief in disaster areas proclaimed by the governor, is found by the judges to be easily the most unobjectionable of all—after all who could be in favor of disaster? Yet this proposition did less well in the state (66 percent favorable) than three other of the noncontroversial propositions. (As seen in Table 2 the proposition did somewhat better in Los Angeles county as it did in other areas which had recently undergone disasters—thus suggesting a certain amount of rationality in the vote.) Proposition 11, which did very well (73 percent favorable) despite a low unobjectionableness score, reads very poorly on the ballot. It sought to delete the requirement that the city electorate must approve when it is proposed that municipal functions be carried out by county officers; thus it appears to take power away from the voters.

It seems very likely that this anomaly in which a highly unobjectionable proposition does worse than a far more objectionable one can be explained by reference to the voters' booklet. There the lone favorable argument for proposition 11 points out that such city-county arrangements have been carried out hundreds of times without voter approval in the past and thus passage of the proposition would only recognize and legalize an efficient arrangement that is already in effect. This fact is not mentioned in any way in the ballot wording. The opposing argument to proposition 12 in the booklet also stresses an implication which is not at all obvious from the ballot wording: disaster tax relief would only go to those who suffer in an officially proclaimed disaster area and thus the measure discriminates against those who lose in small, nonsensational disasters. (If it is the case that the absentee is more likely than the voter in the precincts to become separated from his copy of the voters booklet, these considerations might also explain why the absentees differ with other voters on these two propositions as seen in Table 2.)

It is noteworthy in this regard that the propositions without opposing arguments in the voters booklet, 6, 9, and 11, tend to correlate with one another among the ballot data at somewhat higher levels than do other pairs of noncontroversial propositions. The three also correlate comparatively well with proposition 13. This linkage is interesting for the arguments on proposition 13 in the booklet are rather unusual: specifically, the argument which purports to be against the measure has little to do with the subject of the proposition but rather is a tirade against the lottery as presented in proposition 16! Thus 13 is rather like 6, 9, and 11 in essentially having no negative argument in the booklet.

For assessments of the value of the voters booklet in informing and influencing opinion, see La Palombara, *op. cit.*, p. 119; Key and Crouch, *op. cit.*, p. 532; John E. Mueller, "The Politics of Fluoridation in Seven California Cities," 19 *Western Political Quarterly* 62–63 (March 1966); and especially Stanley Kelley, Jr., *Political Campaigning: Problems in Creating an Informed Electorate* (Washington, D. C.: Brookings Institution, 1960), pp. 41ff.

PARTY, IDEOLOGY, AND THE CONGRESSIONAL CHALLENGER*

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Political Science, like Leo Durocher, is not distinguished by an excessive concern with losers.[1]

Although the reasons are not difficult to fathom, theories of recruitment and representation which omit systematic examination of those who are rejected, in addition to those selected, will only imperfectly comprehend the political conditions and consequences this process implies.[2]

This study, part of a broader cross-national examination of legislative recruitment,[3] focuses on a largely-ignored but perennial figure among the ranks of American political losers: those candidates who challenge incumbents for seats in the House of Representatives.

Few elective institutions in American political life have achieved the degree of stability which since the depression has characterized membership in the House. At no time since 1932 has the percentage of first-term members exceeded 26 percent and in the last 15 years, it has been rare when more than 15 percent of those incumbents who actively sought reelec-

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[1] Relatively more attention has been devoted to losers other than the common electoral variety, particularly the (old) revolutionary left and radical right, although the normative assumptions and methodological adequacy of such research are now a matter of intense dispute. Whatever minor problems might be created by attaching "loser" to revolutionary and/or radical is unintentional; the term is used in the electoral sense and neither group has been conspicuous for its success. However, research is beginning to appear more frequently on electoral losers. Frank Sorauf's study of legislative recruitment in Pennsylvania, *Party and Representation* (New York: Atherton Press, 1963), included social and political background data on winners and losers, as did Lester G. Seligman's analysis of candidates in Oregon, "Party Structure and Legislative Recruitment," this REVIEW, LV (September, 1961), 77–86. More recently, others have focused on the perception of winning and losing candidates toward voters, seeking to ascertain how politicians come to terms with electoral defeat or success. See, for example, John Kingdon, "Politicians' Beliefs About Voters," this REVIEW, LXI (March, 1967), 136–147, and his expanded treatment in *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1968). The substantial barriers associated with acquiring campaign resources—organization, money, information—is explored in a comparative case-study of ten Northern California congressional races by David A. Leuthold, *Electioneering in a Democracy* (New York: John Wiley and Sons, 1968). And, the Survey Research Center's representation and Congress study, under the direction of Warren Miller and Donald Stokes, sampled both incumbents and nonincumbents, but no work has been published which systematically compares these two groups except on occasional indicators of candidate salience; e.g., whether nonincumbents who live in the same community as voters are recognized more often. See their two articles, "Party Government and the Saliency of Congress," *Public Opinion Quarterly* (Winter, 1962), 531–46, and "Constituency Influence in Congress," this REVIEW, LVII (March, 1963), 45–56.

[2] Lester G. Seligman, "Political Leadership: Status Loss and Downward Mobility," (paper delivered at the 1966 Annual Meeting of the American Political Science Association, New York, September 2–6, 1966). Lewis Edinger's study of Kurt Schumacher, the post-war German SPD leader, is a brilliant example of an analysis which explores the conditions which made Schumacher a powerful success within the party, but a tragic failure in the West German political system, *Kurt Schumacher: A Study in Personality and Political Behavior* (Stanford: Stanford University Press, 1965).

[3] For the larger study, see my *Parties, Candidates and Recruitment: West Germany and the United States* (unpublished Ph.D. dissertation, University of California, Los Angeles, 1969).

tion were defeated.[4] The 1964 election was considered a striking anomaly when "only" 80 percent of the membership returned to the 89th Congress. It is not an exaggeration to concede that, collectively, congressional challengers stand somewhere between the Harold Stassens and Norman Thomases of campaign history in their impressive disregard for the first objective of American major-party politics: winning. The consequences of such low turnover on the internal structure of the House, as Nelson Polsby has recently argued,[5] are certainly responsible in part for the increasing institutionalization of this body, but the external consequences seem less demonstrable.[6]

Understandably, if regrettably, most studies of legislative recruitment begin with the members of a legislature and then trace back, directly or inferentially, to the sources and causes of their selection, treating career paths, socialization, and party support as each contributes to political success.[7] Herbert Jacob and Rufus Browning have both pointed out that very often these efforts are in reality studies of political promotion, not initial recruitment,[8] a persuasion increasingly shared by other recent models of political recruitment.[9]

Research which is primarily directed toward assessing the impact of the recruitment process on the functioning of a legislature overlooks a large variety of "outside" phenomena (communication, participation, interest articulation and aggregation, etc.) to which that process is intimately joined. By focusing on the nonlegislative side of recruitment, candidacy becomes a vehicle, in David Easton's terms, for demand articulation and communication feedback.

Therefore, the primary but not exclusive focus here will be on those who are screened *out* of elective office and one major consequence this suggests for the structure of opposition *outside* the legislative environment.[10] Since few

[4] An excellent and comprehensive study of congressional elections since 1920 can be found in Milton G. Cummings, Jr., *Congressmen and the Electorate* (New York: The Free Press, 1966). See also, Charles O. Jones, "The Role of the Campaign in Congressional Elections," in M. Kent Jennings and L. Harmon Zeigler (eds.), *The Electoral Process* (Englewood Cliffs: Prentice-Hall, 1966), particularly pp. 21–27.

[5] "The Institutionalization of the U. S. House of Representatives," this REVIEW, LXII (March, 1968), 144–169.

[6] For some possibilities, see Polsby's speculative comments in his *Congress and the Presidency* (Englewood Cliffs: Prentice-Hall, 1964).

[7] This is equally true of the older social background studies, examined by Donald Matthews in his *The Social Background of Political Decision-Makers* (New York: Random House, 1953); the excellent career-line study by Joseph Schlesinger, *Ambition and Politics* (New York: Rand, McNally, 1966); and those using social-psychological categories, exemplified by John Wahlke, *et al.*, *The Legislative System* (New York: John Wiley & Sons, 1963); or James Barber, *The Lawmakers* (New Haven: Yale University Press, 1965). It is less relevant to the work of Sorauf, or Seligman, *op. cit.*

[8] Herbert Jacob, "Initial Recruitment of Elected Officials in the U. S.—A Model," *Journal of Politics* (1962), p. 708; and Rufus P. Browning, "The Interaction of Personality and Political System in Decisions to Run for Office," *Journal of Social Issues* (June, 1968).

[9] Among which see Lester Seligman, "Recruitment in Politics," *PROD*, (1958), 14–17; Jacob, *op. cit.*; Leo M. Snowiss, "Congressional Recruitment and Representation," this REVIEW, LX (September, 1966), 627–640; Allan Fiellin, "Recruitment and Legislative Role Conceptions: A Conceptual Scheme and Case Study," *Western Political Quarterly* (June, 1967); and David Schwartz, "Toward a Theory of Political Recruitment" (paper presented to the 1967 Annual Meeting of the American Political Science Association, September 4–8, Chicago). Moreover, Rufus Browning has been in the process of developing a simulated model of recruitment, applicable to various groups in the political system, part of which is outlined in his "Hypotheses about Political Recruitment: A Partially Data-Based Computer Simulation," in William Coplin (ed.), *Simulation in the Study of Politics* (Chicago: Markham Publishing, 1968).

[10] Two other consequences were considered in the original study. First, it was anticipated that a modified "social ladder" process would be operating on the structure of recruitment reflecting an openness in the character of access not evident by looking only at the established (incumbent) leadership in each party; i.e., greater proportions would be black, female, socially-mobile, urban-born, and drawn from ethnic and religious minorities. While small cracks in the wall of white, Protestant, middle-class dominance did appear, these are not apparently preludes to a flood. Challengers, like their opponents, tend to reflect the high-status character of American political leaders. Second, ethnic, religious and more recently, racial balancing is a phenomenon long evident in the electoral strategies of each party and one would expect nonincumbent candidates to "mirror" the opposition, or at least depart significantly from their party's "normal"

electoral opportunities are available in a large majority of districts, what alternative function (to realistically competing for seats) might in fact be performed by the party apparatus and the 350-or-so candidates headed for certain defeat? The broad proposition entertained in this study, one suggested by Samuel Huntington 19 years ago[11] and recently modified by Herbert McClosky and others[12] who have returned to a concern with the role of ideology in American politics, is that campaign losers tend to articulate and sustain a coherent partisan ideology in the face of an electorate for whom such matters are still inchoate or irrelevant, or both.[13]

Thus despite substantial countervailing electoral pressures, nonincumbent candidates may in fact provide an indispensable communications network—cemented by a coherent if impure ideology—which sharpens rather than blunts party conflict, intensifying rather than modifying ideological differences.[14]

The intention here is to examine these tendencies empirically and under varying conditions. Four independent variables will in turn be related to ideology, examining the conditions under which candidates move toward or away from, are more or less "cemented" by, the liberal-conservative positions normally associated with each national party. The problem of party unity and electoral outcome is then considered, tracing the fate of the few 1964 nonincumbent winners in the 1966 mid-

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social character. Some evidence did indicate that this was occurring among small numbers of Republican challengers, but, and significantly, GOP candidates from social minorities were almost without exception defeated. All the Blacks (5), all the educators, all those under 30, most of the upwardly mobile, almost all the Catholics, and all the Jews went down to defeat. Nor was this an artifact of the unusually bad Republican year in 1964. It is precisely in those districts where Republican nonincumbents tend most often to mirror the minority status of their Democratic opponents that they stand, statistically, the least chance of winning. This suggests that the electoral system is partially reinforcing those social cleavages which since the depression have divided the two parties, even though these divisions are *not* reflected among leaders to the same degree as among followers.

[11] "A Revised Theory of American Political Parties," this REVIEW, XLIV (1950), 669–677. Unlike Huntington's, this paper does not propose that the most ideologically conscious tend to emerge precisely in those districts where they would stand the best opportunity of winning, but rather that a coherent party ideology is not suspended by a majority of each party's candidates simply because the structure of party competition would permit possible electoral victory. This point is developed further above.

[12] Besides McClosky's "Consensus and Ideology in American Politics," this REVIEW, LVIII (June, 1964), 361–382; and McClosky, Paul J. Hoffmann and Rosemary O'Hara, "Issue Conflict and Consensus Among Party Leaders and Followers," this REVIEW, LIV (June, 1960), 406–427, a sample would include, Dwaine Marvick and Charles Nixon, "Recruitment Contrasts in Rival Campaign Groups," in Marvick (ed.), *Political Decision-Makers* (Glencoe: The Free Press 1963); Robert Hirschfield, *et al.*, "A Profile of Political Activists in Manhattan," *Western Political Quarterly*, XV (September, 1962), 489–497; Edmund Costantini, "Interparty Attitude Conflict: Democratic Party Leadership in California," *Western Political Quarterly* (1963), 956–972: and three recent books, Samuel J. Eldersveld, *Political Parties: A Behavioral Approach* (New York: Rand, McNally, 1963); Robert Agger, *et al.*, *The Rulers and the Ruled* (New York: John Wiley & Sons, 1964); and Harry M. Scoble, *Ideology and Electoral Action* (San Francisco: Chandler, 1967).

[13] The concept "ideology" has undergone such massive variation in use that discriminating observers will probably blanch at its intrusion here. Yet the liberal-conservative continuum has proved a useful organizing device for (some) political activists and (some) political scientists and is, Philip Converse suggests, " . . . a serviceable yardstick for simplifying and organizing events in most Western politics." It shares many attributes of formal ideologies, despite substantial change in particular issues and repeated efforts to refine by adding "corporate" or "radical" before either of these positions on the continuum. See his persuasive argument, "The Nature of Belief Systems in Mass Publics," in David Apter (ed.), *Ideology and Discontent* (Glencoe: The Free Press, 1964), p. 214. See also David Minar, "Ideology and Political Behavior," *Midwest Journal of Political Science* (November, 1961), 317–331; William Connolly, *Political Science and Ideology* (New York: Atherton Press, 1967), pp. 2–3; or Robert Lane, *Political Ideology* (Glencoe: The Free Press, 1962), pp. 13–16.

[14] There is no logical contradiction between a politics which is simultaneously "ideological" *and* "brokerage" although many elements of the "new" and "old" left would deny this, as would most scholars (e.g., Daniel Bell) who take as their model the classic struggles of the European left and right.

term elections. Finally, the study considers the significance of these findings for patterns of campaign behavior, recruitment and party functions.

I. PROCEDURES AND OPERATIONAL MEASURES

Most of the data collected and analyzed below were generated through a questionnaire mailed to all nonincumbent candidates for the House immediately after the 1964 election. Of 388 nonincumbents running in that election, 76 percent completed and returned the questionnaire, a response rate sufficiently high to justify the analysis undertaken here.[15] Selection of a mail questionnaire as the primary research instrument was based on the conviction that carefully administered questionnaires can be almost as efficient (in terms of overall response rate) as direct interviewing, usually are mandatory when the researcher is confronted with sizable geographical dispersion of target groups, and are infinitely cheaper if he is interested in securing data on a large number of respondents.[16] These data were later supplemented by open-ended but structured interviews with 69 of the 83 nonincumbents elected in 1964.

Among others, David Minar has forcefully argued that a substantial problem for political behavior research on ideology is empirical and operational.[17] The indicator employed throughout most of this study, a widely-used and simple self-anchoring index of liberalism-conservatism, enjoys some distinct advantages but is also subject to certain limitations.

The alternative most frequently suggested involves scaling individuals on the basis of responses to specific issues. By deriving cumulative or summated scale scores on a sample of issues, one is able to rank-order persons in a rough but reliable fashion and, with Guttman techniques or factor analysis, explore the in-

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[15] This rate does not compare unfavorably with that reported for the Survey Research Center's national election studies (85 to 86 percent), is higher than James Barber's rate for Connecticut legislators (65 percent), somewhat lower than Leon Epstein's study of Wisconsin legislators (83 percent), but considerably lower than the range reported by John Wahlke, Heinz Eulau, William Buchanan and LeRoy Ferguson for their four-state legislative study (91 to 100 percent).

[16] Initially, the study was designed to include incumbents, but it seems clear that congressmen have been so deluged by mail questionnaires, from so many sources that continued use of "mailers" (and in this I agree with Lewis Dexter) will be destructive of other kinds of necessary research; i.e., via personal interviews. The same judgement does not, however, apply to nonincumbents, nor to a large variety of highly active partisans in American society. Others have shown that mail survey research techniques are not subject to the usual problems of abnormally high non-response rates where the population is skewed away from the normal adult population in terms of income, education, and political activity. See Harry M. Scoble (with Stanley Bachrach), "Mail Questionnare Efficiency: Controlled Reduction of Non-Response," *Public Opinion Quarterly* (Summer, 1967), 265–272, and William Crotty, "The Utilization of Mail Questionnaires," *Western Political Quarterly*, XIX (March, 1966), 44–54. Basically, two techniques are available to increase the researcher's confidence that results from a mail questionnaire are reliable (from the viewpoint of response rate only). First, as Scoble has suggested, one should anticipate resistance and design procedures (advance mailings, personalized appeals, special delivery postage, diversified appeals in follow-ups) in advance to overcome it. Second, some technique for gaining information on the non-response should be introduced (for example, a number keying each questionnarie to an individual) so that statistical tests can be undertaken to enable the researcher to comparatively and partially measure non-response error where these tests are appropriate; that is to say, if one is attempting to inductively generalize from a sample to a finite population. However, where an effort is made at enumeration, and neither simple nor stratified random sampling procedures are employed, it is clearly inappropriate to use statistical tests associated with sampling methodology to estimate non-response error. This study, which was an attempted enumeration, does not rest on any misguided (or misleading) statistical tests estimating the probable bias created by non-response. On the other hand, since information on the non-response was collected, three critical baselines were established by which to compare those who responded with those who did not: party, region, and constituency characteristics. Differences between the response and non-response groups on these three factors did not exceed 2 percent, an encouraging if not completely reassuring finding. The sources of bias in survey research always involve much more than sampling problems (question construction, coding, interpretation, etc.) and every effort was made to reduce these sources, as well as those stemming from non-response.

[17] Minar, *op. cit.*, pp. 329–331.

ternal consistency of various opinion clusters.[18] Since data on the candidate's orientation toward specific issue-areas was obtained in this study, and, as later analysis shows, the relationship between self-classification and these items was strong but far from perfect, a Guttman scale was also attempted. Using the Cornell technique, five items were combined,[19] but neither the coefficient of reproducibility (.82) nor of minimal marginal reproducibility (.52) met minimum standards of reliability. One of the five issue-areas (civil rights) was then dropped, some item "shuffling" undertaken, and the other four recombined. Each related to domestic economic orientation. Although the coefficient of reproducibility improved (.85), it still did not reach a satisfactory minimum. Hence cumulative scales are not used in the analysis.

Quite apart from questions of technical reliability, earlier examination on the Guttman scale and on a scale of Likert-type summated liberalism suggested some rather severe problems of validity with both these "objective" ideological measures. Each distinguished reasonably well between Republican conservatives and *all* Democratic challengers, but, particularly among Democrats, neither sufficiently tapped what appear to be genuine *intra*-party differences. Large majorities of the latter, whether considering themselves "liberal" or "middle of the road" clustered in the upper three positions of each scale.[20] Yet intuitively these two different psychological reference points suggest important distinctions among the candidates, distinctions that merit empirical examination.[21] Because I am centrally concerned with ideological conflict within as well as between the two parties, it was necessary to opt for a measure that does discriminate, however imperfectly, between candidates.

Previous research has demonstrated that the highly educated and politically sophisticated are much more capable than the typical citizen of organizing a variety "... of idea-elements into more tightly constrained wholes."[22] Thus one can anticipate that abstractions like "liberal" and "conservative" are operationally more reliable as devices for classifying and predicting the behavior (including opinions) of the highly active in American politics. The fact that substantial variation does exist—whether one uses objective or subjective tests—underscores the complexity, the lack of complete unidimensionality, and the tentativeness of these and other simplified ideological dimensions. Certainly self-classified liberals or conservatives are going to possess opinions that will be interpreted differently by different observers in different environments. But a similar problem applies equally to other mea-

[18] The use of factor analysis with attitude data is rejected by many statisticians because social science has not yet created attitude scales which meet interval assumptions of measurement. Lewis Guttman and James Lingoes have developed a new set of programs for multivariant analysis on ordinal level data—smallest space analysis—which may enjoy increasing use in attitude research. For an example, see Glendon Schubert, "Ideological Distance: A Smallest Space Analysis Across Three Cultures," *Comparative Political Studies*, I (October, 1968), 319–347.

[19] Each related to the conventional question of federal domestic intervention and were directed at the following areas: regulating business; providing medical care for the aged; providing aid to assist public education; providing help for the poor generally; engaging in programs to assist Negroes and other minorities. See Table 2 for the exact format.

[20] On an eleven-point Guttman rank-ordering, 86 percent of the Democratic challengers scored one, two, or three.

[21] That these questions did not reveal substantial intra-party conflict in the fall of 1964 among Democrats suggests some interesting problems. Democratic nonincumbents were almost exclusively non-southern Democrats and one would expect maximum homogeniety among this group on general questions relating to economic intervention, particularly in 1964. The Vietnam war had not really surfaced as a divisive issue, although one year later my interviews with freshmen showed that some were becoming increasingly restless as American involvement grew more massive. However one might feel about events surrounding the 1968 campaign, and the impact of "new politics" on the stability of the New Deal coalition, it seems clear that many of the issues political scientists have been using for the past twenty years are undergoing as yet unpredictable modification. The situational character of all our operational measures reinforces the need for developmental studies, perhaps using a modified panel technique, on the way politicians adjust "old" labels to "new" issues or vice-versa.

[22] Converse, *op. cit.*, p. 229. McClosky has also provided some evidence along these lines, noting the strong association between scale scores and self-classification for leaders, but not followers, in his "Consensus and Ideology in American Politics," *op. cit.*, p. 375.

sures of ideology. The qualification is no more, nor less, applicable to alternative operational criteria. When used in conjunction with other measures, the self-anchoring index provides a standardized and serviceable tool for large-scale research on the attitude structure of political activists.

II. IDEOLOGICAL PROFILES

It is perhaps a discomforting irony that at precisely the moment specialists on American political parties have returned to empirically documenting the extent of issue-conflict between the parties, new waves of discontent appear which find little that is different. On occasion the more benign may concede here a point, or there an exception, but these mental lapses are quickly remedied when the differences perceived are dismissed as irrelevant, insignificant, or inadequate. Again, as Lincoln Steffens and others found of New York in the late twenties,[23] it is fashionable to insist on the distinction between "radical" or "conservative" and "liberal."[24] Again, in the manner of the then-young turk editors of The New Republic—Wilson, Soule and Cowley—in 1931, conferences are held that question "the adequacy of America's dominant liberal ideology." And again the image of party candidates articulated by a Bryce or a Duverger takes hold: " . . . fundamentally men expert in the winning of votes . . . not ideological groups nor class communities . . . ," motivated by little other than personal gain.[25]

Yet, whether echo or choice,[26] the data presented in Table 1 demonstrate that opposition candidates for the House do quite naturally align themselves on an ideological dimension and that majorities (although not overwhelming) bunch at that position conventionally associated with each of the national parties.

TABLE 1. SUBJECTIVE IDEOLOGICAL POSITION OF NONINCUMBENT CANDIDATES FOR THE HOUSE OF REPRESENTATIVES, 1964[a]

Self-Anchoring Position	Democrats	Republicans
Liberal	59%	8%
Middle of the Road	37	34
Conservative	1	53
Mixed[b]	2	4
Meaningless terms	1	1
	100% (145)	100% (158)

[a] Candidates were classified on the basis of their response to the following question: "Next, we would like some idea about how you feel on certain policy issues being discussed around the country this year. On most issues, would you consider yourself liberal, middle of the road, conservative, or what?"

[b] A category used to classify those who indicated a position between the choices above.

The assumption of a left-right spatial model of party conflict, postulated by Anthony Downs and others, is clearly more applicable to this admittedly atypical group of congressional challengers than it is to the party-in-the-electorate.[27] Candidates do in fact form "ideological communities," particularly between but also within parties, even though the association between party and issue-position, not unexpectedly, is far from one to one.

Obviously identification as "liberal," "middle of the road," or "conservative" does not mean, as stressed earlier, there are rigidly pre-

[23] An excellent intellectual history which emphasizes similarities between the late twenties and mid-sixties is Christopher Lasch's *The New Radicalism in America* (New York: Vintage Books, 1965), especially pp. 251–308.

[24] I don't mean to imply that the distinctions aren't important; quite the contrary. Each has ideological components that are very different and these differences are surely as significant as the similarities suggested in the sometimes radical, sometimes conservative, attacks on the ubiquity of American Liberalism.

[25] Maurice Duverger, *Political Parties* (New York: John Wiley & Sons, rev. ed., 1963), p. 22.

[26] The question of whether American parties offer a significant choice is one so obviously saturated by an individual's value preferences (in addition to empirically demonstrable evidence) that few answers will satisfy anyone. Since I believe that political scientists should make explicit their own value positions, I should add (if it is not already apparent) that mine usually dump me on the liberal to left wing of the Democratic party. Thus, while often finding party differences "inadequate," I rarely agree they are "insignificant" or "echoes."

[27] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), particularly pp. 96–142. This is a phenomenon stressed in the work of McClosky, Eldersveld, and James Q. Wilson and entertained more formally in the reformulation of Downs by Donald Stokes, "Spatial Models of Party Competition," reprinted in Angus Campbell, *et al.*, *Elections and the Political Order* (New York: John Wiley & Sons, 1966), pp. 175–176.

scribed patterns of liberal or conservative behavior. But the distinction does have important consequences, both on specific opinions and behavior. Between that 59 percent among the Democrats who thought of themselves as liberal and the 53 percent among Republicans who identified as conservative, there are large[28] and consistent differences in responses to the six specific issue-areas presented in Table 2. Moreover, the manner in which moderates (middle of the road) of each party differ is striking testimony to the psychological framework the parties provide in mediating the ideological attitude of "moderation."

Conventionally, differences between parties in two-party systems have been conceived as embracing three zones, two on the polar ends and one in the center composed of groups from both parties who are closer to each other than to their respective poles. On the three social welfare issues presented here, the spread suggests a slightly different arrangement, with both the liberal and moderate Democrats joined by the few (8 percent) liberal Republicans constituting one bloc, moderate Republicans a second, and conservative Republicans a third. The tilt on these dimensions is surely "left." The relative absence of disagreement between liberal and moderate Democrats on each of these issues shows how the argument over federal economic intervention—among nonincumbents—has become a party issue and not simply a division between different wings within each party.

Although similar evidence has been provided for the parties in Congress,[29] it should be remembered that Democratic nonincumbents are contesting disproportionately in districts outside the South. On two of the noneconomic issues, one dealing with civil rights and the other with foreign policy, the distribution described above undergoes some rearrangement. The civil rights question, raised *after* the 1964 Civil Rights Bill had been passed, provides the closest approximation of an issue on which candidates are distributed by ideology and not by party affiliation; i.e., liberal Democrats and Republicans on the left, moderates of both parties in the center, conservative Republicans on the right.

Interestingly, however, one out of five of the conservative Republicans felt the federal government should do more in this area although 47 percent also believed government should do less. This question, excluding the labor union item which sought to invert expected responses, also saw a drop-off in the unanimity of support given by liberal Democrats for domestic intervention. While a majority (57 percent) favored increased federal activity, the much-larger-than-normal proportion who were willing to see the federal government continue present levels of activity (40 percent) lends some support to the notion that liberals, like conservatives, are most consistent on economic issues.

The other noneconomic item, a question on relations with the Soviet Union, provoked two significant patterns. First, although one fifth of the liberal Democrats felt the U. S. should do more to encourage cooperation with Russia, huge majorities in both liberal and moderate Democratic camps were agreed on continuing present policy. Among Republicans, the issue evoked not only the most consistent nay-saying response for the conservatives (74 percent

[28] Tests of statistical significance will not be used on these data, for reasons similar to those argued by Heinz Eulau in *The Legislative System*, *op. cit.*, pp. 455–463. Tests of significance, like chi-square, are based on the assumption that data are drawn from a random sample of the population to which the analyst is interested in making references. Since the data reported above are not a sample, but an attempted enumeration, the assumptions of randomization are not met and tests based on that assumption are misleading. On the other hand, Hubert Blalock suggests that even enumerations are implied samples (of a theoretically infinite population) and hence " . . . some sort of significance test will practically always be helpful in evaluating one's findings," but then admits that " . . . . a skeptic can legitimately claim that had he divided his (data) into three classes according to almost any irrelevant criteria, he would probably have obtained differences as large as ours," *Social Statistics* (New York: McGraw-Hill, 1960), p. 270. The choice is thus not an easy one. I have followed those conventions outlined by Eulau and will treat only those differences of 10 percentage points or more as "large," less than that as "small." On these problems, and the long simmering controversy, the best recent discussion can be found in Johan Galtung, *The Theory and Method of Social Research* (New York: Columbia University Press, 1967), pp. 358–388.

[29] Among which, see Julius Turner, *Party and Constituency* (Baltimore: The Johns Hopkins University Press, 1951); Duncan MacRae, Jr., *Dimensions of Congressional Voting* (Berkeley: University of California Press, 1959); David B. Truman, *The Congressional Party* (New York: John Wiley & Sons, 1959); and, Lewis A. Froman, Jr., *Congressmen and Their Constituencies* (Chicago: Rand, McNally & Co., 1963).

TABLE 2. SIX TESTS OF ATTITUDE CONSISTENCY: SUBJECTIVE AND OBJECTIVE COMPONENTS OF LIBERALISM-CONSERVATISM[a]

Issue	Democrats[b]		Party Distance[c]	Republicans			Party Distance
	Libs	Mods		Libs	Mods	Cons	
On programs for minority and Negro rights, should the federal government:							
DO MORE	57%	27%	*30%*	38%	26%	19%	
DO SAME	40	62		54	53	30	
DO LESS	1	8		8	16	47	
NA	2	3		—	5	4	*31%*
On programs to assist public education:							
DO MORE	79%	73%	*6%*	69%	19%	8%	
DO SAME	6	13		8	46	14	
DO LESS	—	2		8	16	56	*45%*
NA	15	8		15	19	20	
On programs to provide medical care for the elderly:							
DO MORE	96%	73%	*26%*	85%	28%	8%	
DO SAME	4	13		15	47	33	
DO LESS	—	2		—	16	53	*37%*
NA	—	10		—	9	16	
On programs to help the poverty-stricken generally:							
DO MORE	87%	60%	*27%*	54%	19%	12%	
DO SAME	13	37		31	63	31	
DO LESS	—	—		12	12	50	*38%*
NA	—	3		—	6	7	
On programs to regulate labor unions:							
DO MORE	9%	12%		31%	56%	60%	*4%*
DO SAME	66	67		54	35	22	
DO LESS	22	17	*5%*	15	9	6	
NA	3	4		—	—	12	
On programs that encourage cooperation with Russia:							
DO MORE	21%	6%	*15%*	31%	2%	3%	
DO SAME	74	81		54	42	19	
DO LESS	3	6		15	49	74	*25%*
NA	2	7		—	7	4	
	(83)	(57)		(12)	(56)	(83)	

[a] The question used: "Much of what the federal government does can be judged in terms of whether it should do *more*, just about the *same*, or *less*. Please check the following to indicate what your judgment is about each of the following policies or program areas."

[b] Only two Democratic candidates claimed to be conservatives; they have not been included in this analysis.

[c] A simple measure based on the percentage difference (for Democrats) between liberals and "middles" favoring *more* activity on each issue, and (for Republicans) between conservatives and middles favoring *less* activity.

advocated "less"), but it also moved about half (49 percent) of the moderates into this category. Moderate and conservative Republicans were moved toward the most unanimity on this question and on the need for increased regulation of labor. Again, the few liberal Republicans were closer to Democrats than they were to either wing of their own party.[30]

[30] LeRoy Rieselbach has shown, however, that few presidents can rely on a generalized ideology of "internationalism" when attempting to build support for specific programs, if the president's

These data are of course derived exclusively from nonincumbents and any ideological differences (or similarities) between Democratic and Republican challengers could be quite deceptive from the viewpoint of alternatives presented the electorate in specific districts. It is possible that nonincumbents are more like their incumbent opponents in most districts while pairs of congressional candidates differ from district to district. Thus my data would show substantial party differences, since only nonincumbents are considered, when in fact district rather than party was the critical variable. I cannot directly test this hypothesis, but other evidence suggests that it is more often the exception than the rule.

First, substantial research has demonstrated that the parties tend to differ on roll call votes in the House in a manner quite consistent with the findings presented here for nonincumbents. The independent effects of constituency, while significant, do not substantially alter this basic partisan division. Second, previous studies which have compared the voting behavior of successive incumbents from different parties, have consistently shown that substantial differences in behavior do occur, that the "particular incumbent does make a difference"; i.e., votes differently from his predecessor on similar issues.[31] Intensive comparative analysis of the few (83) nonincumbents who were successful in 1964 with their predecessors on four Great Society issues corroborated this finding. I am not suggesting that individual challengers don't alter their positions in specific districts, but the findings above suggest that *in the aggregate* congressional challengers do differ significantly by party. This seems persuasive for understanding the structural (as contrasted with micro-level) character of ideological conflict between the parties. Nevertheless, the absence of direct information about incumbents should be remembered and inferences treated accordingly.

Table 2 also presents a rather simple index of ideological distance within the parties on each issue. The bitter division within the presidential wing of the Republican party during 1964, while not to be anticipated as universally occurring in each congressional district, is still partially reflected in the degree of intra-party conflict suggested by differences on four of the six issues.

Considering differences between moderates and liberal/conservatives for each party as an indication of significant intra-party conflict (liberals are clearly out of the Republican party's ideological ballpark, statistically and attitudinally), the GOP is certainly the more divided. On federal aid to education, 56 percent of the conservatives, but only 9 percent of the moderates, felt the federal government should do less, a difference of 45 percent. On three other issues, poverty, medicare and civil rights, the moderates and conservatives were separated by a spread greater than 30 percent. For Democrats the civil rights issue was the only one which approached this magnitude in showing substantial intra-party conflict; 57 percent of the liberals, but only 27 percent of the moderates, believed the government should do more.

Congressional challengers, whether potential winners or doomed losers, are not simply an aggregate of individuals contesting in isolated constituencies on issues relevant only to their districts. As Eldersveld has shown for the organizations in Detroit,[32] or Scoble has argued of the NCEC,[33] these party candidates constitute groups in ideological terms, although the foregoing analysis indicates these "communities of co-believers" are neither monolithic entities nor ideological oligarchies. If, as this study suggests, they provide some degree of ideological coherence for each national party, then an important question centers on those factors which tend to magnify, mediate or reduce this coherence. How tough is the cement?

III. SOURCES OF IDEOLOGICAL PLURALISM

"Congressional recruitment," writes one contemporary analyst, "is essentially a process for organizing power."[34] The primary agency through which opportunity is structured and power organized has generally but not always been the political party.

strategy is based on expectations of party differences that were more obvious in the immediate post-war period. See his *The Roots of Isolationism* (Indianapolis: Bobbs-Merrill, 1966). pp. 141–164.

[31] See the citations in footmote 29. These studies, based on roll call voting behavior, are subject to the same logical problem since they deal only with incumbents. But the probability of inter-party differences disappearing under the impact of constituency (outside the South) seems remote, particularly in light of the extensive constituency controls examined in each of the above. Further, Lewis Froman has shown that there is even significant variation among congressmen from the same party, although his analysis is restricted to votes on reciprocal trade. Froman *op. cit.*, Chapter 8.

[32] Eldersveld, *op. cit.*, pp. 528–535.

[33] Scoble, *Ideology and Electoral Action*, *op. cit.*, pp. 26–61.

[34] Snowiss, "Congressional Recruitment . . ." *op. cit.*, p. 628.

TABLE 3. PARTY STRUCTURE AND RECRUITMENT: RELATIONSHIP OF ELECTORAL COMPETITION TO POLITICAL IDEOLOGY, CONGRESSIONAL CHALLENGERS BY PARTY*

Ideology	Democrats			Republicans		
	Demo. Dominant	Competitive	Repub. Dominant	Demo. Dominant	Competitive	Repub. Dominant
Liberal	53%	57%	57%	9%	4%	5%
Middle-Road	30	39	40	35	30	55
Conservative	7	1	—	51	61	38
NA	10	3	3	4	4	2
	100%	100%	100%	99%	99%	100%
	(13)	(61)	(71)	(98)	(42)	(18)

* The index of party competition is a modified version of the one used in Lewis Froman, *Congressmen and Their Constituencies* (New York: Rand McNally, 1964), p. 29. Categories were collapsed because of the relatively small number of candidates in each cell. The following conventions were used above: Competitive districts were those in which the opposition had won at least 45 percent of the vote in the five elections preceding 1964 at least once or had actually won the district. Party dominant districts are those in which the opposition had not won once in the five preceding elections nor been able to gain more than 44 percent of the vote. About one-fourth of the districts were redrawn in the 1961–63 period, 42 of which are included in the totals above. Electoral totals for these are based solely on the 1962 and 1964 elections. However, those districts in which the 1964 winner was a Democrat, but the preceding elections had been won by a Republican with more than 55 percent of the vote were placed in the Republican dominant category. Data source: *Congressional Quarterly Almanac*, XIX (1963), p. 1173–1184.

Building on the pioneering efforts of Turner and MacRae,[35] numerous studies have sought to ascertain the effects of party competition and constituency characteristics on the roll call voting behavior of legislators, although only a handful have dealt in a comprehensive way with the inter-relationship of party, constituency, recruitment processes, and the professional and ideological styles of those selected in these environments.[36]

Two strands of inquiry have dominated the roll call studies of the past 15 years: one dealing with legislative party cohesion, the other

[35] Turner, *op. cit.*, and MacRae, "The Relationship . . . " *op. cit.*

[36] Among which, see Sorauf, *op. cit.*; John Wahlke, Heinz Eulau, et al., *op. cit.*; and Snowiss *op. cit.*

TABLE 4. URBANISM AND RECRUITMENT: RELATIONSHIP OF CONSTITUENCY TO POLITICAL IDEOLOGY, CONGRESSIONAL CHALLENGERS BY PARTY**

Ideology	Democrats				Republicans			
	Urban	Suburban	Rural	Mixed	Urban	Suburban	Rural	Mixed
Liberal	61%	63%	51%	65%	13%	7%	5%	6%
Middle-Road	35	36	44	35	43	39	28	39
Conservative	4	—	2	—	38	46	66	50
NA	—	1	3	—	6	8	1	5
	100%	100%	100%	100%	100%	100%	100%	100%
	(23)	(39)	(63)	(20)	(47)	(28)	(65)	(18)

** The classification of districts is taken from *Congressional Quarterly Almanac*, XIX, (1963), pp. 1173–1184. "Mixed" districts were those for which CQ could not determine a predominant character. The urban, suburban, rural include those few (38) which CQ classified as "mixed, but predominantly. . . ." A discussion of criteria, cutting points, etc., is contained in the almanac.

with ideology. Considerable evidence has piled up since MacRae's early research showing that departures from party regularity in voting occur most frequently—when they occur at all—among legislators from districts that are most competitive and "least typical" socially and economically of their own parties.

Further, the results on specific measures of liberalism/conservatism, although inconsistent and characterized by exceptions, tend to confirm MacRae: ideological departures occur most frequently—and it should be stressed again, when they do occur—among legislators from districts that are electorally marginal and environmentally atypical of their own parties.[37] The majority of these studies have focused on state legislatures. Lewis Froman, however, has shown the first proposition (party regularity) to be false for Democrats and only partially true for Republicans in the House when inter-party competition is the independent variable, and the second proposition (ideological deviance) generally accurate (with some major exceptions) using both competitiveness and constituency as independent variables.[38]

The reasoning behind both these hypotheses is quite plausible—candidates seek to maximize their electoral appeal by 1) moving toward the rival party in districts where the opposition party is dominant, 2) pursuing a middle of the road course in districts where the parties are evenly matched, and 3) assuming polar positions where their own party is dominant and the problem of reelection not critical. These political forces, perhaps more than any others, would logically be expected to undercut the ideological homogeneity of challengers in congressional campaigns.

Although the indices of competition and constituency presented in Tables 3 and 4 are necessarily crude, the hypothesis linking ideology to party structure receives little confirmation by the data presented here. Among Democrats, for example, approximately equal proportions tend to identify as liberal or middle-of-the-road, despite variation in the degree of inter-party competition. And this holds even for those districts which are overwhelmingly dominated by the opposition (the GOP's share of the total vote never dipped below 60 percent).

Among Republicans, there is in fact a tendency for a greater proportion of candidates contesting marginal districts to identify on the polar position (conservative) than those candidates secure in Republican-dominant districts (who are more often moderates), but the small number of nonincumbents contesting in districts dominated by their own party makes any analysis here risky. The differences between Republicans running in competitive and Democratic-dominant districts are, however, in a direction consistent with MacRae's prediction, but the proportion is just barely large enough to qualify as substantively significant: 51 percent in Democratic-dominant compared to 61 percent in competitive districts were conservative, showing a mild drop-off effect among Republicans confronted with an intractable Democratic district.

The effects of constituency, measured by an urban/suburban/rural typology, are more consistent with expectations, particularly for Republican challengers. Excluding those districts which could not be classified (included in the table as "Mixed"), Table 4 shows a sharp and monotonic increase in the proportion of Republican conservatives and a decrease in moderates as one moves from urban to suburban to rural constituencies. This is almost the same result found by Froman for congressional incumbents and helps to explain why there are *more* conservative Republicans in competitive districts: the most competitive Republican districts tend also to have the most rural constituencies.[39]

Similarly, while there are no significant differences between the proportions of suburban and urban Democratic candidates who are liberal, the percentage does drop among rural Democrats. Though it is probably true, as Robert Friedman has argued,[40] that if present economic and social trends continue, the urban-rural dichotomy will cease to have a distinctive, measurable difference independent of party, it does not follow that some cleavages, particularly intra-party conflict, will not continue to be rooted in the character of these different constituencies. Still, these data, as well as Froman's and others, raise substantial questions of interpretation.

A large majority of rural Republicans and a small majority (51 percent) of rural Democrats do think of themselves as conservative and

[37] Most of these studies are extensively summarized and critically examined in Heinz Eulau and Katherine Hinckley, "Legislative Institutions and Processes," James Robinson (ed.), *Political Science Annual*, I (1966), pp. 85–150.

[38] Froman, *op. cit.*, pp. 85–122. I think he overemphasizes what are actually quite small statistical differences.

[39] See the discussion on this point by Froman, *ibid.*, pp. 118–119. Among the competitive districts included above, 49 percent were rural.

[40] Robert Freidman, "The Urban-Rural Conflict Revisited," *Western Political Quarterly*, (1961), 959.

TABLE 5. COMPARATIVE IDEOLOGICAL POSITION OF CONGRESSIONAL CHALLENGERS, BY AGE AND PARTY

Ideology	All Candidates		Democrats		Republicans	
	40 and Over	Under 40	40 and Over	Under 40	40 and Over	Under 40
Liberal	34%	26%	62%	48%	10%	4%
Middle-Road	36	41	33	52	38	30
Conservative	26	32	2	—	46	64
NA	4	1	3	—	6	2
	100% (195)	100% (105)	100% (89)	100% (53)	100% (105)	100% (52)

liberal respectively. The differences between Republicans and Democrats are obviously much greater than differences within each party, regardless of whether candidates are contesting in urban, suburban, or rural districts. Party is still the most powerful variable for explaining inter-party differences, while the party system (based on competition) is unrelated and constituency factors are only mildly operative on that rare deviance that does exist.[41] But again, since I do not have data matching incumbent and nonincumbent in the same district, these findings must be treated quite tentatively.

Perhaps Leo Snowiss is correct when he suggests that the more important differences are really between "machine" and "amateur" liberals, or between "traditional" Republican conservatives and the "newer" pragmatic variety, and these differences can be explained by reference to party environments which are quite distinctive in terms of organizational resources, structure and professional ethos,[42] but this is a task clearly beyond the present analysis.

Age, the third independent variable, is almost universally considered an enduring factor in ideological conflict within American party organizations. The belief in a sharp and widespread generational conflict is part of the persistent proverbial wisdom of American politics. This belief, however, tends to ignore the considerable social and political research which has established the profound continuity, the extensive sources of stability, which characterize the transmission of values from one age to another.[43] Yet current student unrest, the McCarthy campaign, or the generous publicity heaped on the behavior of young political leaders like Senators Kennedy, Percy or Brooke, are often treated as typical (rather than atypical) of substantial cleavages in the belief systems of two generations.

Usually, the old guard (itself a term reflecting this wisdom) is pictured as representing the forces of moderation, if not reaction, against newer, more liberalizing younger generations. Among others, Harvey Lehman[44] and David

[41] Running counter to the drift of much postwar attitude research on the mass electorate, the importance of party as a strong discriminating factor in explaining differences toward public issues, even on civil rights, has recently been reemphasized in Raymond E. Wolfinger and Fred Greenstein, "The Repeal of Fair Housing in California: An Analysis of Referendum Voting," this REVIEW, LXII (September, 1968), particularly pp. 759–760. The authors found that Democrats with a high school diploma were "scarcely less likely to vote No (favoring fair housing) than Republicans with a college education." Wolfinger and Greenstein suggest this finding "... may serve as a corrective to interpretations of discrimination and backlash politics that place exclusive emphasis on social class as an independent variable."

[42] This finding applies only to the Chicago delegation, a group conspicuous for the extensive number of "machine" liberals. Large-scale comparative research, as Snowiss suggests, will be necessary before the generality of such an argument can be tested.

[43] The work on political socialization is large and growing. More recently, see Richard Dawson and Kenneth Prewitt, *Political Socialization* (Boston: Little, Brown, 1969). Even among those who are now the center of popular "generation gap" literature, college radicals, some evidence exists which suggests that it is less a "gap" and more an extension of the traditionally left-wing, highly involved political activity of their parents. For some not very reliable but highly suggestive data see S. M. Lipset and Philip Altbach, "Student Politics and Higher Education in the United States," in Lipset (ed.), *Student Politics* (New York: Basic Books, 1967), pp. 199–253.

[44] Harvey Lehman, *Age and Achievement* (Princeton: Princeton University Press, 1959).

TABLE 6. AGE, CONSTITUENCY AND IDEOLOGY, CONGRESSIONAL CHALLENGERS BY PARTY

	Democratic Challengers						Republican Challengers					
	Urban		Suburban		Rural		Urban		Suburban		Rural	
	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40
Liberal	70%	60%	67%	50%	58%	40%	11%	8%	13%	—%	6%	—%
Middle-Road	27	40	30	50	36	60	49	37	39	44	36	14
Conservative	—	—	—	—	—	—	35	50	39	55	56	85
NA	3	—	3	—	6	—	6	5	9	—	2	1
	100% (22)	100% (10)	100% (26)	100% (16)	100% (37)	100% (25)	101% (34)	100% (24)	100% (23)	99% (9)	100% (44)	100% (21)

Walker[45] have argued that age in recruitment is a factor which affects the ideological outlook of candidates and that youthful politicos are more often the selection of forces of protest and unrest. Further, one would expect that non-incumbents, whose median age is 10 years younger than incumbents, might clearly reflect such conflict.

Consider, then, the data in Table 5. The so-called generational split does exist, but in precisely the opposite direction from that expected. For both parties, those *under* forty were more often in the conservative ranks than those forty and over. Among Democrats, 62 percent of the candidates over forty considered themselves liberal while 48 percent of their counterparts who were under forty did so. Of Republicans, 64 percent under forty identified as conservative compared with only 46 percent of their older colleagues.

Two interpretations of this rather unexpected finding are possible. First, an argument could be made that the reversal is not really unexpected if one assumes that it is the youth organizations close to the Republican party, such as the YAF, which most closely approximate "pure" conservatism and who are most insistent on sustaining doctrinal purity against the pragmatic, electorally-oriented drive of their elders. However, a similar argument must then apply equally to younger members of the Democratic party, but this clearly is not the case. Nor is it true that Democratic candidates under forty no longer felt the term "liberal" adequately described contemporary choices since they consciously identified as "middle of the road."

Nevertheless, on the assumption that younger challengers, regardless of subjective orientation, do in fact differ on post-New Deal, non-economic issues from older candidates, and that these differences are not adequately handled by the conventional meaning of liberalism/conservatism, analysis was undertaken by age on the civil rights issue. Although the differences are reduced, the distribution is still in the same direction: 43 percent of the older but 40 percent of the younger Democratic candidates believed the federal government should do more in this area; 27 percent of the GOP candidates forty and above felt the federal government should do less on civil rights while 38 percent of the younger Republican challengers considered less activity appropri-

[45] David Walker, "The Age Factor in the 1958 Congressional Elections," *Midwest Journal of Political Science*, IV (February, 1960).

TABLE 7. AGE, PARTY STRUCTURE, AND IDEOLOGY, CONGRESSIONAL CHALLENGERS BY PARTY

	Democratic Challengers						Republican Challengers					
	Tend Demo		Competitive		Tend Rep		Tend Demo		Competitive		Tend Rep	
	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40	40 & Over	Under 40
Liberal	60%	34%	60%	54%	70%	41%	2%	3%	3%	—%	6%	—%
Middle-Road	20	66	36	46	30	59	48	25	30	21	53	75
Conservative	20	—	—	—	—	—	44	70	65	73	41	25
NA	—	—	4	—	—	—	6	2	2	4	—	—
	100% (10)	100% (3)	100% (32)	100% (24)	100% (41)	100% (24)	100% (56)	100% (35)	100% (27)	98% (14)	100% (13)	100% (4)

ate. To the extent federal intervention in civil rights is an issue which goes beyond the older economic conflict, younger men are no more likely than older to favor such extension.

A second interpretation, one consistent with some of Joseph Schlesinger's work on ambition theory,[46] would hold that younger candidates, more ambitious and career-oriented than their elders, are more flexible ideologically because ideology is tempered by realistic appraisals of the opportunity structure. The argument would seem even more persuasive when applied to nonincumbent congressional candidates, many of whom are just beginning serious consideration of the political vocation. But this fails to explain the findings for Republicans.

Nor does the independent effect of constituency or party structure, shown in Tables 6 and 7, substantially alter this paradox. Because the number of respondents in each cell is generally quite small, and any percentage differences based on these cells are not very meaningful, the control variables are included for illustrative purposes only. But they are extremely suggestive.

Constituency characteristics obviously affect the distribution of liberal, moderate and conservative candidates in each party, but this impact tends to move both younger and older candidates in a direction consistent with the original finding; i.e., younger challengers are still more often in the conservative camps of each party. For example, 85 percent of rural Republican challengers under forty thought of themselves as conservative, while only 56 percent of their similarly rural but older counterparts were so inclined. Among Democrats, 60 percent of those under 40 contesting in rural districts, but 36 percent of those 40 and over, were self-identified middle-of-the-roaders. The influence of party competition also reduces generational differences, particularly in competitive districts, yet younger and older Republican candidates continue to differ along the lines suggested above. And this holds precisely in those districts (Demo-dominant) where in fact one might anticipate ambitious younger Republicans to move toward the center.

The relationship between age and ideology is complex and it is doubtful the operational measures used here are powerful enough to embrace some rather important components in a generational conflict, such as style, which surface in situations where age differences are a factor. An excellent example, partially involving just such a conflict, would be the Halleck-Ford leadership struggle.[47] Also, the cutting point employed, 40 years, is obviously not appropriate for youth-adult political conflict although it does fit the requirements for generational battles among political leaders, who are rarely under 30. Still, the fact is that the common proposition about generational conflict receives little confirmation from these data on nonincumbents, and possibly might be quite contrary to expectations. This should serve as considerable warning to those who see younger candidates as an unqualified source for liberalizing tendencies in the two national parties.

The final independent variable, former office-holding, was selected because it taps two dimensions of the process many have argued would lead challengers in congressional campaigns to converge rather than differ on issues of public policy, presumably expanding rather than restricting their electoral appeal.

Edmund Costantini has shown[48] that California Democratic officie-holders as a group while more ideological than the rank and file party members in the electorate, are still more

[46] Schlesinger, *op. cit.*, p. 176.

[47] Peabody, *op. cit.*

[48] Costantini, *op. cit.*

TABLE 8. PUBLIC OFFICE EXPERIENCE AND POLITICAL IDEOLOGY, CONGRESSIONAL CHALLENGERS BY PARTY

Ideology	Democrats		Republicans	
	Some Public Office Experience	No Public Office Experience	Some Public Office Experience	No Public Office Experience
Liberal	55%	56%	11%	7%
Middle-Road	40	38	48	20
Conservative	4	3	36	63
NA	1	1	5	—
	100%	98%	100%	100%
	(77)	(71)	(64)	(91)

TABLE 9. PARTY OFFICE EXPERIENCE AND POLITICAL IDEOLOGY, CONGRESSIONAL CHALLENGERS BY PARTY

Ideology	Democrats			Republicans		
	Nat'l/State	Local	No Office	Nat'l/State	Local	No Office
Liberal	60%	59%	56%	7%	11%	4%
Middle-Road	34	41	41	44	30	39
Conservative	2	—	1	46	56	53
NA	4	—	2	3	3	4
	100% (41)	100% (48)	100% (56)	100% (45)	100% (63)	100% (47)

"centrist" than active, committed party workers, in a test of a notion long prevalent among observers of American politics. Moreover, the simple effect of actually holding office, where one is forced to deal with the conflicting desires, aspirations, and needs of a polyglot constituency, is usually "educational" for even the most ideological office holder in a democratic polity—the less wordy but more popular description for which is "copping out."

Since Costantini used a scale reflecting positions on specific issues, these data are not quite comparable, but one would still expect differences between those who have held public and party—but particularly public—office and those without such experience within each of the parties. This expectation is totally disconfirmed for Democrats, but remarkably borne out for Republicans, as a glance at the data in Table 8 will indicate.

Virtually no difference exists between Democratic candidates with or without prior office experience, but for Republicans the difference is quite large: 63 percent of the GOP candidates who had no public office experience considered themselves conservative, but only 36 percent of those who had held public offices did so, a difference of 27 percent.[49]

[49] A similar lack of experience (although of a different type) was noted by Aaron Wildavsky for Goldwater supporters at the 1964 convention in his "The Goldwater Phenomenon: Purists, Politicians and the Two-Party System," *Review of Politics*, 27 (July, 1965). Nelson Polsby points out that Goldwater appealed, in addition, to a considerable number of professionals, regardless of misgivings about the electoral consequences, simply because they agreed with his specific programs. But he adds that 74 percent of the delegates—an unusually high figure—had not been to either of the two previous conventions. See his "Strategic Considerations," in Milton Cummings (ed.), *The National Elections of 1964* (Washington, D. C.: The Brookings Institution, 1966), pp. 100–1.

On the other hand, *party* office experience apparently has little effect (see Table 9) on tendencies to anchor oneself in either a centrist or polar position, even when different levels (national, state or local) are considered. This is consistent with the belief that, at least for Republicans, there is a distinct difference in the way party and public office-holding socializes candidates, the former maximizing partisan ("purist") attitudes, the latter mitigating them.

TABLE 10. ELECTORAL OUTCOME AND IDEOLOGY IN THE 1964 CONGRESSIONAL ELECTIONS, BY PARTY

Ideology	Democrats		Republicans	
	Winners	Losers	Winners	Losers
Liberal	54%	59%	4%	9%
Middle-Road	39	39	27	35
Conservative	2	—	70	51
Mixed	2	2	—	4
NA	2	—	—	1
	99% (63)	100% (82)	101% (16)	100% (142)

The finding for Democratic candidates suggests that either these men perceive less dissonance between their ideology and office holding, or they hold different kinds of offices than do Republicans, or both. Among Republicans it is clear that intra-party conflict and ideological purity is emphasized by the entry of challengers with little public office-holding experience. Alternatively, countervailing forces may begin to operate when Republicans have managed to endure the tribulations of public office. But the

TABLE 11. IDEOLOGY AND ELECTORAL MARGIN IN THE 1964 CONGRESSIONAL ELECTIONS, BY PARTY

Outcome	Dems	Repubs	Democrats*		Republicans		
			Libs	Mods	Libs	Mods	Cons
Winners	33%	9%	40%	44%	8%	6%	13%
Losers	67	91	60	56	92	94	87
	100%	100%	100%	100%	100%	100%	100%
Won (61% plus)	6%	1%	6%	8%	8%	2%	—%
Won (56–60%)	6	3	7	11	—	—	6
Won (55% & less)	21	5	27	25	—	4	7
Lost (45% plus)	29%	18%	26%	29%	15%	16%	17%
Lost (40–44%)	27	22	21	21	23	26	10
Lost (39% & less)	11	50	13	6	54	53	60
	100% (145)	99% (158)	100% (83)	100% (57)	100% (12)	101% (56)	100% (83)

* The two self-identified conservative Democrats scored a 100% victory, and therein lies a tale of the Democrats in, as opposed to outside, Congress.

experience apparently has differential effects depending on party and ideological position.

These findings also suggest another line of speculative inquiry. A huge majority of these candidates face demonstrably hostile electoral conditions and it stretches the imagination to believe that any amount of institutional tinkering with the national party organization (regardless of what else might be accomplished) would in fact generate even more candidates who identified clearly as liberal or conservative without radically altering the number of parties in the system, a common argument of those attracted to the "responsible" party position. Despite the electoral environment, and the powerful countervailing social and organizational features of American political life, it is remarkable that a majority of congressional challengers in each party do in fact continue to offer what this observer takes to be a clear choice, at least in districts outside the South. The question remains: what follows electorally?

IV. THE STRUCTURE OF DEFEAT

Two observations can be made about the character of congressional campaigns in the United States. First, except for a handful of districts, and under unusual circumstances, the behavior of the challenger, including his ideological stance, is probably the least influential factor bearing on electoral outcome.[50] Second, given necessary conditions, even a small increase in the number of incumbents defeated by challengers can have significant consequences on the output of the House of Representatives.

The data presented in Tables 10 and 11 can be interpreted as providing some evidence consistent with the first argument: liberals and moderates are distributed among winners and losers in about the same proportion as they are among Democratic nonincumbents as a whole. Nor does Table 11 show any evidence that indicates moderate Democrats tended to gain victory—when they did so—by a larger margin than liberals, an assumption implicit in some campaign theories. There are no appreciable differences in either the margin of victory, or defeat, between Democratic moderate and liberal candidates.

I am aware that no evidence regarding different campaign resources, strategies, activities etc., of liberals or moderates is provided here. Rather, I assume that these factors, more or less under the control of individual candidates, are evenly distributed among both groups. The question involves the common argument that a liberal (and by extension, a conservative) ideology can only be sustained in districts that are leaning to the other party at the expense of electoral victory. These data suggest that a candidate's general position (within the usual ideological bounds) is unimportant.

Among Republicans, the magnitude of de-

[50] Lewis A. Froman, "A Realistic Approach to Campaign Strategies and Tactics," in M. Kent Jennings and Harmon Zeigler (eds.), *The Electoral Process* (Englewood Cliffs: Prentice-Hall, 1966) p. 4.

TABLE 12. PARTY STRUCTURE, CONSTITUENCY, AND ELECTORAL OUTCOME, NONINCUMBENT CANDIDATES FOR THE HOUSE OF REPRESENTATIVES, 1964

Party and Outcome	Two-Party Relationship			Constituency			
	Demo Dominant	Competitive	Repub Dominant	Urban	Suburban	Rural	Mixed
Democrats							
Won (61% plus)	91%	—%	—%	26%	3%	9%	—%
Won (56–60%)	9	10	6	13	5	2	5
Won (55% & less)	—	42	22	27	19	35	35
Percent won:	100	52	28	66	27	46	40
Lost (45% plus)	—	36	21	13	31	29	20
Lost (40–44%)	—	10	30	9	26	21	25
Lost (39% & less)	—	2	21	13	18	3	10
N	(13)	(61)	(71)	(23)	(39)	(63)	(20)
Republicans							
Won (61% plus)	—%	5%	6%	2%	4%	6%	—%
Won (56–60%)	4	11	11	—	7	8	—
Won (55% & less)	—	9	11	—	4	3	—
Percent won:	4	25	28	2	15	17	—
Lost (45% plus)	7	33	27	11	14	22	17
Lost (40–44%)	13	26	12	13	21	14	28
Lost (39% & less)	76	14	33	74	50	45	55
N	(98)	(42)	(18)	(47)	(28)	(65)	(18)

feat affected all wings almost equally, except that the few (16) winners considered here were disproportionately conservative, but 87 percent of the GOP conservatives running in 1964 were defeated, just as were 94 percent of the Republican moderates and 92 percent of the liberals. As V. O. Key once observed in another context, when the rain falls, it falls equally on the willing and unwilling. Although the data are far from satisfactory, they do lend some support to the notion that the more important determinants of electoral outcome are structural, involving the constituency and the pattern of party competition in each district. Some indication of the relationship of different party structures and different constituencies to the electoral fate of the 1964 challengers is shown in Table 12.

The Democratic margin is underscored by the following: nonincumbents were able to sweep all of the few Democratic-dominant districts in which they ran (five of the thirteen considered here were opened to nonincumbents by defeating incumbents in the primaries), while Republicans held only seven out of ten of their own Republican-dominant districts, perhaps equally significant given the massiveness of Goldwater's defeat.

Democratic nominees were also able to win 52 percent of the competitive district races they entered, but four out of five were by margins less than 55 percent. Contrarily, Republican nonincumbents won only 28 percent of the competitive districts they sought. Excluding Alabama, Milton Cummings' study of the 1964 electoral races also shows how successful Democratic incumbents were in maintaining their seats, even in districts (mainly southern) where Johnson's total dipped below 40 percent.[51]

Moreover, as these same data suggest, Democratic challengers maintained a predictable dominance in urban constituencies and were able to capture almost half of the rural dis-

[51] Cummings, *op. cit.*, pp. 232–233.

TABLE 13. ELECTORAL FATE IN THE 1966 MIDTERM ELECTIONS OF FRESHMEN IN THE 89TH CONGRESS, BY PARTY AND IDEOLOGY[a]
(In Numbers)

Outcome	Freshmen		Democrats		
	Demos	Repubs	Libs	Mods	Cons
Won (61% plus)	18	14	6	5	4
Won (56–60%)	4	—	3	1	—
Won (55% & less)	17	—	9	7	—
Lost (45% plus)	19	—	12	7	—
Lost (40–44%)	4	1	2	2	—
Lost (39% & less)	—	—	—	—	—
Retired/Other office	4	3	2	2	—
Total Losers	23	1	14	9	—
Total Winners	39	14	18	13	4
Other[b]	4	3	2	2	—
Totals	(66)	(18)	(35)	(23)	(4)

[a] The three freshmen who entered in the second session and three former Congressmen who ran in 1964 were omitted from these figures. The ideology classification includes three Democrats and two Republicans who either didn't respond to the original questionnaire or were not subsequently interviewed. They were classified on the basis of federal role scores. Since only one Republican freshman was defeated, Glenn Andrews (Alabama), breakdowns for the GOP would be pointless.

[b] Among those who voluntarily left the House, Democrats Charles Farnsley (Ky.) retired; Paul Krebs lost his seat through reapportionment in New Jersey; Robert Sweeney ran for the Attorney General's office in Ohio; Teno Roncalio contested for United States Senator from Wyoming. Both Sweeney and Roncalio were defeated. Among Republicans, all three were southerners and all ran for governor: Howard "Bo" Calloway in Georgia; James Martin in Alabama; Prentiss Walker in Mississippi. All were defeated.

tricts in which they were running. Because of the small number of rural Democratic winners, no extensive further cross-tabulations were undertaken, but it is suggestive that of the 30 newly-elected rural-based Democrats, 16 considered themselves liberal, 13 middle of the road, and one conservative, a finding once more calling into question the presumed electoral advantages of moving to the center. But again this judgment must be treated gingerly since this analysis does not explore variation in individual stands on particular policies.

One other fact about electoral success—rare as it may be for nonincumbents—is suggested by these figures. Those Democrats who did win in environments that were either competitive or in constituencies generally not oriented to the Democratic party, were also invariably the most unstable in the next elections. Although subsequent analyses will deal intensively with the behavior and fate of the Republican and Democratic freshmen of 1964, some indication of what happened in 1966 is presented in Table 13.

This election, of course, reversed the Democratic victories of 1964, giving the Republicans a net gain of 48 seats, 10 more than the Democrats had managed two years before, and an unusually high off-year gain in congressional elections (the average off-year switch since 1950 has been 25). The lone Republican loser, Glenn Andrews of Alabama, was widely believed to be "too moderate" on civil rights.

Parenthetically, if Andrews was in fact defeated on the civil rights issue, and there is every reason to believe he was, this case should serve as a reminder of the inapplicability of the generalizations explored above to individual campaigns. Miller and Stokes find that single issues, in special areas under unique circumstances, can become powerful determinants of electoral outcome.[52] But the present data again underscore the above argument since there are no significant differences in the electoral margin of defeat, or victory, among those Democratic moderate and liberal freshmen up for reelection in 1966.

During the course of long and structured interviews with most of the winners of 1964, I was impressed by the consistency of two essentially different approaches to the problem of reelection among freshman Democrats. The first is typified by the following, a midwesterner:

> Let me tell you a story. Of course I'm more liberal than my district, but I've been a life-long liberal Democrat and I didn't come down here to simply play games with a bunch of people back home whom I wouldn't fart with—so to speak—normally. I've done a better job, I'm sure, than my predecessor, servicing my constituency, but I'm appalled by the war and the poverty programs are absolutely necessary. When I go back, I tell them exactly that and many obviously don't like it. So what? I'm here to help with the President's program and I intend to do it . . . except the war . . . things have got to change, and change soon, in Vietnam . . .

The other was captured also by the response of a midwestern Democrat:

> People are always saying, and this bothers me, that Congressmen don't do anything unless it's in the interest of reelection . . . but hell, that's the name of the game. I can't compete with Wil-

[52] The Hays-Alford contest in the fifth district of Arkansas, the example cited, was extraordinary: 100 percent of the voters knew who Hays and Alford were and apparently also thought Brooks Hays, a 20-year veteran, too soft on civil rights. He was defeated by a write-in campaign for Alford.

bur Mills or George Mahon. How much sense does it make to spend hours wading through some of these reports to be able to change a comma, or going over the legislation in detail which comes down here every day. The problem is my district and I've got to win again there before any of this makes any sense. Sure I vote with the President on most programs, but I've been in my district twice a week, every week, for the past year and I intend to continue it until next November. These people have never known this kind of attention and it's going to pay off . . .

Both of these men, so dissimilar in approach, shared one thing in common: they were defeated in November.

V. CONCLUSIONS AND SPECULATIONS

Every two years a sizable number of candidates for public office go through the sometimes tedious, often strenuous process of gaining nomination, creating organizations, raising money, charting strategies, making speeches, working charity, service and labor groups, knowing, as Key once said, that "the voice of the people consists mainly of the words 'yes' and 'no' and at times one cannot be certain which word is being uttered." For at least four-fifths of the group examined in this paper, however, the response was quite audible. Boss Plunkitt's famous epitaph, "He seen his opportunities and he took 'em," could properly be rephrased for the majority of congressional challengers to read, "He seen no opportunities but he took 'em anyway."

Popular and scholarly opinion has not been exceptionally kind to this cadre of highly active citizens—the two most frequently heard stereotypes are attorneys on the make and party dullards—but this study suggests another role.

It seems pointless continually to reiterate that American political parties are aggregations of extraordinarily diverse, often conflicting social and political units and then professionally gun down those who long for the classic, presumably well organized and clear ideological choices associated with continental political systems. Just as there are tendencies in the party system fostering homogeneity, so too there are countervailing forces creating significant choice.

The function most often imputed to political opposition is to provide alternatives, and the data examined here indicate that congressional challengers in the aggregate are psychologically by and large performing that function. Whether liberalism and conservatism are meaningful alternatives I shall leave to the judgment of my colleagues, but they apparently seem so to these candidates, who empirically resist being poured into those old stand-bys—the twin bottles—of intellectual contempt.

Differences between the two parties are not a function of differences in the social backgrounds of their respective candidates (with one minor exception),[53] but rather a result of the way similar backgrounds are apparently differentially internalized. The small social cleavages that do exist between the candidates are reinforced by fact that the majority of social "deviants" nominated by Republicans were almost without exception defeated.

The organizational decentralization of American political parties has led many commentators to believe significant nationalizing tendencies (other than the drive to gain and hold power) are rarely operative. While three of the four independent variables introduced do in fact generate sources of ideological variation, this should be considered in light of a substantial similarity of commitment. The data suggest there is a large core of Democratic liberalism and Republican conservatism which withstands the impact of almost any environmental factor, and this core may provide a strong corrective to further structural balkanization. But it appears to be more typical of Democratic then Republican challengers.

Some evidence is presented which very tentatively supports the argument that for most districts the electoral outcome is little dependent on whether a nonincumbent candidate is conservative, middle-of-the-road or liberal, a finding which undercuts the belief that it is electorally profitable for parties to enter campaigns on the assumption that moving to the center will produce at least short-run gains (whatever else might be the reason for adopting such strategy).[54] And it is precisely because large numbers of ideologically consistent challengers do not act "rationally"[55] in this sense

[53] Professionals among the Democrats were more often educators, journalists and ministers than were Republicans, but the differences are not substantial.

[54] The low visibility of most congressional campaigns restricts any possible extension of this argument to other (more salient) electoral arenas. For two recent studies of the presidential struggle which go beyond the more typically unsystematic or "how to" research on political campaigns, see Karl Lamb and Paul Smith, *Campaign Decision-Making* (Belmont: Wadsworth Publishing Company, 1968); and, John H. Kessel, *The Goldwater Coalition* (Indianapolis: Bobbs-Merrill, 1968).

[55] On this point, I part company with Froman, if I understand the character of his recommenda-

that when the rain falls, and a few are elected, large enough majorities (or minorities) are created to move (or prevent from moving) major public innovations in social policy. As one Democratic freshman sarcastically remarked, "We are the cannon fodder of the 'Great Society'."

Of course the stability of congressional elections also discourages more rapid and consistent action in the House (or consolidates past gains, depending on one's perspective). This is hardly an original observation and I know it will not sit well with those Republicans whose goal is winning, since few elections following the thirties have provided similar occasions for ideologically conservative nonincumbents to participate in charting a clear alternative course for public policy. For some—and not only conservatives—this represents the tragedy of American politics; for others, the genius.

Independent of the other functions performed by these candidates, it seems clear that congressional challengers make a substantial contribution to whatever ideological difference now exists between the two national parties.

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tions, since I still believe campaigns can be educational in the old-fashioned sense and that restructuring the present relationship between candidates and the public is perhaps one of the most important long-run effects losers could have on electoral politics. Froman does recognize that "latent functions" (including issue development) are important components of the campaign process, but the gist of his paper is concerned with exploiting the current environment. See his "A Realistic Approach. . . " *op. cit.*, pp. 8–19.

DEDUCTION, EXPLANATION, AND SOCIAL SCIENTIFIC INQUIRY

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I. INTRODUCTION

The purpose here is to explore certain aspects of the philosophy of science which have serious implications both for the practice of social and political science and for understanding that practice. The current relationship between social science and the philosophy of science (or the philosophy of the social sciences) is a curious one. Despite the emergence of a considerable body of literature in philosophy which is pertinent to the methodological problems of social science, there has been a lack of ostensive ties between the two areas. A justified concern with the independence of social scientific research has contributed to a tendency toward isolation which is unfortunate in view of the proliferation of philosophical problems which necessarily attends the rapid expansion of any empirical discipline. Although in the literature of contemporary social science there are frequent references to certain works in the philosophy of science and to philosophical issues relating to methodology, these are most often in the context of bald pronouncements and shibboleths relating to the nature of science, its goals, and the character of its reasoning. But what is most disturbing about the fact that social scientists have little direct and thorough acquaintance with the philosophy of science is not merely that there has been a failure to carefully examine the many logical and epistemological assumptions which are implicit in social scientific inquiry, since this task might normally and properly be considered to be within the province of the philospher of science. It is more significant that this unfamiliarity has, paradoxically, obscured the extent to which certain doctrines originating in the philosophy of science have been uncritically insinuated into the enterprise of social science as informing categoricals.

Much of the theoretical literature in political and social science, as well as attempts to explicate the epistemic features of social scientific investigation, rest on a belief that the activity of the natural scientist is an appropriate model for understanding and prescribing the role of the social scientist. Although it may be granted that social and natural science must be distinguished in terms of such characteristics as technique and subject matter, this is often understood as explaining certain inherent or temporary limitations of social science. Generally it has been assumed that with regard to the logic of explanation, social science must be fundamentally symmetrical with natural science if it is to count as science. Although there are a number of common features that would support the idea of symmetry, such as a mutual empirical orientation and concern for systematic explanation and description, and although philosophy, science, and everday life may be analytically as well as existentially differentiated as modes of thought and activity, this view is misconceived to the extent that it tends to postulate a hierarchy of epistemological structures with impermeable boundaries and fails to take account of some of the more generic aspects of explanation. As a consequence of this misconception, social scientists have been reluctant to move beyond the realm of natural science when seeking a model of inquiry even when the requirements of explanation in the two spheres seem hopelessly disparate. The result has been principally either an imperious attempt at assimilation or an assignment of social science to some inferior position within the general class of activities understood as science. Both positions are seriously defective, and a more adequate formulation is possible only by breaking down some of the alleged constraints which are assumed to be endemic to scientific inquiry.

A conception of social scientific explanation can be elaborated which will not only support the case for logical asymmetry between the natural and social sciences and at the same time preserve the characteristics essential to any mode of empirical inquiry but will better illuminate what most social scientists in fact do in producing explanations as well as what sort of explanations are required by the character of the phenomena with which they are concerned.[1] But the problem of supporting the contention that natural science is inadequate as a paradigm governing the conception of methodology in the social sciences is complicated by the fact that what is often construed by social scientists as natural science is actually

[1] For some preliminary arguments supporting this position, see John G. Gunnell, "Social Science and Political Reality: The Problem of Explanation," *Social Research*, 35 (Spring, 1968).

an ideal typification of the logic of science which is the invention of certain philosophers of science. This philosophical reconstruction has been accepted in one form or another by many social scientists, especially those concerned with the problems of theory construction, as an adequate representation of the character of all scientific explanation and as a prescriptive norm for inquiry. What the social scientist accepts as his model is, then, itself a model and one that, although popularly received for many years, has recently been severely criticized within the philosophy of science itself not only as a reconstruction of the logic of scientific explanation in general but even natural science in particular.

A thorough critical analysis of this model, the so-called deductive model, is a necessary prologue to any further evaluation of the thesis of logical symmetry between explanation in the natural and social sciences as well as an explication of the substantive character of social scientific explanation, and such an analysis is the principal, although limited, concern of this essay. Consideration of the deductive model also provides a vehicle for examining certain more general problems about explanation and the relationship between social science and the philosophy of science. Since social scientists, and especially political scientists, have been influenced, either directly or indirectly, by the deductive model and other aspects of the philosphy of logical empiricism from which it emanates, there is not only the obvious question whether social scientists have correctly understood this construct and this school of thought but the question of the validity of logical empiricism, even if correctly understood, as an approach to the philosophy of science (and social science). Finally, and possibly most important, there is the problem of the extent to which the philosophy of science may be understood as an enterprise charged with establishing a theory of science which can serve as an authoritative guide for substantive empirical inquiry and a norm for the evaluation of particular explanations.

II. THE DEDUCTIVE MODEL

The persistent, pervasive, and yet largely unexamined assumptions that the logic of any enterprise claiming the title of science must be symmetrical with that of natural science and that there is universality in the logic of empirical inquiry, as well as the conception of the logic of explanation in natural science accepted by most social scientists, have for the most part been derived either explicitly or implicitly from certain formulations in the philosophy of science. In seeking to emulate natural science, "the model taken for imitation has been the artifact of the philosopher of science rather than the actual practice of scientists."[2] This formulation, the deductive model, has become the basis of the modern "textbook" view of science. Although in many instances it is difficult to judge whether social scientists have drawn their model of inquiry directly from the philosophy of science or whether they have merely unreflectively accepted some popular version of that model, the latter hypothesis seems more tenable in view of the numerous and basically unexplicated statements in the literature of social science to the effect that "the task of science is the reasoned interpretation of experience through the discovery of valid generalizations and the application of such generalizations to particular events."[3] The precise meaning of such assertions is seldom unpacked; they are inserted as ritualistic formulae. But whether the conception of natural science which is entertained is once or twice removed from that activity, it is clear that social scientists have rarely attempted either to defend in any rigorous manner the idea that there must be symmetry in the logic of inquiry or to systematically support the notion of the exemplary character of natural science. Yet, more specifically, there has been a failure both to justify the deductive model as a proper reconstruction of the logic of natural science and to demonstrate its applicability to empirical science as a whole, including social science.

The argument to be advanced here is that the deductive model is deficient in most relevant respects. It presents an incorrect, or at least otiose, reconstruction of the logic of natural science, and there has been no adequate demonstration that it is applicable either as a norm for social scientific inquiry or as an explication of social scientific explanation. But unfortunately this argument can be set against social scientists only indirectly, since they themselves have not developed a defense of the deductive model. The battle must be joined on the ground of the philosophy of science, and this would be entirely the proper ground for such a discussion if the question were merely one of developing a general structural description of social scientific explanation, i.e., if it were not for the extraordinary fact that much

[2] Norton E. Long, Foreword to Eugene J. Meehan, *Explanation in Social Science* (Homewood, Ill.: Dorsey Press, 1968), p. v.

[3] Harry Eckstein and David Apter, *Comparative Politics* (Glencoe: Free Press, 1963), p. v.

of theory construction in the social sciences takes the philosophers' deductive model as a standard, however poorly approximated and understood. This assertion may invite the charge of setting up a straw man since the defense of the deductive model in principle as well as its applicability to social scientific explanation has been conducted essentially by philosophers of science while there is little in the way of explicit advocation by social scientists.[4] But although a separate treatment would be required to thoroughly support the intellectual history of the influence of the deductive model and logical empiricism on social science, it is assumed that this influence is not a matter of serious dispute and that it can be readily recognized in a variety of works dealing with methodological problems of social and political science as well as popular treatises on theory construction and specific empirical studies.

Some of the most prestigious theoretical literature in political science contains numerous phrases such as "scientific method," "scientific enterprise," "scientific credo," "the rigorous logic and rules of science," "scientific reasoning," and "scientific rules of procedure," but little concrete elaboration is presented. Despite professions of faith in the unity of science and an enumeration of attributes often associated with natural science such as "systematization," "quantification," and the search for "regularities," there is a singular absence of argument which would support the assumption of the logical equivalence of social and natural science, and there are few explicit references to either the philosophy of science or the actual practice of natural scientists which would give tangible meaning to these vague assertions about the defining characteristics of scientific activity.[5] When David Easton, in *The Political System*, challenged political science to become truly scientific, it was far from clear what he meant in substantive terms. Notwithstanding the plea for the development of an empirically grounded "causal theory," which in his view was the only indication of "the attainment of reliable knowledge," and despite his later pronouncements about the "gargantuan strides" which have been taken during the intervening period toward the goal of a social science that would compare favorably with the natural sciences, the meaning of "science" in the sense of a logic of inquiry has remained nebulous.[6] Neither in 1953 nor in the 1960's has Easton either defended the accuracy of his conception of natural science or detailed the philosophical view on which this conception is based. Yet it was assumed that the character of this anticipated theory would be "deductive" and that although a theoretic framework which "might reach the stage of maturity associated with theory in physics" where "from a few basic premises, empirically derived, it has proved possible to formulate deductively a whole body of intermediate theory and from this in turn, to predict the occurrence of empirical events," might be a long way off, such a "theoretical system" was to be the goal and standard of inquiry in political science.[7]

For political scientists, the meaning of "science" is seldom interpreted beyond such formal, and essentially empty, equations of explanation and "generalization" and statements to the effect that explanation can be understood as "simply the subsuming of a particular event or class of events . . . under a more general law or hypothesis," or "deriving it from some more general proposition that describes a regular sequence which we believe to hold true in the world of politics."[8] It is from the philosophy of science, and from a rather well-defined group of scholars within that discipline, that political and social scientists have ultimately derived their views about explanation and the requirements of scientific logic. The works of such philosophers as Carl Hempel and Ernest Nagel[9] have become philosophical primers for social scientists and are continually cited as presumably unimpeachable authority to support statements about the purpose and function of science, the symmetry of natural and social science, and the logic of scientific explanation. The indefeasibility of this philosophical perspective and the incorrigibility of its representation of science is taken for granted, and it is assumed that from this body of literature can be extracted the

[4] See, for example, Richard S. Rudner, *Philosophy of Social Science* (Englewood Cliffs, N. J.: Prentice-Hall, 1966).

[5] David Easton, *A Framework for Political Analysis* (Englewood Cliffs: Prentice-Hall, 1965), pp. 316–317; *The Political System* (New York: Knopf 1953), p. 25.

[6] Easton, *System*, pp. 52–59; *Framework*, p. 3.

[7] Easton, *System*, p. 58; *Framework*, p. 7.

[8] Robert Dahl, "Cause and Effect in the Study of Politics," in Daniel Lerner (ed.), *Cause and Effect* (New York: Free Press, 1963), pp. 75, 77–78.

[9] For the most comprehensive statement of Hempel's position see his *Aspects of Scientific Explanation* (New York: Free Press, 1965). Ernest Nagel's most popular work is *The Structure of Science* (New York: Harcourt, Brace and World 1961).

fundamental principles which must govern and give direction to empirical inquiry in political science.[10]

As Eugene Meehan has recently noted, the deductive model has become "an albatross around the neck of the social scientist"[11] in the sense that it provides a set of demands which are not only impossible to realize but often irrelevant to the requirements of explanation in social science. But the answer to the problem is not to substitute a "systems paradigm" for the deductive model as Meehan has suggested; these constructs are not logically comparable. Before embarking on any extended criticism of the deductive model, it is necessary to precisely determine its character and to understand its present status in the philosophy of science. Meehan, perpetuating a common misconception in social science, tends to view the deductive model as if it were essentially a procedural recommendation for scientific inquiry; but the model, as presented by most of its proponents, was never conceived either as a methodological norm in the sense of how to go about the task of explanation or as a reconstruction of the process of explanation. Meehan also maintains that this model has been "rarely criticized" in the literature of the philosophy of science, but, on the contrary, every indication is that at present the weight of argument is against the deductive model, and that the revolt against the model as well as other aspects of logical empiricism "has now lasted long enough to produce something of a counterrevolution."[12] Even the defenders of the deductive model recognize that the "global nature of the attack gives the current controversy its special interest."[13] Yet the fact that the impact of the debate has scarcely been felt in the social sciences is only a symptom of the cultural lag in the relationship between these two disciplines.

As the term "deductive model" is used here, it refers to a quite specific formulation which in its essential characteristics is embraced and advocated by a number of philosophers but most prominently associated with the work o Hempel. The Hempelian thesis of deductive nomological explanation is most fundamentally an argument for the "methodological unity of all empirical science"[14] since it attempts t reduce the criteria of scientific explanation t one universal logical pattern. It asserts tha "all scientific explanation," as opposed to th manifold everyday senses of "explain" an other specialized meanings of the term, "in volves, explicitly or by implication, a subsump tion of its subject matter under general regu larities; that it seeks to provide a systemati understanding of empirical phenomena by show ing that they fit into a nomic nexus."[15] Mor specifically, it is argued that an event or phe nomenon that is to be explained, or strictly speaking, the statement describing the phe nomenon or event (the *explanandum*), mus be deducible from premises (the *explanans* which are assumed to be true and contain, i addition to certain statements of fact or initia or antecedent conditions, at least one genera law expressing empirical regularities. Empiri cal laws are in turn explained in the same man ner by deductive subsumption under mor inclusive laws or comprehensive theories whic consist of a deductively joined system of ax ioms and theorems. The proponents of th model are unequivocal in their assertion tha "science looks for laws, because without then neither explanation nor prediction is possible and that the sense in which the term "deduc tion" is to be taken is the strict one of followin the rule of *modus ponens* of formal logic, i.e. that laws function as the "premises of a de ductive argument" and "the conclusion ma be deduced from the premises because th premises logically imply the conclusion. An the premises logically imply the conclusio because the corresponding conditional, if th premises are true then the conclusion is true is a tautology."[16] Explanation is viewed prin cipally as answering "why" questions, an explanation and prediction, although differin pragmatically, are construed as possessing th same logical form in the sense that any ex plained event could, in principle, have bee predicted in advance. There are certain as pects of the deductive model about whic there is less than complete agreement amon

[10] See, for example, Eugene Meehan, *The Theory and Method of Political Analysis* (Homewood, Ill.: Dorsey Press, 1965), pp. v, vi, 3, 8, 9.

[11] Meehan, *Explanation*, p. 3.

[12] *Ibid*, p. 2; Edward MacKinnon, "Epistemological Problems in the Philosophy of Science, I," *Review of Metaphysics*, XXII (Sept., 1968), p. 113.

[13] May Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," in Brodbeck (ed.), *Readings in The Philosophy of the Social Sciences* (New York: Macmillan, 1968), p. 364; cf. M. B. Hesse, "A New Look at Scientific Explanation," *Review of Metaphysics*, XVII (Spring, 1963).

[14] Hempel, "Explanation in Science and His tory," in Robert G. Colodny (ed.), *Frontiers c Science and Philosophy* (Pittsburgh: University c Pittsburgh Press, 1962), p. 32.

[15] Hempel, *Aspects*, p. 488.

[16] Brodbeck, "Introduction," in Brodbeck (ed.) pp. 7, 9.

its exponents, such as whether the relationship between explanation and prediction is actually one of logical symmetry and whether an explanation in terms of statistical and probabilistic laws is strictly deductive. There are also a number of other features of the model, and logical empiricism in general, which are significantly related to issues of social scientific inquiry, such as the problems of hypothesis confirmation and especially the questions of theoretical reductionism and the relation between theoretical and observation terms and their cognitive status. But these problems are less relevant to the particular focus of this discussion.[17]

[17] For a discussion of some of these problems and their relation to assumptions about theory construction in social science, see John G. Gunnell, "The Idea of The Conceptual Framework: A Philosophical Critique," *Journal of Comparative Administration*, I (August, 1969). In order to scotch any misunderstanding from the outset, it must be emphasized that in this context the "deductive model" is not to be understood as equivalent to the rather loose and variable characterization of scientific reasoning often referred to as the hypothetico-deductive method or, roughly, the idea that explanation is a process of formulating hypotheses which are tentatively confirmed or disconfirmed by observational tests. Although there are often numerous similarities between the deductive model and specific formulations of the H-D method [*e.g.*, R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953)] such as the tendency to view the logic of science as essentially deductive in character and although many of the criticisms relating to the accuracy and sufficiency of the H-D account such as those most prominently associated with the work of N. R. Hanson and his concern with the "logic of discovery" are pertinent to a consideration of the deductive model, it is probably generally accepted that science utilizes a form of inference which in its broad outlines resembles this representation. Neither should the deductive model be equated with Karl Popper's ideas on the deductive testing of theories and the attending thesis of falsifiability. Although Popper maintains that the logic of science is entirely deductive in character in the sense that the principle of *modus tollens* is the only rule of inference available to science, his concern is essentially with the criteria for the rejection and corroboration of empirical hypotheses and choosing between hypotheses [*The Logic of Discovery* (New York: Science Editions, 1961), pp. 41, 75–76]. Whatever labels may be attached to Popper's position, his focus is on "providing some sort of justification for a mode of nondemonstrative inference," and it might well be argued that in this sense his approach "is not properly characterized as *deductivism*." [Wesley C. Salmon, "The Foundations of Scientific Inference," in Robert G. Colodny (ed.), *Mind and Cosmos* (Pittsburgh: University of Pittsburgh Press, 1966), pp. 160–161]. While the deductive model stresses the subsumption of phenomena under general laws, Popper stresses the development of theories and conjectures which can survive the severest possible tests or attempts at falsification, and the difference in distribution of emphasis is significant. Finally, Popper's views, especially those concerning the relation between theory and observation language, have provided the foundations for some of the most damaging criticism of traditional logical empiricism.

What must be clearly understood is that the deductive model is expressly "not intended to reflect the manner in which working scientists actually formulate their explanatory accounts."[18] Although it is offered as an explication of the "logical structure and rationale of various ways in which empirical science answers explanation-seeking why-questions," it is a "philosophical thesis" and not an empirical claim.[19] It is sometimes argued that a defect of the model is that it accepts physical mechanics as the paradigm of scientific explanation and thus judges all other forms of explanation accordingly, but the reverse is the case. From the point of view of May Brodbeck and others who accept and defend this concept of explanation, "physics is an advanced and 'model' science precisely because it does provide deductive explanation and prediction of its phenomena."[20] But to argue that the model is prescriptive rather than descriptive may be to attempt too general a characterization, since on the one hand its exponents do not primarily intend it as a norm for scientists to follow and on the other hand it does provide a conceptual framework in terms of which one can view and categorize particular explanations.

The model principally provides a set of formal logical criteria which are timeless and placeless and state what any explanation must include to truly count as a scientific explanation, i.e., "a *philosophic* demand is being put upon explanation—a demand that the cohesion

[18] Hempel, "Explanation in Science and History," p. 15.

[19] Hempel, "Explanatory Incompleteness," in Brodbeck (ed.), p. 399.

[20] Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," p. 369.

of its components should not depend upon the background of the person who asks for the explanation, nor upon the context in which it is given, but upon the relations which are, so to speak, intrinsic to explanation."[21] And the essence of the demand is simply that what is to be explained must be strictly deducible from a law to count as a fully adequate explanation. Whether the content of explanations in any branch of science in fact does, or conceivably can, coincide with this meta-scientific reconstruction is considered irrelevant as far as judging the validity of the model, and there is "no claim whatever concerning the extent to which scientific explanations can actually be achieved for the phenomena studied in different branches of scientific inquiry."[22] In other words, there is no necessary coincidence between science and scientific explanation.

III. LOGICAL EMPIRICISM AND CONTEXTUALISM

In addition to clarifying the content of the deductive model, it is necessary to understand the philosophical position which supports it. Logical empiricism as an approach to the philosophy of science has been concerned with developing formal representations or reconstructions of the logical structure of scientific explanation and with a meta-logical analysis of the language applied to science. In this view there is a very strict correlation between the philosophy of science and formal logic. Although the object of analysis is science, and despite the claim of providing reconstructions of scientific logic, neither the history nor the current practice of any branch of science is considered pivotal, and references to specific scientific theories are seldom invoked to support conclusions about the character of scientific explanation. Although the aim is not to provide a guideline for empirical research, the import of this reconstructed logic is clearly nominative or legislative, in the sense that it is considered to stand above and outside any particular scientific context and to serve as an authoritative guide for judging or evaluating the explanatory adequacy of any particular scientific assertion. Recent work in the philosophy of science has severely challenged many of the specific formulations of logical empiricism such as the deductive model, but most of these criticisms are intelligible only in terms of a fundamental change in the conception of the philosophy of science itself. This recent literature is marked by its empirical orientation, i.e., its attention to actual scientific practice and especially the history of science. The attitude is non-legislative, and the role of the philosophy of science is understood as principally one of explication; the stress is on the diversity rather than the unity of science and on the autonomy of science rather than its dependence on formal logic and philosophy.

Philosophers such as Stephen Toulmin and Michael Scriven, who may be described as embracing a descriptive or contextualist approach to the philosophy of science, attack the deductive model on many grounds, but the basis of the attack is centered around the charge that the model does not actually reflect the character of scientific explanation. From their standpoint, the philosophy of science is principally an endeavor which describes and renders scientific practice intelligible rather than attempting to construct formal and definitive representations of scientific explanation and provide trans-contextual logical standards by which to judge any aspect of scientific explanation. What is involved in the conflict between these approaches is "a disagreement about the nature of philosophic method,"[23] and this difference in perspective can be clearly discerned in the attitude of the adherents of these respective positions toward the notion of logic. For the deductivist, "logic" is defined in the narrow and specific sense of the formal logician, and a sharp distinction is drawn between the "context of discovery" or theory formulation and the "context of justification," with the logic of science restricted to the latter.[24] The opponents of the deductive model use the term "logic" in its more generic sense to encompass the various modes of inquiry and inference actually followed by scientists and stress the distinction between "working logic" and "idealized logic" or "logic-in-use" and "reconstructed logic."[25] It is the reluctance of

[21] Rudolph H. Weingartner, "The Quarrel About Historical Explanation," in Brodbeck (ed.) *op. cit.*, p. 355.

[22] Hempel, "Explanation and Prediction by Covering Laws," in Bernard Baumrin (ed.), *Philosophy of Science: The Delaware Seminar*, (New York: Interscience Publishers, 1963), Vol. I., p. 116.

[23] Weingartner, p. 350; Also see Dudley Shapere, "Meaning and Scientific Change," in Robert G. Colodny (ed.), *Mind and Cosmos* (Pittsburgh: University of Pittsburgh Press, 1966), pp. 41–50.

[24] Rudner, *op. cit.*, pp. 4–5.

[25] See Stephen Toulmin, *The Uses of Argumen* (Cambridge: Cambridge University Press, 1958) Ch. IV and Abraham Kaplan, *The Conduct o Inquiry* (San Francisco: Chandler, 1964), Ch. 1

the reconstructionists to grant any autonomy to "working logic" and the non-justificatory aspects of explanation, or in fact to recognize it as logic at all, which leads them to ignore many aspects of scientific inquiry and to characterize all explanations which do not approach the deductive ideal as "imperfect," "incomplete," "elliptical," "partial," or, worst of all, merely "explanation sketches" which is the phrase understood as best describing most explanations in history and social science. In this view, "perfect knowledge is the ideal, actualized only in certain branches of physical science. Elsewhere, as in biology, economics, sociology, psychology, and the social sciences generally, knowledge is conspicuously 'imperfect'. "[26]

The opponents of the deductive model reject this rationalistic approach and the attempt to postulate an extra-scientific standard of completeness of explanations and perfect knowledge. They argue that the notion of complete explanation in any absolute sense is misconceived and that completeness and perfection are contextually relative rather than depending on some universal standard such as the idea that explanations are "imperfect" if they do not make it possible "to compute (predict or postdict) the state of a system, either an individual or a group, at *any* moment from its state at *one* moment."[27] The contextualist holds that "where the standards for judging the soundness, validity, cogency or strength of arguments are in practice field-dependent, logical theorists restrict these notions and attempt to define them in field-invariant terms."[28] For the contextualist, "explanations are practical, context bound affairs" and the "completeness or correctness of an explanation is a notion without meaning except in a given context."[29] To neglect substantive "context criteria" and require that a complete scientific explanation involves laws, that it be logically symmetrical with a predictive statement, and that it be limited to answering "why" questions is to impose standards which are at once too restrictive in the sense that they exclude much of what scientific practice finds perfectly acceptable and too inclusive in the sense that they admit assertions which in many instances would not count as relevant explanations.[30] Mere deduction may be wrong, trivial or no explanation at all, and thus "it is an illusion to suppose that scientific explanation is anything as simple as deductive subsumption under a true generalization."[31] While the deductivist holds that explanations which do not conform to the model are inadequate *a priori*, the contextualist maintains that there is a "family of explanations" and "a diversity of types" and that "the simple fact must be faced that certain evidence is adequate to guarantee certain explanations without the benefit of deduction from laws."[32]

The criticism of the deductive model, however, reaches deeper than merely asserting a certain relativity and autonomy with regard to the logic of explanation and renouncing a legislative role for the philosophy of science, for it is further maintained that there can be no deductive demonstration in empirical inquiry in the sense in which Hempel and others employ the term "deduction," since there is a necessary "divergence between the categories of practical argument-criticism and those of formal logic."[33] Toulmin argues that many of the current problems in logic and the philosophy of science have arisen from a historical tendency to make the analytic syllogism and the categories of formal logic a paradigm for all arguments. Contrary to what certain defenders of the deductive model maintain,[34] the opponents do not wish to claim that there is no such thing as "deduction" in science but rather that the term should apply to detective work as well as physics and that there is no reason why the notion of deductive inference and the meaning of concepts such as "valid" and "certain" should be interpreted in terms of formal logic or why formal logic should be a standard for judging the infinite variety of substantive scientific arguments. The seemingly irredeemable difference between the certainty of formal

[26] These distinctions are set forth by Hempel in many of his writings. See *Aspects*. Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," p. 375. Also Gustav Bergmann, "Imperfect Knowledge," in Brodbeck (ed.), *op. cit.*

[27] *Ibid.*, pp. 375, 371–372.

[28] Toulmin, *op. cit.*, p. 147.

[29] Michael Scriven, "Truisms as the Grounds for Historical Explanation," in Patrick Gardiner (ed.), *Theories of History* (Glencoe: Free Press, 1959), p. 450.

[30] *Ibid.*, pp. 468–469; Scriven, "Explanations, Predictions, and Laws," in Herbert Feigl and Grover Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* (Minneapolis: University of Minnesota Press, 1962), Vol. III, pp. 173–190, 196, 202.

[31] Scriven, "Explanations, Predictions, and Laws," p. 225.

[32] Scriven, "Truisms as the Grounds for Historical Explanation," pp. 452, 456.

[33] Toulmin, *op. cit.*, p. 146.

[34] Brodbeck, "Explanation, Prediction, and Imperfect' Knowledge," p. 371.

logic and the contingent character of scientific assertions has sometimes led to a view such as that of Reichenbach that all scientific explanation is inductive and probabilistic, but such a formulation accepts the logician's preference for certainty and fails to recognize that terms like "probable" and "certain" are modals whose substantive meaning is contextually determined.

For Toulmin, and contextualists in general, the structure of analytical arguments, and the very notion of deduction implied there, are inapplicable for judging substantial arguments whether in science, ethics, or jurisprudence, but it is precisely this standard which the strict deductivist has attempted to apply to scientific explanation. Many difficulties "spring from the misapplication of these categories to arguments of other sorts," and this approach tends to obscure the fact that the most important distinction is not between deductive and inductive reasoning but between analytical and substantial arguments or between demonstrative inference and ampliative or non-demonstrative inference.[35] The philosopher of science who embraces the contextualist perspective does not reject formal logic but only its application as a criterion for evaluating and explicating actual forms of justification in various fields of empirical inquiry. What the contextualist will accept as the logic of explanation in any field, the strict deductivist dismisses as the "psychological and pragmatic aspects of explanation" and demands a "non-pragmatic conception of scientific explanation."[36] But for the contextualist, explanation itself is a "pragmatic notion." The contextualist approach is, again, "largely an empirical one" and assumes that the "meaning of terms and concepts or logical problems can only be thoroughly understood if we include a meticulous examination of the circumstances in which they occur, rather than relying on a relatively rapidly extracted formulation of their apparent internal logical features."[37]

[35] Toulmin, *op. cit.*, p. 146. Also Scriven, "Definitions, Explanation, and Theories," in Feigl, Scriven, and Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* (Minneapolis: University of Minnesota Press, 1958), Vol. II, pp. 192–193; Max Black, "The Justification of Induction," in Sidney Morgenbesser (ed.), *Philosophy of Science Today* (New York: Basic Books, 1967).

[36] Hempel, "Explanatory Incompleteness," p. 400 and "Explanations and Prediction by Cover-Laws," p. 130.

[37] Scriven, "Truisms as the Grounds for Historical Explanation," p. 452 and "Definitions, Explanations, and Theories," p. 100.

IV. THE INADEQUACY OF DEDUCTIVISM

Once this difference in perspective is understood the basis for much of the controversy over the logic of explanation becomes clear, and it should be equally clear that the conflict between these positions cannot be reconciled. An understanding of these two approaches to the philosophy of science makes it evident why the arguments on both sides tend to pass one another by, yet there are grounds for choosing between them, and such a choice is necessary if there is to be any clarity in philosophical discussions of social scientific explanation and any meaningful critical analysis of the claims of social scientists regarding the adequacy of substantive explanations. The choice in the first instance must be essentially between the reconstructionist and contextualist approaches, yet even if the reconstructionist position were to be accepted, the particular philosophical criteria advocated by the deductivist seems questionable. The basic assumptions which inform reconstructionism as well as its particular offspring, in this case the deductive model and the implications of this model as a standard for judging social scientific practice, are unacceptable.

Philosophers such as Hempel maintain that deductive-nomological explanation, or what has been termed the "covering laws," obviously do not encompass all the contexts in which one speaks of explanation. They are quick to point out that the concern is with explicating the concept of *scientific* explanation rather than explanation in its more generic sense, in somewhat the same way that there are meta-mathematical formulations of mathematical proof which do not apply to all the fields in which one might speak of proof.[38] But the problem facing the deductive model is its justification as *the* standard or ideal for the critical appraisal of scientific explanation and as a form for exhibiting its underlying "logical structure and rationale." To approach scientific explanation from the standpoint of an ideal type may be defensible, but it is more problematical when this idealization is admittedly not primarily extracted from an examination of the activity which it seeks to judge and illuminate. It would seem that we could not ask more of an argument in any particular field than that it "achieve whatever sort of cogency or well-foundedness can relevantly be asked for in that field."[39] But even if an idealization is constructed in view of some non-empirically

[38] Hempel, "Explanatory Incompleteness," p. 399.

[39] Toulmin, *op. cit.*, p. 45.

grounded external philosophical criterion, such a procedure is not necessarily defective if the criterion can be adequately justified, i.e., if it can be demonstrated that deductiveness should, or must, be the standard of the logic of scientific explanation. Hempel admits that such a conclusion is not "susceptible to strict 'proof'" and that "its soundness has to be judged by the light it can shed on the rationale and force of explanatory accounts offered in different branches of empirical science."[40] But this would seem to land him in something of a contradiction. He emphasizes that the validity of the model is independent of explanations actually formulated by scientists in various branches of empirical inquiry and yet maintains "that the logic of *all* scientific explanations *is* basically of the covering-law variety"[41] and further that the viability of the covering-law model must essentially be justified pragmatically. The peculiar and circular chain of reasoning would seem to be that only explanations subsumable under the covering-law model can be classified as scientific, and consequently, by definition, the logic of scientific explanation is of this type and thus the model necessarily illuminates "scientific" explanations.

It is apparent that few, if any, actual explanations in natural science, let alone social science, do conform to this schema, and thus the saving clause which enables the deductive model to perform a descriptive or typological function is the notion of explanatory incompleteness. This allows almost any branch of empirical inquiry and most of history and social science a certain claim to scientificness as long as it is willing to pay the price of humility and accept the stigma of incompleteness and imperfection. But

> the important difference between the social sciences and the physical sciences is not that one has and one does not have complete explanations, but that one has more quantitative laws than the other... But they confer no benefit on the explanation that cannot be obtained in other ways, and in particular they do not convey the blessing of deduced truth since they are usually only approximations.[42]

Hempel and other deductivists have been singularly unsuccessful in providing examples from physics or any other science which conform to the deductive model. Even the laws of physics produce no automatic deductions or explanations; they require judgment in their application, and judgment necessitates substantive criteria.[43] The idea of "exact deduction" in science is a myth because the laws of science are not exact.[44] To claim that scientific inference is purely "a logical question" in the sense of formal logic is to fail to recognize that explanation is primarily an epistemological concept.[45]

The justification for the deductive model is in fact not "pragmatic" at all. It relies instead on the submerged informing assumption that the norm of acceptable argument is analytic or demonstrative argument, but it is unreasonable, and misleading, "to treat this type of argument as a paradigm and to demand that arguments in other fields should conform to its standard regardless, or to build up from a study of the simplest forms of argument alone a set of categories intended for application to arguments of all sorts."[46] It is not merely that the identification of the logic of science with analytic or demonstrative argument presents a very truncated view of both logic and science, but that the judgment of all justification by the ideal of analyticity neither provides a relevant standard for judging empirical or substantial arguments nor illuminates in any significant manner the underlying logical structure of most aspects of scientific explanation. The deductivist denies the charge that he equates deduction with either analytic or syllogistic reasoning.[47] But whatever label is applied, the argument is that "geometry is the prototype of all scientific theories" and that "in philosophy and elsewhere an appeal either to a definition or to a logical truth is conclusive" and "conclusive in a way that appeal to contingent truth is not" and that "any other kind of explanation of individual facts cannot pos-

[40] Hempel, "Explanatory Incompleteness," p. 411.

[41] *Loc. cit.*, emphasis added.

[42] Scriven, "Explanations, Predictions, and Laws," p. 211.

[43] Scriven, "Truisms as the Grounds for Historical Explanation," pp. 459, 460, 462.

[44] Scriven, "Definitions, Explanations, and Theories," p. 193. See also Scriven, "The Limits of Physical Explanation," in Bernard Baumrin (ed.), *Philosophy of Science: The Delaware Seminar* (New York: Interscience, 1963), Vol. 2.

[45] Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," pp. 369, 374; John Yolton, "Explanation," *British Journal for the Philosophy of Science*, 10 (1959), p. 195.

[46] Toulmin, *op. cit.*, p. 144.

[47] Scriven, "Truisms as the Grounds for Historical Explanation," p. 462; Toulmin, *The Philosophy of Science* (London: Hutchinson, 1953) p. 25, 33, 49, 102; Hempel, "Explanation and Prediction by Covering Laws," p. 120; Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," pp. 370, 371, 373.

sess this conclusiveness. Either the explanation is deductive or else it does not justify what it is said to explain."[48]

It is often maintained that the deductive model is epistemologically neutral, and although this is not entirely true since it does preclude certain substantive explanatory accounts, it is precisely this formal or empty character which vitiates its adequacy as an explanatory norm. What the model demands is that the cogency of an explanation depend on a particular logical form, but this severing of logic and epistemology and the attempt to find an Archimedean point from which to view and judge all scientific explanation is untenable. "Not deducibility, but intelligibility constitutes the basic feature of the logic of explanation,"[49] and to understand and criticize the explanations produced in any science is at once to become acquainted with that science both in its present form and its historical development and with what characterizes its claims to know. The point is not that existential explanations in social science and history are by their very nature partial, incomplete, sketchy, or in some other way fundamentally unsound (although in any particular instance and judged by the contextual field-dependent criteria of a specific science they may be) but rather that they are qualitatively different and autonomous kinds of explanation. To reject the deductive model and argue for the contextual evaluation of explanations does not constitute, as Brodbeck and others insist, an abandonment of critical judgment and philosophical questions about explanation or the acceptance of a precept that would read "every man his own explainer." Nor does it entail the approval of "ordinary usage" as a paradigm of explanation or make all criticism finally dependent on subjective psychological tests.[50]

The request for an explanation presupposes that *something* is understood, and a complete answer is one that relates the object of inquiry to the realm of understanding in some comprehensible and appropriate way. What this way is varies from subject matter to subject matter just as what makes something better than something else varies from the field of automobiles to the solution of chess problems; but the *logical function* of explanation, as of evaluation, is the same in each field. And what counts as complete will vary from context to context within a field; but the logical category of complete explanation can still be characterized in the perfectly general way just given, i.e., the logical function of 'complete' as applied to 'explanation' can be described.[51]

What will count as an adequate explanation or what will constitute valid inferences in any scientific or other context in which there are recognized rules and forms of judgment and criticism has a "systematic" aspect. It is against a background of accepted laws, theories and regulative principles, i.e., a substantive scientific paradigm, that the intelligibility of any explanation is judged.[52] Generalizations, laws, and theories function as rules, "inference tickets," or "warrants," or justifying grounds in terms of which explanatory inferences are drawn, not as premises in a deductive argument.[53] To see this as the logical function of laws and theories is not to confuse "experience and the logical methods by which we think about the experience"[54] as long as the distinction between the logical function a justifying statement plays and the substantive meaning of that statement are distinguished. But the more important point is that much of scientific explanation does not involve laws. The statements justifying inferences or serving the logical function of principles of inference cannot be restricted to laws without either radically curtailing what might be called science or arbitrarily assigning a large part of the results of inquiry in both the natural and social sciences to the status of imprefectness or incompleteness. To maintain that "insofar as an individual fact justifies the inference to one or more facts, laws are always implicitly invoked" since "otherwise, the justification remains a mystery,"[55] is not only to take a very narrow view of scientific reasoning but to distort scientific

[48] Bergmann, *Philosophy of Science* (Madison: University of Wisconsin Press, 1958), p. 31; Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," pp. 370, 385.

[49] Yolton, *op. cit.*, p. 207.

[50] Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," pp. 365–368.

[51] Scriven, "Explanations, Predictions, and Laws," p. 202.

[52] Theodore Mischel, "Pragmatic Aspects of Explanation," *Philosophy of Science*, 33 (1966).

[53] Toulmin, *Philosophy of Science*, pp. 93–94, 103–104; *Uses of Arguments*, 101, 114, 121–122, 220; Gilbert Ryle, *The Concept of Mind* (New York: Barnes and Noble, 1949), pp. 120–125; Scriven, "Truisms as the Grounds for Historical Explanation," pp. 445–451; Wilfrid Sellars, "The Language of Theories," in Herbert Feigl and Grover Maxwell (eds.), *Current Issues in The Philosophy of Science* (New York: Holt Rinehart and Winston, 1961), esp. p. 71.

[54] Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," p. 384.

[55] *Ibid.*, p. 383.

practice. Yet such a narrow view is precisely what the deductive model requires by its insistence that laws which can constitute the premises of a formal deductive argument must be present, either overtly or in some suppressed form, before any assertion may be accepted as a fully adequate explanation.

This demand becomes particularly pernicious in its effects on social science which, attempting to be "scientific," begins with this model, or at least hearsay about it, and seeks to formulate procedures and goals of inquiry accordingly while uncritically accepting the philosophers' evaluation of its present claims to knowledge as imperfect. What has happened in social science, unlike natural science in which the philosophy of science and reconstructed logic have had little or no impact or as in history where, despite a long-standing debate about the Hempelian model in the philosophy of history,[56] there has been only a minor influence on historiography, is that there has been a peculiar synthesis of the language of the philosophy of science and the language of substantive inquiry which has resulted in a rather bizarre corpus of theoretical literature. In a sense the idea of a science of society modeled after natural science has been an ideology from the very beginning of social science, and the acceptance of the deductive model is part of the continuing tradition of that ideology. The result has been an overriding concern with developing generalizations and a proliferation of pseudo-laws which are little more than abstracted summaries of empirical findings which carry no explanatory value in any commonly accepted sense of the term "explain".[57] The theorist in the social sciences tends merely to take Hempel and others at face value without careful regard for what constitutes, or must constitute, explanation in his particular field, and finds it sufficient to state that "the higher the level of generality in ordering. . . facts and clarifying their relations, the broader will be the range of explanation and understanding."[58] Social scientists find little in the deductive model except the precept ordering them to generalize, but explanation in terms of a " 'mere empirical generalization' is very rarely explanatory, and it is only because laws usually involve more than this. . . that they carry explanatory force."[59] But raising the question of "more than this" is what social scientists have avoided by their allegiance to the formalistic standard of the deductive model.

The idea derived from the deductive model which most seriously infects the social scientific enterprise is the attenuated picture of the model which enters the literature of social science as the dictum that all suitable explanation involves the subsumption of phenomena under general laws, or, given the imperfect nature or at best temporary theoretical impurity of social science, at least generalizations. But the acceptance of this dictum, the equation of explanation and generalization, has been *a priori*, i.e., it has not evolved from a consideration of what constitutes social scientific phenomena and consequently what in any substantive sense would constitute an adequate explanation, and thus it has been able to provide neither a tenable description nor a viable prescriptive ideal. As Abraham Kaplan points out, any such offering of reconstructed logic must be treated as a "hypothesis."[60] But to accept without question the general demand for laws and generalizations as the prerequisites of explanation often leads in practive only to truisms which add nothing to the explanation of individual phenomena.[61] The problem is not so much to "stop trying to imitate physics" but rather "only what a particular reconstruction claims physics to be,"[62] and the first lesson that social science might learn from natural science, if it penetrated beyond the ideological screen of the deductive model, is the value of devotion to the autonomy of inquiry and a passionate concern with the substance of explanation rather than a fixation on the form of explanation. Social science might well remember that some of the most important advances in science have been achieved not by solving problems but by abondoning them, and this might well be the case in regard to the social scientist's attempt at explanation through conformance with the deductive model.

The deductive model is only one more attempt to find *the* scientific method. The model's more strenuous advocates go so far as to identify the acceptance of the deductive model with the acceptance of science itself and, like Brodbeck, classify those who dissent as humanists,

[56] For representative discussions see Gardiner (ed.), *op. cit.*

[57] See, for example, Bernard Berelson and Gary A. Steiner, *Human Behavior: An Inventory of Scientific Findings* (New York: Harcourt, Brace and World, 1964).

[58] Easton, *System*, p. 4.

[59] Scriven, "Explanations, Predictions, and Laws," p. 212.

[60] Kaplan, *op. cit.*, p. 10.

[61] A. R. Louch, *Explanation and Human Action* (Berkeley: University of California Press, 1966) pp. 38–40.

[62] Kaplan, *op. cit.*, p. 11.

members of the literary tradition, and intuitionists.[63] But this dichotomizing technique is too facile to be convincing, and the contemporary logical empiricist must be reminded, as the logical positivist before him, that "often what presents itself to us as obscurantism is only a departure from the particular reconstruction that we have come to identify with reason itself."[64] The deductivist's definition of science is especially misleading, because it subverts what Kaplan has termed the "autonomy of inquiry"[65] not only by creating a hierarchy of sciences but by attempting to subject empirical inquiry, which is always contingent, to the rule of noncontingent *a priori* extra-scientific principles and ideas of certainty and conclusiveness which belong to formal logic. There would seem to be little justification for social science, or any other science, seeking its standards and concepts of explanation from philosophers and logicians. This is not to say that science does not involve "philosophic" and even metaphysical presuppositions or that philosophy is irrelevant for science. It also does not mean that there are no similarities between certain modes of scientific and philosophic explanation. One of the disabilities of the deductive model is that it implies a strict separation between science and other forms of explanation which may not adequately represent scientific practice and which, if accepted, may block fruitful avenues of inquiry. Finally, to deny that philosophy and formal logic can provide the criteria of what will count as explanation in empirical science is not to deny that there are philosophical and logical problems in science or that science is amenable to logical or philosophical analysis either in the sense of illuminating the structure of scientific reasoning or clarifying, and even dissolving, certain "scientific" problems, dilemmas, or confusions which have their origin in the language of science.

V. CONCLUSION: THE MEANING OF "EXPLANATION"

The social scientists' fascination with the deductive model, or at least the mandates which spring from it, is closely allied to the search for what Toulmin has termed "portmanteau characterizations of science, finding in some one requirement (such as predictive success) the unique test of a scientific hypothesis."[66] But this attempt to discover some final standard by which to define and judge scientific assertions is ultimately regressive since some exception or counter-example can always be suggested where prediction, the presence of laws, or deductive relationships between statements is not the criterion. Any analysis of scientific inquiry must include not only classificatory criteria which serve to differentiate it from other activities but also standards of "appraisal."[67] Yet it is a mistake to assume that there is some ultimate criterion and form of appraisal for scientific assertions which is based on or implied by the purpose or function of science. Appraisal in science does not carry with it any such universal criterion, nor does it entail any unitary logical standard. There is appraisal in ethics, politics, and in the various branches of science, but both the criteria and logic of appraisal differ from field to field and from context to context within a field. There is no reason to believe that in not accepting some purpose such as prediction as the ultimate standard for judging a scientific assertion or in rejecting deduction from general laws as the hallmark of adequate explanation that one is somehow placing himself outside the realm of scientific activity. One must not be mislead by the fact that scientists have purposes and science has functions and assume that there is a given purpose or function for science which governs its logic and serves as a final standard of justification and appraisal. The meaning of "science" at any time or place is determined by the paradigm which informs scientific activity and reasoning and which specifies the conception of reality and the phenomena to be investigated, designates problems for investigation, and determines the criteria of acceptable explanation and inference.[68] But the specific content of paradigms is in principle infinitely variable both in time and space.

> Science has not one aim but many, and its development has passed through many contrasted stages. It is therefore fruitless to look for a single, all-purpose "scientific method": the growth and evolution of scientific ideas depends on no one method, and will always call for a broad range of different inquiries. Science as a whole—the activity, its aim, its methods and ideas—evolves by variation and selection.[69]

Faced with the dilemmas arising out of attempts to state the governing value and defining characteristics of science, there has been

[63] Brodbeck, "Introduction," p. 2.

[64] Kaplan, *op. cit.*, p. 346.

[65] *Ibid.*, p. 3.

[66] Toulmin, *Foresight and Understanding* (Bloomington: Indiana University Press, 1961), p. 15.

[67] *Ibid.*, p. 14.

[68] See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

[69] Toulmin, *Foresight and Understanding*, p. 17.

a tendency to move toward positing some more general and seemingly more neutral formulation of the purpose of scientific activity. Probably the leading candidate in this regard is the idea "that the goal of inquiry is explanation,"[70] but such a notion seems to complicate the problem rather than solve it. Such a definition is at once too broad and too narrow, for, on the one hand, the search for explanation encompasses a greater range of activities than science and, on the other hand, it would seem much too restrictive to limit science to merely a search for explanation. It would seem that "if the goal of some branch of science does *not* consist in explaining, it can scarcely be criticized on the count of *failing* to do so."[71] A great deal of scientific activity is concerned with what might be termed "positing" and "substantiating" which cannot easily be accommodated within the notion of explanation.[72] It might well be argued that although science includes more than explanation, this is nevertheless its central aim, but then the problem becomes one of distinguishing *scientific* explanation, and inevitably one is driven back to seeking the essence, purpose or function of science and to such postures as the equation of science and a search for causes. But, as with prediction, it would appear that "neither the search for causes, nor causal explanation, is *primary* in any likely sense that may be assigned to this vague term."[73] In the end, attempts to isolate and define scientific explanation seem to result in a regressive process of conceptual reduplication.

These comments should suffice to demonstrate that there is something fundamentally wrong with this entire line of argument. There is, however, a sense in which science and explanation are related, or, better, there is a manner in which one can speak cogently about the relationship between these concepts. But this involves conceiving of explanation neither as a goal of science nor as a product, but rather as a kind of activity of which scientific explanation is one type. This idea might be expressed by saying that in this sense of "explanation" it would be as odd to say that explanation is the goal or function of science as it would be to say that playing games is the goal or function of card games. To engage in science is, in part, to engage in a particular kind of explaining activity, and "explaining" must be understood in terms of "systemic explanation" or a "generic form of explanation of which scientific explanation is only a species," and the various fields of science, such as social science and natural science, are sub-species.[74] This idea of explanation may be described as a "search for understanding" or intelligibility and it encompasses many modes of thought including logic, philosophy, science, and everyday life; "explaining, in short, is a particular way of using a form of argument; it has no logical form peculiar to it."[75] What the particular logical structure of explanation will consist of in any of these areas will differ, but "the only ultimate element in the logic of explanation is understanding itself, and that comes in many ways."[76] There are certainly aspects of scientific explanation which distinguish it from other species of explanation, but to state precisely what the boundaries are is an empirical problem and a problem to which there is no final solution. Most important, it is necessary to guard against the presumption that the logic of explanation in any particular field such as formal logic can claim trans-contextual validity or that a model, and therefore corrigible picture, of the logical structure of some sub-field, such as natural science, can be universalized into an authoritative paradigm for all sub-fields.

At first it may appear that the introduction of concepts such as "understanding" and "intelligibility" merely confuses rather than clarifies the issue, since to seek their meaning is to again embark on a regressive journey. It is obvious that what one person or community may accept as intelligibility will be rejected by another and that what will constitute scientific understanding in one historical period may be viewed as inadequate at a later time. But this is precisely the point. Explanation, and in fact most of the central concepts relating to the epistemic and logical structure of science, must be construed as carrying a certain universal force with regard to their characteristic use or meaning but varying contextually with regard to their specific and substantive meaning, i.e., with regard to their sense and reference and criteria of application. To speak of intelligibility and understanding as the characteristic logical force of explanatory assertions and as the differentiating criteria of explanatory activity is not to lapse into some sort of obscurantism. Explanation (or intelligibility) is not merely a psychological matter, for what will count as explanation as well as what requires explanation is pragmatically determined against a framework of ideas about reality,

[70] Meehan, *Explanations in Social Science*, p. 1.

[71] *Ibid.*, p. 21.

[72] Israel Scheffler, *The Anatomy of Inquiry* (New York: Knopf, 1963), pp. 53–55.

[73] *Ibid.*, p. 54.

[74] Yolton, *op. cit.*, p. 194.

[75] Toulmin, *Foresight and Understanding*, p. 99.

[76] Scriven, "Explanations, Predictions, and Laws," p. 213.

theories, and rules which together constitute a paradigm or conceptual scheme for interpreting and organizing experience. This is true whether we are speaking of philosophy, science, religion, or everyday life. What will constitute the standards of adequate understanding in any field at any time is similarly contextually determined; "what is required for understanding differs with the type of explanation," and "the epistemic features, usually in the background of explanations, emerge when we shift from one to another kind of explanation within science."[77] "Scientific explanation" can be defined as "a topically unified communication, the content of which imparts understanding of some scientific phenomenon" and

> understanding is, roughly, organized knowledge, i.e., knowledge of the relations between various facts and/or laws. These relations are of many kinds—deductive, inductive, analogical, etc. . . . It is for the most part a perfectly objective matter to test understanding, just as it is to test knowledge, and it is absurd to identify it with a subjective feeling, as have some critics of this kind of view.[78]

To throw off the burden of the deductive model and the approach to the philosophy of science and explanation advocated by logical empiricism implies first of all a re-examination, although not necessarily a rejection, of the idea that there is logical symmetry between explanations in the natural and social sciences; it allows an investigation of the actual logical and epistemological foundations of these two enterprises rather than merely accepting popular philosophical reconstructions. What is required is that social scientists undertake a consideration of what counts, or should count, as adequate explanation, and this in turn means the formulation of a substantive view of social and political phenomena which must provide the criteria for such judgment. It is precisely such an undertaking which has been prevented, or avoided, by adherence to the deductive model. Yet to reject the deductive model and support an approach to the philosophy of science which recognizes the relativity and autonomy of explanations in various branches of science immediately engenders the charge of subjectivity and the abdication of objective standards of appraisal which would allow everything from astrology to cooking to claim the title of science. What Hempel and other deductivists are searching for is a standard of explanation which clearly distinguishes scientific assertions and which is "objective in the sense that its implications and evidential support do not depend essentially on the individuals who happen to apply or to test them."[79] But to charge the advocates of the contextualist approach with abandoning the ideals of objectivity and intersubjectivity is to completely miss the point of their argument.

The reconstructionist and contextualist approaches to the philosophy of science are not comparable in the sense that the contextualist thesis is basically descriptive and explanatory rather than normative. It is not an argument for subjectivity and the abandonment of objective criteria since it assumes in any particular context that perfectly objective or inter-subjective standards may obtain, and it is not an argument advocating relativity but rather a recognition that appraisal in science is, and must be, relative to a context. The choice is not between logical positivism and subjectivism. To reject a legislative role for the philosophy of science and advocate the autonomy of scientific inquiry is not to suggest that evaluations of scientific explanations are impossible. Those who see the contextualist approach as "the current threat of subjectivism" assume that the philosophy of science must be responsible for developing standards of evaluation for empirical science, but to accuse the contextualist of advocating subjectivism because he fails to develop universal standards of appraisal and criteria for what will count as science to replace the deductive model would be like suggesting that an anthropologist must be an ethical relativist if he recognizes a diversity of moral systems and fails to establish universal moral norms for judging the values of particular societies. The analogy with morality is not strained, for one recent critic of the contextualist position sees this approach as a threat to "the moral import of science: its dynamic articulation of the impulse to responsible belief, and its suggestion of the hope of an increased rationality and responsibility in all realms of conduct and thought;"[80] He suggests that this was the real meaning of the positivist doctrine of "the unity of science,"[81] and it may well have been. But moral dogmata have often been obstacles to the advancement of science.

[77] Yolton, *op. cit.*, pp. 200–201.

[78] Scriven, "Explanations, Predictions, and Laws," p. 225; "Truisms as the Grounds for Historical Explanation," p. 452.

[79] Hempel, "Explanation and Prediction by Governing Laws," p. 130.

[80] Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967), p. 4.

[81] *Ibid.*, p. 5.

ON THE NEED FOR CONTEXTUALIST CRITERIA: A REPLY TO PROFESSOR GUNNELL

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Professor Gunnell seems to be arguing that if political scientists follow the deductivist path they will almost certainly fall into a bottomless and sterile abyss. My understanding of the deductivist conception of science, and my *verstehen* about social phenomena lead me to believe that this is a very real risk. The risk derives from the apparent complexity of social phenomena and the relatively limited ability of the human mind to deal with complexity in a rigorous deductive manner. It is just possible that social phenomena are sufficiently complex that they will never be manageable within the neat framework of deductive models. It is precisely for this reason that, at least at this point in time, I would prefer *not* to have the whole of the discipline tread this path. But why should even one man risk a lifetime of scholarly endeavor against such a possible outcome? Well, if there is another possible outcome to which he attaches a sufficiently high utility, he will take the gamble. I suggest that there *is* another possible outcome, and hope that, for some, its utility will be sufficiently high to warrant the gamble. That outcome consists in the production of a cumulatively reliable body of knowledge about those social phenomena called political. Consider the worth of being able to reduce the errors of calculation on the parts of voters, office seekers, secretaries of state, etc.

I assume that Professor Gunnell believes that the same rewards are possible along the contextualist path, but with a much lower risk of falling into the sterile abyss. However, he most certainly has not proven this in his article. What he has proven is that there is an argument about this matter currently in progress among philosophers of science. He proves this by offering us quotations from participants in the debate. Unfortunately, the quotations from the contextualists consist typically in simple assertions that theirs is the better path. Let us consider just one case.

Professor Gunnell quotes Michael Scriven as follows: "The simple fact must be faced that certain evidence is adequate to guarantee certain explanations without the benefit of deductions from laws."[1] The correctness of this assertion not being at all obvious to me, I referred to Scriven's article. Having read it, I am persuaded that the assertion is either irrelevant or incorrect. Scriven's assertion is made in relation to an illustration which he offers in argument. He suggests we consider the following situation:

> As you reach for the dictionary, your knee catches the edge of the table and thus turns over the ink-bottle, the contents of which proceed to run over the table's edge and ruin the carpet. If you are subsequently asked to explain how the carpet was damaged you have a complete explanation. You did it by knocking over the ink.[2]

He then asserts:

> The certainty of this explanation is primeval. It has absolutely nothing to do with your knowledge of the laws of physics; a cave-man could supply the same account and be quite as certain of it.[3]

Two things seem worth pointing out about Scriven's example. First, both the ink-spiller and the cave-man could be wrong, despite their respective feelings of certainty. Second, if one is interested only in this carpet, this bottle of ink, this knee, etc. and in no more general question, then the matter is irrelevant for the production of a cumulatively reliable body of knowledge.

Although it is not really the main issue, let us consider for a moment the possibility of error in this "explanation." One might first inquire into the possibility that the carpet is not actually ruined. By "ruined," I mean that there has been produced an irreversible change in the dye pattern of the carpet. Depending upon the chemical composition of the fibers of the rug, the dyes used in the carpet, and the ink, the carpet may or may not be ruined. Indeed, it may well be the case that the carpet is not ruined, but that some well meaning but improperly tutored person attempting to remove the ink may finally bring it to ruin. "Who then 'caused damage' to the carpet?" is a proper question only for metaphysicians and lawyers.

For purposes of analyzing the main issue, let us assume that the carpet is actually ruined. Is it really of any interest to those who would build a general body of knowledge which would

[1] Michael Scriven, "Truisms as the Grounds for Historical Explanation," in Patrick Gardiner (ed.), *Theories of History* (New York: The Free Press, 1959), p. 456.

[2] *Ibid.*

[3] *Ibid.*

enable man to cope better with his environs to know that "You did it, by knocking over the ink"? I think not. Of far greater interest would be answers to some of the following questions:

Why does striking the edge of the table result in the ink-bottle being turned over?
Why does the ink move laterally across the surface of the table?
Why does the ink move downward when it reaches the edge?
Why is the ink drawn into the carpet more readily than it is drawn into the top of the table?

Answers to these questions might enable one to develop ink-bottles which would be difficult to tip over, ink which would not pour easily, and carpeting which might be quite impervious to ink. Consider, in particular, how useful it has been to us to have a good deductivist explanation for why the ink moves downward from the edge of the table. In the absence of the deductivist method, there would have been little to learn from Newton's experience with the apple, save perhaps that one ought not to sit under apple trees.

What, then, is the deductivist method? Most briefly it consists in asking a series of questions, and, hopefully, of generating some answers. The central question is "How might the world be organized (i.e., according to what basic principles) such that one would logically expect the phenomenon of interest at the moment (e.g. the *downward* movement of an apple or of ink) to occur? In general, the answer requires an inspired guess, sometimes called retroduction.[4] If one succeeds in conjuring up a relatively simple set of premises which logically explain the phenomenon in question, one then asks two other questions, to wit: "If these premises are indeed operative in the universe, what other phenomena ought logically to follow from them? Are these deduced phenomenological relationships actually observable in the 'real' world?" (In this context, inductive research is primarily a search for clues as to what might be rewarding premises.)

Whatever the motivations of the individual scientist (intellectual curiosity, greed, one-upsmanship), and whatever the bases of his insights (random thoughts, inspired guesses, plagiarism), the social benefit derived from the enterprise called scientific is an increasingly reliable body of knowledge about the phenomena which encompass us. Such a body of knowledge has probably been sought by man since he first perceived the threatening nature of his environment. With such knowledge, he could enhance his chances for survival by manipulating or adapting himself to that environment. Such prestige as science may have seems to derive from the provision of that knowledge.

The success of science in this regard is the result of a characteristic relationship between its idea content and the phenomena toward which it is directed. Specifically, the former are testable against the latter. It is not retroduction which is the secret, but rather the constraints placed upon the product of that retroduction. Throughout his written history, and probably long before that, man has sought to understand the rhyme and reason of things. He has conjured up gods, goddesses, and demons to account for observed phenomena. The distinguishing feature of science is that it seeks to generate not only explanations, but logically consistent deductive explanations, testable against data. When an hypothesis fails empirically, the whole set of premises from which it is derived is called into question. When it is no longer possible to dismiss the failure as a product of measurement error, we are forced to face the fact that the theory is *empirically in error*. The deductive nature of the model then becomes relevant We search therein for a permissible adjustment; one which will allow us to account for the phenomenon in question, without losing our ability to account for phenomena already explained within the theory. No such adjustment may be possible, and that particular paradigm may have to be replaced—a most formidable task, However, if we succeed we have increased the reliability of our knowledge.

In general, I do not believe that Professor Gunnell and I are very far apart in our perceptions of the scientific enterprise—save on one decisive point. He seems to believe that in science, at least as perceived by social science deductivists, explanation is equated with generalization.[5] He cites an an example Berelson and Steiner, *Human Behavior: An Inventory of Scientific Findings*. While I cannot claim to speak for all social science deductivists, I must, for myself, dissent from his position. For me, explanation does not consist in generalization, but in deduction from premises. That a correlation has been found to exist between self-identification, in terms of social class, and party identification, and that such a correlation has

[4] On retroduction, see Norwood Russell Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958), chap. iv.

[5] John G. Gunnell, "Deduction, Explanation and Social Scientific Inquiry," this REVIEW, LXIII (December, 1969), p. 1243.

been found in numerous studies, suggesting a general relationship between these two phenomena is, to me, no explanation whatsoever. It is, instead, the stimulus for the question, "*Why* does such a relationship exist?" An answer might be derived from an appropriate set of premises, and they, in turn, might be tested against the empirical aspects of their implications. In fact, I have made some such gestures myself on this particular question.[6]

There are a few major works in political science which I believe reflect the scientific-deductivist thrust. Riker's *Theory of Political Coalitions*[7] and Downs' *An Economic Theory of Democracy*[8] come readily to mind. However, the Berelson and Steiner work most certainly should not be taken as an end product of science. It is instead, a source of stimuli for questions which *might* be answered scientifically.

Here, perhaps, it ought to be pointed out that science is not suited to answering all questions. Professor Gunnell inveighs against any hierarchy of epistemologies and I couldn't agree more. The tendency to attach status, in the sense of prestige, to any particular mode of explanation is pernicious to inquiry. Science simply does not provide an all-purpose epistemology. Its requirements for deduction in the context of the limited human mind require one to abstract and strip away much of the richness of life. Elegance and precision are gained at the cost of the poets' pleasures. It deals in the correctness of descriptions regarding relationships among abstractions, and not in the correctness of the ends of real men. Thus, to seek in science answers to questions of ethics or faith is to give science both more and less than its due.

In so far as we believe that we are dealing with caused phenomena governed by a constant set of principles (i.e., not by some variant of a Manichaean devil), the epistemology of science seems appropriate. This is not to say that no contextual questions are relevant. While I believe that the structure of deductive logic, and hence of explanation is invariant with context, the matter of empirical testing is more complex. As my colleague William H. Riker has pointed out to me, the set of constraints which enable one to characterize a field of inquiry as a "discipline" deal very much with the question of what shall count as persuasive evidence. The answer to that seems to be very much contextually determined. If one is dealing with a relatively simple set of phenomena, such as moving bodies, with concepts that are well-defined in terms of their operational implications, in the context of a theory which has yielded many accurate predictions, then a rather small and esoteric bit of evidence, e.g., a streak in a Wilson cloud-chamber, may count for a very great deal. If, on the other hand, one is dealing with a very complex phenomena, such as a human bargaining situation, with concepts which are so defined as to leave their operational implications rather vague, e.g., utility, in the context of a theory still in its infancy, then perhaps one wants a great deal of evidence, before yielding much credence to the theory.

If the problem of evidence is Professor Gunnell's central concern, then we have no quarrel. But I doubt that this is his central concern, for he keeps hammering away at the deductive model. He says, for example:

> What is required is that social scientists undertake a consideration of what counts, or should count, as adequate explanation, and this in turn means the formulation of a substantive view of social and political phenomena which must provide the criteria for such judgment. It is precisely such an undertaking which has been prevented, or avoided, by adherence to the deductive model.[9]

For me, adequate explanation, *in any field*, consists in logical derivation from premises and correct prediction. If the same derivations can be obtained from two sets of premises, I opt for parsimony. Indeed, there may be situations in which equally parsimonious theories account for equally large but overlapping sets of phenomena. Then I may not be able to choose. Yet, if there is a phenomenon of great importance to me in one of these sets, but not in the other, then I might well choose to act as though the premises explaining the relevant phenomena were truly operative in the world.

If the contextualists have other criteria for adequate explanation, or rules for developing such criteria, they should set them forth. If they wish to call them scientific, then I believe that these criteria should have the following characteristics: a) they should enable one to recognize both logical and empirical errors, b) they should permit the development of non-obvious, testable inferences, and c) they should

[6] See Arthur S. Goldberg, "Social Determinism and Rationality as Bases of Party Identification, this REVIEW, LXIII (March, 1969), 5–25.

[7] William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962).

[8] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Brothers, Publishers, 1957).

[9] Gunnell, *op. cit.*, p. 1246.

provide for explanatory paradigms with internal structures sufficiently explicated that they can be modified with inferable consequences. Of course, they may call anything they wish scientific. No sanctioning system obtains.

Although Professor Gunnell is nowhere very clear about the substance of the contextualist approach, its nexus seems to consist in the notion of understanding. Now, I have always regarded understanding as an intrasubjective phenomenon.[10] Yet the author denies that this is a problem. He quotes Scriven as follows:

> Understanding is, roughly, organized knowledge, i.e., knowledge of the relations between various facts and/or laws. These relations are of many kinds—deductive, inductive, analogical, etc... It is for the most part a perfectly objective matter to test understanding, just as it is to test knowledge, and it is absurd to identify it with a subjective feeling, as have some critics of this kind of view.[11]

I am afraid that, for me at least, this simple set of assertions does not contain evidence of its own correctness. Rather, it puts me in mind of two other quotations—one from Hanson and the other from Easton. Hanson comments:

> Causes certainly are connected with effects; but that is because our theories connect them, not because the world is held together by cosmic glue. The world *may* be glued together by imponderables, but that is irrelevant for causal explanation.[12]

For Scriven, and possibly for Professor Gunnell, knowledge seems to imply having a direct comprehension of the cosmic glue. The second quote that comes to mind is Easton's comment that "A fact is a peculiar ordering of reality in terms of a theoretic interest."[13] For the contextualists, facts seem to be readily discernible self-circumscribing sub-sets of reality.

However, I am afraid that reality is not nearly so amenable to immediate and correct apprehension as the contextualists would have us believe. It seems, therefore, crucial to have means for recognizing errors in our approximations of reality. Two such intersubjective means exist, i.e., discernment of logical errors in a model, and the failure of the model to provide empirically correct predictions. The contextualists seem to reject both of these for social phenomena. It behooves them, then, to tell us what alternatives are available.

Apparently with this in mind, Professor Gunnell, in part quoting Toulmin, asserts the following:

> It would seem that we could not ask more of an argument in any particular field than it "achieve whatever sort of cogency of well-foundedness can relevantly be asked for in that field."[14]

This is as close as the author seems to come to stating the contextualists' rules of adequate explanation. I granted earlier that the deductivists' approach runs the risk of the sterile abyss But if this is the contextualist approach, it seems to run a much greater risk of leading to the all too fecund morass.

[10] For an excellent treatment of one aspect of understanding, see Theodore Abel, "The Operation Called *Verstehen*," *American Journal of Sociology* LIV (1948), 211–218.

[11] Michael Scriven, "Explanations, Predictions, and Laws," in Herbert Feigl and Grover Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, Vol. III (Minneapolis: University of Minnesota Press, 1962), p. 225. The reader should note that while I disagree with Scriven on the matter of objectivity, I do not disagree with him in much of his work. His general theme seems to be that there are a whole host of decisions that one must make, and that one cannot wait upon formal deductive theorists. One therefore uses other ways of trying to make sense of things. Professor Gunnell, however, pushes the argument much further, seeking to carve out domains of epistemological appropriateness, and to label all of them "scientific."

[12] Hanson, *op. cit.*, p. 64.

[13] David Easton, *The Political System* (New York: Alfred A. Knopf, 1953), p. 53.

[14] Gunnell, *op. cit.*, p. 1240.

GUNNELL ON "DEDUCTIVISM," THE "LOGIC" OF SCIENCE AND SCIENTIFIC EXPLANATION: A RIPOSTE

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I. INTRODUCTION

It is impossible to tender a reply to Professor Gunnell's essay, "Deduction, Explanation and Social Scientific Inquiry," that would be both brief and adequate. It would be impossible to be brief because Gunnell conjures up a tagraggery of issues, none of which he seems prepared and/or disposed to resolve. But no matter how extensive a reply might be, it would still be impossible to conceive it as adequate for I am not sure that I, or anyone else, can determine precisely what he means to say in the essay before us. It is impossible for me to determine with any specificity whatsoever, for example, what it means to say:

> Logical empiricism as an approach to the philosophy of science has been concerned with developing formal representations or reconstructions of the logical structure of scientific explanation and with a *meta-logical* analysis of the language applied to science. In this view there is a very strict *correlation* between the philosophy of science and formal logic.[1]

I haven't the foggiest notion what a "metalogical analysis of the language applied to science" might mean. I had always understood "metalogic" to refer to discourse employing logic as its object language—just as metamathematics would be a language employed in the analysis of mathematics as an object language.[2] If this is what "metalogic" means, how one might go about undertaking a "metalogical analysis of the language applied to science"—a language conducted essentially in the *material* rather than the *formal* mode—is a complete mystery to me.

Moreover, to insist that "logical empiricists" conceive "a very strict *correlation*" to obtain "between philosophy of science and formal logic" leaves me completely baffled. What could such an assertion mean? If "formal logic" is minimally understood to refer to the systematic study of the valid forms (rather than the substantive content) of inference, and has as a consequence only the *forms* of argument as its concern—and the "philosophy of science" is minimally understood to refer to analytical and methodological discourse about science, and has as its object of concern the *substantive* claims of science (in terms, for example, of analyzing strategies employed in generating and confirming lawlike assertions, empirical generalizations, theories, direct, inverse and extensional statistical inference)[3]—and since the objects of formal logic and the philosophy of science are clearly disparate—I fail to understand how anyone, much less a "logical empiricist," could conceive of a "very strict correlation" obtaining between them.

When this confusion is coupled with Gunnell's countersuggestion that not only is there no "correlation" between formal logic and the philosophy of science, but that science is "autonomous" rather than "dependent on formal logic"—the implications stagger the imagination. In what sense would Gunnell have science (however he would like to have the term understood) be "independent" of formal logic? Does he mean to say that if a formal logician could demonstrate to an empirical scientist that the complex linguistic artifact the scientist identifies as a theory involves, in fact, one or more literal contradictions, that the scientist could simply proclaim his "autonomy" and "independence" from formal logic? While such a posture would seem incredible to me—I might timidly suggest that it would appear

[1] John G. Gunnell, "Deduction, Explanation, and Social Scientific Inquiry," this REVIEW, LXIII (December, 1969), p. 1238, emphasis added.

[2] Lukasiewicz and Carnap have used "metalogic" to refer to that metalanguage concerned with logic as its object language—more specifically with a subdomain of syntactical (not substantive) analysis which treats of logical sentences to the exclusion of specifically mathematical ones. Certainly "metalogic" would never refer to the analysis of sentences in the material mode, i.e., substantive sentences. Arthur Pap simply uses the term. "metalogic" to designate discourse about logic (as a formal language). Cf. R. Carnap, *The Logical Syntax of Language* (New York: Humanities Press, 1951), p. 9; and A. Pap, *Semantics and Necessary Truth* (New Haven: Yale University Press, 1958), p. 432.

[3] Cf. J. J. C. Smart, *Between Science and Philosophy* (New York: Random House, 1968), pp. 3–6; S. Morgenbesser, "Introduction," *Philosophy of Science Today* (Edited by S. Morgenbesser; New York: Basic Books, 1967), p. xiii.

to be equally outrageous to most other social scientists—Gunnell seems to suggest as much.

In effect, I cannot believe that Gunnell means what he says—either that, or he hasn't said what he means. Which makes it very difficult to discharge the obligation of treating his ideas adequately. Even if I had been allowed more space, his utterances, which I find singularly opaque and idiosyncratic, would have rendered such a task all but impossible.

Nonetheless, a proper regard for the integrity of our discipline and a fiduciary responsibility to our students compels an assessment (albeit inadequate) of Gunnell's arguments. I have chosen, as a consequence, to attempt to disentangle one or two major confusions in which all of Gunnell's subsequent confusions find their source.

II. THE "DEDUCTIVIST" THESIS

Gunnell's article seems to grow out of (1) a positive attraction to Stephen Toulmin's work, particularly his *The Uses of Argument* and (2) a relatively object specific repugnance to the work of Carl Hempel. Unfortunately, there is only a partial overlap of the several concerns which characterize Toulmin's work and that of Hempel. As a consequence what the one says has very little direct bearing on anything said by the other. Only in one specific, but important, instance is there a direct engagement of their concerns—and that, as I shall suggest, is with respect to scientific inference. Other than that Toulmin's interesting and insightful work can be construed as devoted to an exclusive inquiry into "the soundness of the claims we make—with the solidity of the grounds we produce to support them—or, to change the metaphor, with the sort of case we present in defense of our claims."[4] This preoccupation, what Toulmin calls a concern with the "logic" (in the informal or extended sense of the term) of inquiry in general, has very little (if any) relevance to Hempel's analysis of the logical properties of explanation. Toulmin's book on argument is a systematic appraisal of the nature of truth accreditation in a variety of scientific *and* nonscientific concerns including, among others, jurisprudence and ethics. His book is a sustained attack on the conception of "deductive inference," i.e., logical criteria, as some sort of ultimate or prescriptive truth warranting strategy subtending all rational or cognitive truth claims.

If one turns, on the other hand, to the work of Hempel one finds, in general, a relatively restricted concern with one relatively specific cognitive problem: an adequate construal of what it means to say we have achieved *scientific* explanation. Wherever Hempel has occupied himself with general problems of truth ascription and confirmation it is clear that he has not committed himself to a consideration of logical criteria of adequacy alone. He has explicitly stated that the concept of confirmation of substantive claims necessarily entails *pragmatic* as well as *semantic* concerns, i.e., concerns which are clearly *extra-logical*.[5]

Recognizing these substantive distinctions between the works of Toulmin and Hempel we find that Gunnell attempts to make the work of one relevant to the work of the other only by confounding a variety of problems. One set of problems has to do with something vaguely characterized as the "logic" of science or of inquiry in general. A host of other ill-defined problems are made to revolve around this broad question. Can, for one thing, the "logic" of inquiry be somehow construed as "deductive"? Gunnell goes so far as to maintain that some unnamed philosophers of science have insisted that "deductive inference," in some obscure sense, is the ultimate truth-warranting strategy for the *substantive* claims of science! Another set of problems has to do with Hempel's construal of explanation that maintains that certain logical properties subtend *all* competent *scientific* explanations. Certainly any decision concerning the nature of scientific explanation could not conceivably be interpreted as a solution to the question of how one might construe the "logic" of science or inquiry in general. Explanation is only *one* product of science. Science, as an activity, produces a wide variety of linguistic entities, taxonomies, typologies, axiomatic systems, calculi, descriptions, statistical generalizations, exhaustive enumerations, lawlike assertions, direct, inverse, ex-

[4] S. Toulmin, *The Uses of Argument* (Cambridge: Cambridge University Press, 1964), p. 7. That Toulmin's interesting arguments involve (in any sense of the term) a rejection of Hempel's models of explanation is, at least in part, belied by the fact that I have made extensive use of Toulmin's insights in my forthcoming *Introduction to Metapolitics: A Brief Inquiry into the Conceptual Language of Political Inquiry* (Free Press) without abandoning the substance of Hempel's construal of scientific explanation. In effect, one can find Toulmin's arguments substantially correct and yet not abandon Hempel's models of scientific explanation.

[5] Cf. C. G. Hempel, "Studies in the Logic of Confirmation," in *Aspects of Scientific Explanation* (New York: Free Press, 1965), pp. 3–51, particularly p. 51.

tensional and theoretical inferences, to mention only those that come immediately to mind.

As though these distinct, but complex, concerns were not enough Gunnell interjects still another issue. He enjoins us to consider the "more generic aspects of explanation." There are obviously more kinds of explanations than those advanced by scientists. There are theological explications (e.g. "*Explain* what Jesus meant when he admonished us to turn the other cheek."), questions about linguistic usage ("*Explain* what you meant by calling your girl-friend a dog."), lexical definitions ("*Explain* what 'diadic' means."), geometric demonstrations ("*Explain* why the area of a triangle equals $\frac{1}{2}$bh."), as well as requests for the conveyance of procedural instructions ("*Explain* how one sets a ship's course"). All this is very true—there are many kinds of explanation—but don't we really have enough problems for one essay without concerning ourselves with explanation *in general*?

But not only does Gunnell confront us with a veritable avalanche of issues—he proceeds to move artlessly and capriciously among them. He seeks to conceal his transits by attempting to relate all these issues to something he calls "the so-called deductive model."

We are never told precisely what Gunnell takes the "so-called deductive model" to be. What Gunnell calls the "deductive model" in some instances would more accurately be referred to as models of scientific explanation—since Hempel distinguishes between deductive-nomological and inductive-probabilistic models. Hempel's models attempt a construal of explanation which conceives adequate scientific explanation to be characterized by relatively specific logical properties. In other instances of its use, however, Gunnell uses the term "deductive model" to refer to questions of the general "logic" of science or to "generic explanations." Hempel's models of explanation address themselves specifically to the issue of criteria governing the adequacy of *scientific* explanation, not to the question of the general "logic" of science, and still less to explicating the *generic* notion of explanation.[6]

Gunnell seems manifestly indisposed to keep all these things separate. He uses "deductive model" to refer to all of them. He makes bewildering transits from the "deductive model" conceived as (1) a "proper reconstruction of the *logic* of *natural* science," (2) a "standard" for "*theory construction* in the *social* sciences," (3) as contributing to the explication of "a general structural description of *social scientific explanation*," and finally (4) an effort to characterize *generic explanation*.

Gunnell seems incorrigibly unaware of the ambiguity that afflicts his use of the expression "deductive model." The systematic ambiguity conspires to produce in Gunnell the conviction that "logical empiricists" somehow entertain a "deductivist definition of science" which entertains "noncontingent *a priori* extra-scientific principles and ideas of certainty and conclusiveness which belong to formal logic."

He attempts to support this particular grotesquery by producing what must easily be the most bizarre sentence in the history of scholarship. After insisting that some philosophers of science—"logical empiricists" in general and Carl Hempel and May Brodbeck specifically—employ the "ideal of analyticity" in judging "empirical or substantial arguments" he produces the following sentence in support:

> But whatever label is applied, the argument is that 'geometry is the prototype of all scientific theories' and that 'in philosophy and elsewhere an appeal either to a definition or to a logical truth is conclusive' and 'conclusive in a way that appeal to contingent truth is not' and that 'any kind of explanation of individual facts cannot possess this conclusiveness. Either the explanation is deductive or else it does not justify what it is said to explain.'[7]

The quotation is composed of four snippets or "quotlets'—the first, "geometry is the prototype of all scientific theories" is from Gustav Bergmann's *Philosophy of Science* (p. 31) where Bergmann is attempting to illustrate the logic of theory construction—recognizing full well that substantive theory is produced by "observation, induction, and deduction" (*ibid.*) and not "deduction" alone. The second snippet, "in philosophy and elsewhere an appeal either to a definition to a logical truth is conclusive" is taken from pages 369 f. of May Brodbeck's article, "Explanation, Prediction, and 'Imperfect' Knowledge," where Brodbeck is concerned with characterizing the truth of an assertion that is *true by convention*. At the same time, she makes perfectly clear (p. 370) that if a proposition is empirical it *cannot* be construed as true by convention—it is a *contingent truth*. The third quotlet, "conclusive in a way that appeal to contingent truth is not," is from the same article only separated from the antecedent quotlet by fifteen pages (it is found on page 385) and simply refers, once again, to the distinction between truths that result from lin-

[6] *Ibid.*, pp. 333–412, "Scientific Explanation," in Morgenbesser, *op. cit.*, pp. 79–88.

[7] Gunnell, *op. cit.*, p. 1241–42.

guistic convention and truths which are confirmed directly or indirectly by observations. The fourth snippet, "any other kind of explanation of individual facts cannot possess this conclusiveness. Either the explanation is deductive or else it does not justify what it is said to explain," comes once again from Brodbeck's article—only it is drawn from page 370 and refers to the *logical properties of explanation*—not to the *logic of theory construction* in general nor to *linguistic conventions*.

Gunnell has contrived the above "quotation" in order to charge "logical empiricists" with being "deductivists"—a grievous charge which he seems to construe to mean that they hold nothing to be true except definitions, formal truths and truths derived from formal truths. He does this even though Brodbeck goes to great pains to say "no one holds that deductions from definitions are the only kind we make in the course of an explanation" (p. 394 of her article, "Explanation, Prediction and 'Imperfect' Knowledge"). In effect, neither Brodbeck, nor any "logical empiricist" I know of, has confused the criteria for assigning truth status to formal truths with those appropriate to assigning truth status to substantive truths —whether such truth claims appear in explanations, theories, or as particular candidates for truth status.

Only irrepressible and endemic confusion could produce sentences about philosophers of science undertaking *metalogical* analyses of the language of science and "correlating" formal logic and the philosophy of science. Only confusion could compel Gunnell to systematically equivocate between using the expression "the deductive model" to mean, on the one hand, the disposition on the part of some formal logicians to insist "that the criteria for assessing analytic arguments should be given a preferential status, and *arguments in all fields be judged by these criteria alone*"[8]—and, on the other hand, using the same expression to refer to an entirely different issue, i.e., the claim made by philosophers of science such as Popper, Hempel, Nagel, and Brodbeck that *all scientific explanations share isomorphic properties*.

It is relatively clear, of course, that the principal advocates of the Hempelian models of scientific explanation accept the general thesis of the methodological unity of all the sciences, predicated on the hypothetico-deductive "model" of science.[9] This commitment could not entail—in fact, it would preclude—a commitment to any "deductivist thesis" that would hold that "concepts such as 'valid' and 'certain' should be interpreted in terms of formal logic" if that is understood to mean that "*formal logic* should be a standard for judging the infinite variety of *substantive* scientific arguments" (my emphasis).

To make this obvious to Gunnell I suggest that he ponder the expression "logical empiricist." Why should an ill defined group of philosophers identify themselves in such a manner —and why should they be so identified by others? I suggest that it is crystal clear to anyone that has read no more than A. J. Ayer's *Language, Truth and Logic* that they have been so identified because they wished to indicate the *significant* differences between certifying logical as distinct from substantive knowledge claims. That is why they have passed into history as "logical *empiricists*" and not "formalists," "rationalists," or "deductivists." One of the cardinal tenets which we now identify with "logical empiricism" is the claim that the evidence conditions certifying truth ascriptions in the formal language domain are irreducibly different from those certifying truth ascriptions in the substantive (or to use the language loosely, empirical) language domain. Hempel very carefully rehearses the differences between formal demonstrations and the evidence conditions which produce the warrant for empirical hypotheses.[10] The difference between formal proofs and empirical confirmation turns on the degree of reliability of their domain specific truth status. Formal truths provide *absolute* reliability, i.e., in any case involving formal inference it would be a contradiction to assert the truth of the antecedent and then reject the truth of the consequent. Empirical confirmation (on the other hand and at its best) provides for *maximal*, but, in principle, corrigible, reliability. What constitutes maximal reliability (a domain variant reliability) will be determined by the intersection of an indeterminate but finite array of factors including funded or entrenched auxiliary assumptions,[11] linguistic

[8] Toulmin, *op. cit.*, pp. 168f.

[9] Cf. M. Brodbeck, "General Introduction," in *Readings in the Philosophy of the Social Sciences* (edited by M. Brodbeck. New York: Macmillan, 1968), pp. 1–11; E. Nagel, *The Structure of Science* (New York: Harcourt, Brace & World, 1961); R. Rudner, *Philosophy of Social Science* (Englewood Cliffs, N. J.: Prentice Hall, 1966).

[10] Cf. C.G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N. J.: Prentice Hall, 1966).

[11] For a discussion of "entrenchment," cf. N. Goodman, *Fact, Fiction and Forecast* (New York: Bobbs-Merrill, 1965), chapter iv; for Hempel's acceptance of funded and contingent conditions for truth ascription in the case of substantive truths, *vide* his "Studies in the Logic of Confirmation," *op. cit.*

and quantitative precision, and the reduction of threats to internal and external empirical validity (Toulmin's "field variant conditions") —to mention only the most obvious.

Such a characterization of the variety of criteria for establishing truth status would minimally satisfy Toulmin's central arguments. To assert, as Gunnell does, that *only* "Toulmin and contextualists in general" advance the notion that "analytical arguments, and the very notion of deduction implied there, are inapplicable for judging substantial arguments whether in science, ethics, or jurisprudence . . ." is to misconceive the most central tenet of "logical empiricism" (however loosely that term is applied by Gunnell). The very notion of a distinction between logical and substantive truth claims (not to involve ourselves in assessing normative claims), the one meeting *formal* criteria, and the other *empirical evidence conditions* for truth certification, is the one tenet that has served to identify some ill-defined class of philosophers as "logical empiricists" for over a generation.

Toulmin (and Gunnell) insist that the criterion of incorrigibility is applicable to one "field" (I would suggest one "language domain," i.e., the formal domain) and cannot be used as a standard against which *substantive* truths are measured. But "logical empiricists" have never denied that. Toulmin argues that some formal logicians have. Whether or not he is correct, and if correct what that implies, has nothing to do with Gunnell's confused and confusing discussion about the "logic" of science or what "logical empiricists" conceive that "logic" to be. Nor does it have anything to do with the thesis that there is a methodological unity subtending the sciences or any attempted explication of the concept of scientific explanation.

III. "METHODOLOGICAL UNITY" IN THE SCIENCES

The thesis of "methodological unity of the sciences," as distinct from what might be called the "deductivist thesis" need claim no more than that all the sciences, in varying measure and proportion, employ the same formal as well as empirical techniques for truth accreditation. The more formalized sciences will employ highly articulated and artificial language artifacts like calculi and axiomatized systems; the less formalized sciences (like political science) will employ empirical generalizations sometimes housed in partially formalized systems called "conceptual schemata." In effect, the truths of science *are* domain variant and field contingent and every "logical empiricist" so conceives them.

When a formalized science like geometry or physics employs a calculus, its deductions are *empty*—simply because a calculus is an *uninterpreted* axiomatic system. To apply the uninterpreted system, even partially, to the real world requires restoring the binding ligaments of "meaning rules," "semantic rules," or "rules of correspondence" that relate elements of the calculus to the object world. Every "logical empiricist" knows that this requires judgment and "substantive criteria"—that is what "rules of correspondence" are all about. That Gunnell doesn't know this and assumes that there are some philosophers of science who believe that "the laws of physics" produce "automatic deductions or explanations" is staggering. Deductions are, of course, "automatic" in the sense that they obey the transformation rules of logic—but that any "logical empiricist" should think that "the laws of physics" automatically produce "explanations" is an absurdity that boggles the mind. A minimal condition for the application of any "law" (however construed) is a statement of the empirical initial conditions in which it is understood to have effect.

What we have rehearsed is a simplistic account of the hypothetico-deductive method of science. In footnote seventeen Gunnell advises us that he is not concerned with "the rather loose and variable characterization of scientific reasoning often referred to as the 'hypothetico-deductive method.' " He is concerned with the "deductive model." But as I have tried to indicate, he systematically equivocates in his use of the expression "deductive model." Is he really arguing that Hempel, Nagel, and Brodbeck conceive only analytic truths as true? If that is what he means then clearly no "logical empiricist" has ever defended such a contention.

The point to be emphasized here is that the notions of "deductivism" (whatever it means to Gunnell), the "methodological unity of the sciences," and the "deductive model of explanation" refer to a variety of distinct and essentially unrelated issues in the philosophy of science. That Gunnell chose to jumble them all together serves no cognitive purpose whatsoever.

IV. THE LOGIC OF EXPLANATION

Hempel's construal of scientific explanation asserts that any adequate *scientific* explanation of an event (the explanandum) traffics explicitly or implicitly on conjointly held (1) relevant general laws (or a relevant general law) and (2) specified particular and empirical cir-

cumstances (the "initial conditions") that (3) make the explanandum event their deductive consequence.

Once we have boiled off all the confusions which attend Gunnell's discussion, what criticisms remain lodged against the "deductive model of explanation"? The coherent criticisms that emerge are that (1) "explanations are practical, context bound affairs," that the "completeness or correctness of an explanation is a notion without meaning except in a given context" and that (2) "certain evidence is adequate to guarantee certain explanations without the benefit of deduction from laws." Now these criticisms—which are coherent—are quotes from Michael Scriven. It is clear that Gunnell takes these criticisms to be applicable to scientific explanations—one subset of explanations, but it is equally transparent that Scriven addresses himself not to *scientific* but to the "logic" of *generic* explanation. Generic explanation includes a variety of explanation-types including, as we have suggested, literary exegesis, lexical definitions, formal demonstrations, conveyances of procedures, the recounting of recipes, demands for justification ("*Explain* why you did that!") among an indeterminate number of others.

It is not surprising that Scriven can maintain that explanations are "practical, context-bound affairs," and that some explanations obtain "without benefit of deduction from laws." What function would general laws have in literary or religious exegesis? or conveying procedural information? or providing excuses? No one denies that all such activities are spoken of in ordinary language as "explanatory." But Hempel is concerned with the adequate construal of only a subset of explanations—those conceived as *scientific*.[12] Scriven's comments would be perfectly germane had Hempel addressed himself to the problem of generic explanation—but since he didn't, I fail to understand why Gunnell thinks they are relevant to the discussion.

Hempel has *not* attempted to explicate the logic of explanation in general. He would accept, I am sure, Scriven's contention that "explanation presupposes that something is understood, and a complete answer is one that relates the object of inquiry to the realm of understanding in some comprehensible and appropriate way . . . " He might also indicate that as much could be said, *to no profit*, of *any* explanation. It is as true of explaining how to fry an egg, perform logical proofs, convey lexical meanings as it is of scientific explanations. But how does this help us to distinguish *bona fides* from counterfeit *scientific* explanation? How are we to explicate "understanding," "intelligibility," "comprehensibility," and "appropriateness"? To tell us, as Gunnell does, that "explanation" is "contextually dependent" is to tell us nothing at all. To compound our frustration by confusing us with the suggestion that the "criteria of acceptance and inference" are "determined by the paradigm" is cruel. It would seem than "contextually dependent" means nothing more than "paradigm dependent" and the whole notion of "paradigms" is as opaque as "contextually dependent."

Surely Gunnell is right in maintaining that "the search for explanation encompasses a greater range of activities than science"—everyone is prepared to grant as much. But what does all this have to do with Hempelian models of *scientific* explanation?

Hempel's suggestions concerning the logical properties of scientific explanation have been the objects of scrutiny now for over a generation. They have been subject to considerable revision—both at the hands of others and of Hempel himself. For an account which advertises itself as a "thorough critical analysis," Gunnel's is singularly innocent of any substantive criticism. His account does not even reiterate past criticism. Precisely what are the objections that Gunnell raises against Hempel's construal?

Only one possible criticism remains—and that attends Gunnell's curious and *ad hoc* reference to "inference tickets." Hempel refers, in his construal of scientific explanation, to "covering laws" in his schema of scientific explanation. If "inference tickets" are to be substituted for "lawlike assertions" or "covering laws" it would be pleasant if Gunnell provided some indication of what they are supposed to be, how they function in explanation, and what objections have been raised against the entire notion.

Gunnell tells us that "generalizations, laws, and theories function as rules, 'inference tickets,' or 'warrants,' or justifying grounds in terms of which explanatory inferences are drawn, not as premises in a deductive argument."[13] In a footnote appended to this elusive pronouncement Gunnell cites Toulmin, Ryle, Scriven and Sellars as the sources of this insight.

Now the notion of "inference tickets" has a

[12] C. G. Hempel, "Explanation and Prediction by Covering Laws," in *Philosophy of Science: The Delaware Seminar* (edited by B. Baumrin; New York: Wiley, 1963), pp. 125–132.

[13] Gunnell, *op. cit.*, p. 1242.

relatively long history and seems to have been coined by Gilbert Ryle. It seems relatively clear that as a "rule" Ryle considers such a "ticket" or "warrant" as neither true nor false. Gunnell is clearly committed, as a consequence, to the construal which argues that what we call "laws" are more *rules* than *propositions*—that (as Toulmin insists) the "laws of nature tell us nothing about phenomena, if taken by themselves, but rather express the form of a regularity whose scope is stated elsewhere; and accordingly, they are the sorts of statements about which it is appropriate to ask not 'Is this true or not?' but rather 'To what systems can this be applied?'" In effect, Toulmin is arguing an instrumentalist position and a "law" is not a "law" at all—but a "principle" or "rule." The "laws" employed by scientists in making inferences are more like "rules and regulations. These are not themselves true or false . . ."[14] Now such a construal of the "laws" with which scientists operate can only apply to a special subset of laws, the generic and most basic "laws of nature" for example (if they apply to any at all), because many if not most laws, as they are employed by scientists do, as a matter of fact, specify their scope of application. Moreover, with some possible few exceptions, the laws employed by scientists (particularly social scientists) can be directly or indirectly falsified by empirical observations, which means that they can best be construed as empirical generalizations or general lawlike propositions that can only be characterized as being either empirically true or false. To refer to "laws" (particularly those of interest to political scientists) as "inference tickets," implying they are more rules than propositions is, at best, deceptive.[15] Michael Scriven himself has raised similar objections in his review of Toulmin's book[16]—all of which makes Gunnell's invocation of "inference tickets" singularly ineffectual in the criticism of Hempel's models of explanation.

No discussion of Sellar's interpretation of "inference tickets" will be attempted here because it would involve a digression of imposing length—and because I am not at all convinced that Gunnell is serious in his introduction of the notion. In the article by Brodbeck ("Explanation, Prediction and 'Imperfect' Knowledge," pp. 383–85), to which Gunnell frequently refers she reviews the difficulties which afflict the notion of "inference tickets." Nagel, in turn, discusses some of the problems generated by the introduction of this vexed notion in his *Structure of Science* (pp. 129–140). Gunnell neither reviews these criticisms nor rebuts them. This is sad, because his injection of the notion of "inference tickets" is the sole substantive criticism of Hempel's construal of scientific explanation offered in Gunnell's essay. Finally, and as an aside, since none of the protagonists of the "inference ticket" notion ever suggested that a theory could be an "inference ticket" or a "rule" (as Gunnell insists), I suggest that Gunnell offer some account of what he might mean by the now hopelessly tortured expression.

V. CONCLUSIONS

All this suggests to me why I find Gunnell's discussion confusing: Gunnell is confused. His discussion is, in various places, either confused, trivial or basically mistaken—and where it is none of these—it is irrelevant to the central issue—which I understood to be the adequacy or inadequacy of Hempel's models of explanation. Gunnell does not seem to have, for one thing, the vaguest idea of what the claims of "logical empiricism" might be—furthermore, he has failed to attempt even the most elementary analysis of the models of explanation Hempel advances—and finally he has left us with notions about explanation that can only be stultifying and irredeemably subjectivistic and obscurantist.

What, in effect, does Gunnell imagine an explanation to be? We are all prepared to grant that any effort to unpack so complex and systematically ambiguous a term must necessarily be difficult. We are all equally aware that the issues can be reduced, in some sense, to "appraisals" of adequacy. And surely no one would resist the suggestion that there are different standards for adequacy governing the variety of language domains which reveal themselves in field-variant criteria (in ethics, politics and the various branches of science). But to suggest that not only "the criteria of acceptable explanation and inference" but the very "meaning of 'science' " is *determined* by something he calls a "paradigm" is really to tax our patience. Such an assertion would mean that something called a "paradigm" develops and *then informs* "science" and *determines* what an "acceptable explanation" and an "inference" might be! And how, and where, pray tell, does a "para-

[14] Cf. S. Toulmin, *The Philosophy of Science* (London: Arrow, 1962), pp. 86 and 79.

[15] H. Gavin Alexander, "General Statements as Rules of Inference," in *Minnesota Studies in the Philosophy of Science* (edited by H. Feigl, M. Scriven and G. Maxwell; Minneapolis: University of Minnesota, 1958), II, pp. 309–29.

[16] M. Scriven, *Philosophical Review*, XXIV (1955), 124–128

digm" originate? Is the articulation of a "paradigm" *antecedent* to logic, as well as prior to, and independent of, the collection, processing, and interpretation of data? Surely a "paradigm" must be a most unusual kind of cognitive product—recognizing no logical or linguistic rules nor observational constraints.[17]

Gunnell does want to address himself to "explanation"—and we are told again that there are many kinds of explanation. With which we can only (once again) heartily agree. (We thought we were only concerned with *scientific* explanation.) We are then told that "explanation" is a "kind of activity."[18] If Gunnell wants to call "explanation" a kind of activity we can grant that without making too much of the issue. But just what kind of activity might "explanation" be? We are told that "explanation" is a "search for understanding." Even Gunnell seems prepared to admit that this is not a very sharp analytic distinction. By characterizing explanation as "a search for understanding" we have no reason to exclude from the reference class yoga exercises (done to "enhance your understanding"), phrenology, tealeaf reading, crystal ball gazing, Christian Science, and reading Mao's Little Red Book, as well as a variety of other activities advertised as constituting just such a search.

But what *can* "understanding" (not to concern ourselves with the "search" for it) mean? It is "obvious," Gunnell tells us, that what one person or community may accept as intelligible or understandable will be rejected by another. This is as true for scientific understanding as it is for any other variety. But should we take this to mean that every man should be his own "explainer?" No indeed. Explanation, understanding and intelligibility are not simply "psychological matters," simple avowals that one understands or finds something intelligible. That would be admittedly obscurantist. Gunnell tells us that what will count as an explanation is "pragmatically determined against a framework of ideas about reality, theories and rules which together constitute a paradigm . . . "[19] But we had just been told that a "paradigm" determines rules of inference—and remember we were also informed that a "theory" is a "rule of inference." Now I am hopelessly confused. Do theories and inference rules (they are two ways of saying the same thing apparently) constitute part of the "paradigm" or does the antecedent "paradigm" determine inference rules and theories? It doesn't seem that one can have it both ways—but since Gunnell feels that "substantive" claims are "independent" of logic, perhaps this is an illustration of his more generous rules of inquiry.

But more than that, if we commit ourselves to a notion of explanation "pragmatically determined against a framework of ideas about reality, theories and rules which together constitute a paradigm," would we not be compelled to admit that the necromancer provides perfectly legitimate "explanations"? And what of devil-worshippers? Do they not meet the same criteria of adequacy? Explanations by flying-saucer addicts and Zoroastrians would seem equally well to meet such adequacy criteria. The real question is: who could *not* meet such loosely framed and porous criteria?

But really what *is* to count as an explanation or as an understanding? Gunnell tells us that "in any particular context" he can provide "perfectly objective or inter-subjective standards." Would he provide just *one* instance to support such a claim? Perhaps he might provide a "contextual" explanation in political science. I suspect, should he attempt such a *tour de force*, what would result would be the generation of a collection of propositions containing one or more lawlike assertions which, conjoined with some reasonably adequate statement of initial conditions, would logically imply the event to be explained. The lawlike assertions would be perhaps statistical or unrestricted generalizations—and the initial conditions would be essentially descriptive—and the relationship between the generalizations, the initial conditions, and the explanandum would be strict entailment or probabilistic subsumption.

Having done that, Gunnell would have resolved some of his confusions and then he would have to rewrite his paper—and start somewhere near the beginning.

[17] For a discussion of Thomas Kuhn's notion of "paradigms" in science, cf. I. Sheffler, *Science and Subjectivity* (New York: Bobbs-Merrill, 1967), pp. 74–89 and Gregor, *op. cit.*, chapter 10.

[18] This, on the face of it, seems to involve what has been called the process-product confusion—there is the *process of explaining*, which is an activity, and then there are *products called explanations*—in much the same way that we say we both swim and have a swim.

[19] Gunnell, *op. cit.*, p. 1242.

SCIENCE AND THE PHILOSOPHY OF SCIENCE: A REJOINDER TO PROFESSORS GOLDBERG AND GREGOR

JOHN G. GUNNELL

I

Since both Mr. Goldberg and Mr. Gregor have limited their responses principally to my specific criticisms of the deductive model, it would seem appropriate to emphasize once more the purpose of this criticism. The issue which I wish to raise, using the discussion of the deductive model as a vehicle, goes far beyond the model itself and even the approach to the philosophy of science which it represents. It should be apparent that there may be little chance of resolving the differences that stand between the respondents and myself regarding either the character of the philosophy of science as a discipline of inquiry or its product, but to some extent these differences can be separated from the more general problem of the relationship between social science and the philosophy of science.

I suggested that there is a significant intellectual lag or gulf between political science and contemporary work in the philosophy of science. This situation need not, in principle, be viewed as odd or even undesirable since the distance between philosophy and other fields of science is at least as great. What is unique about political science is that, although it has lost touch with philosophy, it has to a large extent derived its conception of science and its notion of the procedural rules of empirical inquiry from a restricted body of literature in the philosophy of science, i.e., traditional logical empiricism. Among the many notions appropriated from this literature is a relatively well-defined philosophical reconstruction of the logical structure of scientific explanation which I, and many others including some of its proponents, refer to as the deductive model. It is doubtful that political scientists have correctly understood the character of either this model or logical empiricism, but it is undeniable that they have taken the model, or at least their interpretations of it, as an authoritative representation of the nature of science in general, a guide to substantive empirical inquiry in social science, and a formal measure of adequate explanation. One important derivation of the attachment to this model has been the assumption that any complete and truly scientific explanation in social science must involve laws and generalizations. In this essay I attempted neither to formulate any alternative formal criteria of adequate explanation nor to present any substantive notions about the explanation of social phenomena. My purpose was to call into question both the deductive model and certain other aspects of logical empiricism and to suggest that the specification of the criteria of acceptable social scientific explanation belongs to social science. It is doubtful that it is either logically or epistemologically sound for any empirical discipline to seek its methodological categoricals and concepts of explanation from the philosophy of science, but once it has taken this step, it can hardly avoid the implications of controversies in the philosophy of science for its own conception of inquiry. Probably at least part of the historical explanation for the acceptance of logical empiricism and the deductive model was its utility as an ideological device in the struggle for the autonomy of empirical political science; it provided a convenient set of definitions of science and scientific explanation. But to continue to simply catechistically intone these largely unexamined precepts, especially when logical empiricism and the deductive model are being soundly attacked within the philosophy of science itself, can no longer be regarded as sufficient.

II

I am not sure how much divergence there is between Goldberg's position and mine except on certain issues which may involve misunderstanding rather than disagreement. Goldberg seems to believe that I am advocating something which he terms "the contextualist path" in political science but that all I have "proven" is that there is "an argument about this matter currently in progress among philosophers of science." If I have succeeded in "proving" this point, then I have accomplished much of what I wished to do within the limits of this specific essay. To admit that many of the theses of logical empiricism, including the deductive model, are seriously being questioned in the literature of the philosophy of science should certainly raise doubts about whether political scientists can any longer merely repeat popular versions of these theses and assume that they are delivering final truths about the nature of science. With regard to my recommendation of the "contextualist approach," I do recommend it as an approach to the philosophy of

science (and the philosophy of the social sciences), but I explicitly denied that it entailed any recommendations about the character of explanation in political science or any other science; the formulation of such answers is precisely what the contextualist argues philosophy should not undertake. To ask what, in general, this approach specifies as the "criteria for adequate explanation, or rules for developing such criteria" is to ask an inappropriate question, i. e., from the contextualist perspective it is science and not philosophy which establishes these criteria and rules while the philosophy of science seeks to understand and explain the character and content of these criteria and rules.

My references to the contextualist literature and statements such as those of Scriven were intended to demonstrate that the deductive model is not free from challenge. Scriven's assertion about the adequacy of explanations which do not include deductions from laws cannot be passed off as "either irrelevant or incorrect." Goldberg's discussion of this point leads me to believe that although he has read the article from which the statement is extracted he has not grasped the thrust of Scriven's argument which can be briefly, although partially, summarized as the idea that a judgment of the adequacy of an explanation depends primarily on the criteria of acceptable explanation formulated and applied in the community of science in which the explanation is offered, and if science (either social or natural) does in fact count certain explanations as adequate and complete when deduction from laws is not involved, then it makes little sense, either logically or epistemologically, for philosphers to designate them incomplete or imperfect.

What Goldberg characterizes as "deductive explanation" and "the deductivist method" appear to have little to do with what I explicitly stated that I meant by the phrase "deductive model." Goldberg seems to be offering a general characterization of the H-D method which I specifically excluded from my discussion. When Goldberg states that he believes "that the structure of deductive logic, and hence of explanation is invariant with context," he would appear to directly confront my argument which is directed against any simple equation of explanation and deductive logic. But he does not pursue this point, and I am not at all sure what Goldberg means by "deductive logic," which is far from an unproblematical notion. He states that "adequate explanation, *in any field*, consists in logical derivation from premises and correct prediction," but this is both vague and ambiguous. Is it a philosophical recommendation or an empirical generalization about science? Is it an affirmation of the thesis of the logical symmetry of explanation and prediction in any field of science? And above all, what is the meaning of "logical derivation" in this case?

Finally, although I attempted to carefully specify what I meant by the relationship between understanding and explanation, I suppose that it was inevitable that it would be taken as an advocation of some kind of intuitive approach grounded on "direct comprehension." I can only reiterate that my discussion of "understanding" does not refer to any approach at all, but only to the view that it makes no sense to attempt to offer a universal characterization of scientific explanation and that neither the formal nor substantive meaning of explanation and understanding can be discussed apart from particular scientific contexts.

III

I might be able to make better sense of Gregor's "riposte" if I understood the meaning implied in his use of the term. After surveying some of the possible dictionary definitions and applying a "contextual" analysis it would seem that what he has in mind must be a "retaliatory verbal sally."[1] I find it difficult to separate the substance of his criticism from the many overt and implicit accusations, the sustained misrepresentations of my argument, and the extravagant and emotive language which he employs (tagraggery, idiosyncratic, bewildering, grotesquery, snippets, irrespressible and endemic confusion, absurdity, jumble, trivial, stultifying, irredeemably subjectivist and obscurantist, etc.). I can appreciate his hypersensitivity to the issues I have raised, since in a recent article published in the REVIEW he finds it adequate to state without elaboration that "for a science to most reliably discharge its two principal functions, explanation and prediction, statements embodying acquired knowledge must be systematically organized in subsumptive or deductive relations."[2] He then goes on to provide a sketch of the formal characteristics of a "theory" that is little more than a précis of statements from the literature of logical empiricism which he supports only by footnotes referring to the traditional wisdom literature and philosophers such as Braithwaite,

[1] *Webster's Third New International Dictionary* (Springfield: G. & C. Merriam, 1963).

[2] "Political Science and the Uses of Functional Analysis," this REVIEW, LXII (June, 1968), p. 425.

Rudner, and Nagel. This is precisely the kind of ritualism that I wish to question. Since I am limited by the space allotted for my reply, I cannot respond to all of Gregor's "confusing" remarks, and "no matter how extensive a reply might be, it would still be impossible to conceive it as adequate for I am not sure that I, nor anyone else, can determine precisely what he means to say." But nevertheless I am compelled by "a proper regard for the integrity of our discipline and a fiduciary responsibility to our students" to attempt an "assessment." Enough of this pompous nonsense.

By characterizing logical empiricism as concerned with a "meta-logical analysis of the language applied to science," I am simply referring to an analysis of terms such as "explanation," "inference," "theory," which are common in discussions of the logic of science as distinguished, for example, from the actual language of science (although the two may overlap). This is not an unusual use of the term "meta-logical," and there is no reason to suppose that all the language "applied" to science is in the "*material* rather than the *formal* mode." But this point, as well as my assertion to the effect that logical empiricists conceive "a very strict correlation between the philosophy of science and formal logic," are clarified in the argument of the essay, and I am not at all sure what Gregor's purpose is in fastening on a couple of sentences and attributing all sorts of hypothetical dicta to them. Although both the logical empiricist (or reconstructionist) and the contextualist see the task of the philosophy of science as logical analysis and an explication of the logic of science, their conceptions of both logical analysis and the logic of science are, as I attempted to demonstrate, quite disparate. My "countersuggestion" regarding the relation between formal logic and the philosophy of science involved my approval of the contextualist position. It should be obvious that to press these distinctions has nothing whatsoever to do with whether formal logic is relevant to science in the sense of, for example, pointing up "literal contradictions" in a theory.

Whether or not these two approaches to the philosophy of science and these conceptions of the logic of science are compatible or complementary is a more difficult problem. I tend to think that they are not, but the answer to this question is not as crucial to the main argument of my essay as a rejection of certain specific products of the reconstructionist approach. It should be clear to anyone familiar with this literature that the relation between, for example, the work of Toulmin and Scriven on the one hand and Hempel and Brodbeck on the other cannot be passed off, as Gregor suggests, as having "very little direct bearing on anything said by the other." At least this is certainly not the judgment of the individuals involved who have expended a great deal of effort in criticizing one another. He states that the only "direct engagement" is with regard to "*scientific inference*," but if Gregor is suggesting that the deductive model does not relate to the problem of scientific inference, I am mystified. He then proceeds to a very partial characterization of the positions of Toulmin and Hempel and suggests, as he does throughout his riposte, that these positions are really not at all in conflict, either in approach or substance. The only evidence of such a consensus which he introduces is in a footnote where he tells us that in a forthcoming book he has "made extensive use of Toulmin's insights . . . without abandoning the substance of Hempel's construal of scientific explanation." Social scientists have tended to believe that there are no important disagreements in the philosophy of science and that it offers a neat and integral body of knowledge to which they can authoritatively refer for guidance, and Gregor seems to wish to perpetuate this myth.

Despite the fact that I explicitly stated that it was a mistake to equate the activity of science with explanation, alone, Gregor accuses me of such a move, and then proceeds to question the relevance of my discussion of the "generic aspects of explanation" without any reference to the context in which I introduced the notion, i.e., with regard to questioning the assumption that the meaning of scientific explanation could be specified in a non-contextual sense. My doubts that Gregor carefully examined my argument are corroborated by the fact that although I devoted nearly a thousand words to a detailed statement of what I mean by the "deductive model" and in a long footnote differentiated it from other possible interpretations (including that of Popper, which Gregor suggests I equate with those of Hempel, Nagel, and Brodbeck), he nevertheless asserts that "we are never told precisely what Gunnell takes the 'so-called deductive model' to be."

If I am to go down in history for "producing what must easily be the most bizarre sentence in the history of scholarship," I am certainly happy that Gregor will allow that the sentence belongs to "scholarship," because I am not at all sure about the genus to which Gregor's interpretation of that sentence belongs. Notwithstanding Gregor's assertion to the contrary, each of my "quotlets" is extracted from the context of arguments which imply that the standard of acceptable scientific argument and

justification is analytic or demonstrative argument. The purpose of the sentence is to summarize that portion of my argument which preceded it and in no way is it offered as a proof "that logical empiricists" are necessarily "deductivists." These terms are certainly not definitional equivalents although it is within the philosophy of logical empiricism that the deductive model has been formulated. There are many other claims of logical empiricism, especially those revolving around the cognitive status of theories and the relationship between theoretical statements and observation statements, which I believe are fundamentally unsound but which I explicitly put outside the boundaries of this essay. If Gregor wishes to insist that a concern with "*empirical evidence conditions*" is the distinguishing characteristic of logical empiricism, I have no particular quarrel with this view, but I fail to see its direct relevance to my arguments here. It is certainly crucial to my thesis to demonstrate that those who embrace the deductive model assume that demonstrative argument is a norm for judging the validity of scientific argument. Gregor clearly disagrees with this, but, instead of confronting the issue, he simply feigns incredulity, states repeatedly that it is not the case, and jumps off onto such other points as the proper definition of logical empiricism, restatements of the H-D thesis, and what amounts to a summary of Hempel's reply to Scriven (Gregor's footnote 10).

Gregor devotes considerable attention to my "curious and *ad hoc* reference to 'inference tickets'," and I am quite willing to admit that I raised an issue which I could not adequately discuss within the scope of this essay. The problem of inference tickets raises a number of questions relating to the complex problem of theoretical instrumentalism and realism which I wished to rule out here, and my only defense is that it was Gregor, not I, who "tortured" the "expression." I have discussed these problems and their implications for social science elsewhere (my footnote 17), and my only purpose in mentioning the problem was to indicate that there are concrete arguments against the deductivist's conception of the logical function of laws in scientific explanation and the necessity of such laws for adequate explanation.

With regard to Gregor's discussion of my concluding remarks on science and explanation, I can only agree with his admission that he is "hopelessly confused," but it would be presumptuous for me to claim complete credit for the extent of his confusion. Rather than discuss the issue which is raised in this section, Gregor is content to engage in a facetious and ridiculous parody of my argument. That argument is that social scientists in their search for scientific respectability have turned to a restricted body of literature in the philosophy of science for authority and have found, especially in the deductive model and other formulations of logical empiricism, a set of ready-made answers about the meaning and purpose of science, the unity of science, the logic of science, and the character of scientific explanation. What they have derived from this literature amounts to little more than a series of empty cliches about the equivalency of explanation and generalization, the logical symmetry of explanation and prediction, and the deductive subsumption of particulars under laws. I would again suggest that it is, in principle, a mistake to assume that any scientific community can turn to the philosophy of science, as an academic discipline, for standards of adequate explanation and that, in particular, logical empiricism, as an approach to the philosophy of science, and the deductive model as its product, have severe deficiencies. This belief in the authority of the philosophy of science and the deductive model has in part been responsible for the failure of social science to develop a substantive conception of what constitutes adequate explanation of social phenomena. This is the task of social science, and to assume, as Gregor apparently does, that there is some final incorrigible meta-scientific standard, either logical or epistemological, to which we can appeal regarding what will constitute acceptable scientific inferences and explanations in any field of natural or social science is precisely what I wish to call into question. I would reject the idea that the task of the philosophy of science is to produce answers to abstract questions about the purpose, meaning, and structure of all scientific explanation. To suggest, as I do, that the meaning of science is "determined by the paradigm which informs scientific activity and reasoning and which specifies the conception of reality and the phenomena to be investigated, designates problems for investigation, and determines the criteria of acceptable explanation and inference" in no way entails asserting the impossibility of discriminating between science and necromancy. But whatever may account for the differentiation between science and necromancy in our society, it is doubtful that it can be attributed to the deductive model.

COMMUNICATIONS

To the Editor:

At the "open meeting with the editor" held at the recent APSA convention, you expressed the view that it was not the function of the APSR to respond to the diverse tendencies within the organization, since "the APSR cannot do everything." Instead, you suggested, APSA encouragement of other journals, such as *Public Policy*, could meet these needs. My impression was that you wish to reserve as much space as possible for the backlog of sound articles of the type currently dominating the APSR and do not wish to sacrifice space to meet the demands of other tendencies within the organization.

In my opinion, this policy is wrong. It is one which seems designed to promote a formal split within the APSA. For most members, the APSR *is* the APSA; it is the only concrete manifestation of our organization that they ever see, with rare exceptions. If the APSA is a democratic organization, then the APSR must be a representative journal.

The APSR is not now representative of the diversity of what is best in our profession. As an undergraduate, graduate student and now teacher at "Ivy League" and other East Coast schools, I have noticed how few of the better teachers seemed to pay any attention to the APSR. Instead, I constantly encountered colleagues turned off by the Review and the association. In fact, the supporters of the Review whom I have met number no more than a handful. I believe that the Review represents a vast failure in communication and a lost opportunity.

It does not need to be this way. There are many things that can and should be done, but in this letter I would like to outline only four—two in response to failures of communication and two in response to failures of representativeness.

The first failure of communication lies in the area of methodology. Most members of the association receive the APSR and cannot understand most of its articles. In my opinion this is not primarily because APSA members are ill-trained or stupid. I myself teach undergraduate and graduate-level coures about statistics, yet I must struggle with many of the articles simply because the presentation of method is condensed into virtual code. If the APSR is going to publish articles with complex methodologies, then it has an obligation to explain those procedures in greater detail for the general political scientist. It has that obligation if it conceives of itself primarily as a medium of communication, rather than primarily as an avenue for advancement through publication or as a club for methodologists.

Viewing the APSR as a medium for communication brings me to my second point. From the point of view of communication, the APSR is a literary and aesthetic disaster area. The APSA should spare no expense in hiring someone literate with an eye toward format diversity, layout design, reader engagement, not to mention style. Let us reintroduce "sophistication" to elegance.

Third, and more important, the APSR will never be representative until it accepts partisan politics as a legitimate human activity. It is a fact that our profession contains thousands of persons like myself who have come to political science because of our interest in politics, not science. The APSR may select articles partly from the view of building the study of politics into a rigorous, scientific thing. But it must also select articles from the view of substantive relevance to partisan issues now widely felt to be important. The APSR cannot be a representative medium of communication while denying partisan exchange.

Finally, there are many in our profession who, in addition to keeping "up" with the field and perhaps publishing, have a deep, perhaps deeper interest in teaching. Most of us could benefit from studies and experiences related to teaching political science. It is a disgrace to our profession that teaching has been shown to rank so low in prestige among our members. There is quite a bit of relevant information around, most of it in other disciplines; its publication should be encouraged by the APSR, and not relegated to the backwaters of PS (which would thus confirm our profession's attitude toward teaching as an inferior relation).

These proposals are not radical. In fact, they are only the barest beginning toward penetrating the spell which seems to enchant the association.

Dave Garson

Tufts University

To The Editor:

Professor William Riker has reported the results of a study in which some of his subjects divided the payoffs to coalitions in a three-person, non-zero-sum game as the solution notions of von Neumann and Morgenstern, and Aumann and Maschler prescribe. He contrasts

his findings with results I obtained in a three-person, zero-sum game study, where subjects frequently divided payoffs to a coalition equally. Professor Riker also states that he obtained strong evidence that my results can be disregarded "as the artifact of particular and restrictive institutions"—the particular conditions of my study. Essentially, his argument is that the prescriptive solution notions of Aumann and Maschler are quite successful as a descriptive theory, and that the results I obtained—the equal divisions of the payoffs—are idiosyncratic and result from the artificial, unrepresentative conditions of my study.[1]

After examining Professor Riker's results quite carefully and reconsidering my own work I must conclude that my results are not an artifact. Unfortunately the space limitations of this journal do not permit me to discuss Professor Riker's results and my own in detail, but a more comprehensive discussion of the issues is contained in a research memorandum which is available.[2] In the memorandum I offer and discuss the following, and some other arguments.

1) The result that Professor Riker has called an artifact has been found so frequently by myself and others, and is so pervasive a feature of experimental game studies, that it does not seem to make sense to consider it an artifact.

2) In his study Professor Riker used procedures quite different from mine; he employed a three-person, non-zero-sum game while my subjects played a three-person, zero-sum game. In addition, Professor Riker permitted communication among his subjects outside of the experimental situation. I employed more conventional procedures, restricting the communication among subjects to the experimental situation.

3) Further analyses of Professor Riker's data and my own indicate that, although there are differences in our results, both of us obtained some outcomes that are equal divisions, some that conform to the Aumann-Maschler prescription, and some that have neither of these two characteristics.

Finally, in my memorandum, I describe an empirical study that, if done, would shed some light on the question of whether Professor Riker's results or my own are more representative of behavior in game situations.

BERNHARDT LIEBERMAN
University of Pittsburgh

TO THE EDITOR:

I am sorry that I may have hurt Professor Lieberman, whom I respect as a very conscientious scholar. He apparently believes my use of the word "artifact" reflects on his conscientiousness. It certainly was not intended to do so. We do have, however, a genuine intellectual disagreement.

The disagreement concerns the appropriate conditions in a test of the validity of game theoretic predictions. Von Neumann and Morgenstern and Aumann and Maschler in setting up the initial circumstances of the simplest three-person game assume an indefinitely lengthy amount of negotiation. Obviously no real world experiment can satisfy that assumption. But one can approximate it so far as is practical. The difference between us concerns how far one ought to go in such an approximation.

Professor Lieberman believes that, regardless of the assumptions of the theory, one should conduct experiments in the conventional ways of psychologists whose traditions certainly have not dealt with such complex and thoughtful behavior as is involved in games. Presumably out of fear of some sort of contamination he did not allow his subjects to spend enough time on the game to understand it. Hence, in my opinion, his subjects did not engage in a meaningful test of the theory simply because they never had a chance to do what theory assumes that they will do, namely learn and understand the game.

I do not argue that, on the contrary, my subjects did engage in a valid experiment. All I assert is that they came closer to it because they negotiated for nearly an hour in each game (as against a couple of minutes for Lieberman's subjects) and they played over a period of several weeks and talked about the problems of the game with other players (as against Lieberman's subjects who spent only one several hour session on the game).

[1] William H. Riker, "Bargaining in a Three-Person Game," *The American Political Science Review*, 61 (1967), 642–656. My results are reported in two papers: Bernhardt Lieberman, "Experimental Studies of Conflict in Some Two-Person and Three-Person Games," in J. Criswell, H. Solomon, and P. Suppes (eds.), *Mathematical Methods in Small Group Processes* (Stanford, Calif.: Stanford University Press, 1962) and "*i*-Trust: A Notion of Trust in Three-Person Games and International Affairs" *Journal of Conflict Resolutions*, 8, (1964), 271–280.

[2] The memorandum can be obtained by writing to me at the Department of Sociology, University of Pittsburgh, Pittsburgh, Pennsylvania, request: *Not An Artifact*, Research Memorandum SP-108. 1.

A three-person game is more complex than poker. Sophisticated behavior in such a game—and all game theory is about sophisticated behavior—can occur only after considerable immersion, probably on the order of tens and even hundreds of hours of thoughtful experience. Since the conditions of Professor Lieberman's experiment largely prevented the occurance of sophisticated behavior in the game, the majority of his outcomes are, from the point of view of the theory, second-best. It is in that sense that his results are an artifact of the conditions, as are the results of all other investigators whose experiments embody the same conditions.

WILLIAM H. RIKER
University of Rochester

TO THE EDITOR:

There seem to be two primary justifications for the social sciences to develop their own vocabulary, or jargon. The first is to denude concepts of connotations suggested by everyday usage that are not intended by the scholar. The second is to allow scholars working in a given field to achieve some consensus on the names by which social phenomena are to be known. This latter function expedites communication among researchers, and serves to provide key-word referents that allow them to place new information in the context of the appropriate body of accumulated knowledge. The scholar who uses conceptual terms merely as pegs upon which to hang bodies of data does disservice thereby to himself and to his colleagues.

Such a disservice was performed by Boyd in his paper on "Presidential Elections: An Explanation of Voting Defection" (this REVIEW, June, 1969, pp. 498–514). Boyd calls his major independent variable cross-pressures. According to my reading of the literature, the cross-pressured individual is one who belongs to multiple reference groups, at least two of which have mutually incompatible behavioral expectations of him, e.g. one expects him to vote Democratic, while the other expects him to vote Republican. In Boyd's usage, however, the notion of reference groups has little relevance.

Boyd defines cross-pressures as a state of incompatibility among party, candidate, and policy orientations. This operationalization involves an intrapsychic rather than a social structural phenomenon and there exists a large body of social psychological literature on such cognitive-affective inconsistencies. Save for a passing reference to a paper by Rokeach and Rothman, we have no indication that Boyd sees the relevance of this literature to the problem that he is confronting. I would expect a researcher to place his findings in their appropriate theoretical context, especially where he must cross disciplinary lines to do so. As an expert in his field, he would perform a service to his readers if he informed them of relevant lines of thought that he presumably is more aware of than they are.

Boyd's labelling of his independent variable has a further theoretical dysfunction, for it confuses the relationship between status inconsistency, social mobility, and cross pressures. While both status inconsistency and social mobility may be seen as contributing to cognitive-affective inconsistency (there are other causes as well), they are special cases of, rather than causes of, the cross-pressure phenomenon.

None of this criticism, of course, is meant to challenge the importance of Boyd's findings. If anything, they become more interesting when placed in the proper theoretical perspective.

DAVID R. SEGAL
University of Michigan

TO THE EDITOR:

At first blush it appears that Professor Segal is arguing that it is improper to define cross-pressures as an attitudinal as opposed to a structural property. Only if this were the right interpretation would his point that status inconsistency and social mobility are special cases of cross-pressures be of any merit. However, other points in the letter—plus the fact that I am certain that Professor Segal knows that all of the voting research of Michigan's Survey Research Center defines cross-pressure as an attitudinal state—lead me to believe that he accepts the propriety of my attitudinal operationalization.

What he is saying, I conclude, is that if I am going to define cross-pressures in terms of attitudes, I should have utilized more the social-psychological literature on attitudinal conflict (e.g., dissonance, balance, and congruity theory). As a matter of fact, in the initial stages of the research, I made a considerable effort to construct my hypothesis in terms of one of these "strain toward consistency" theories. However, I discarded my work because at critical steps either I failed to come up with interesting deductions from these theories or I failed to find measures of some necessary concepts across the three election surveys.

RICHARD W. BOYD
Wesleyan University

To the Editor:

Professor Giovanni Sartori in his "Politics, Ideology, and Belief Systems" (this Review, June 1969, 398–411) sets out to accomplish some important objectives. Most significantly, he seeks to "explain" ideology, and to use ideology to "explain" politics (p. 409).

Imposing as Sartori's formulations are, they may be open to two qualifications:

1. Sartori employs two important dimensions of ideology to construct a typology of belief systems: (1) cognition: whether a belief system is open or closed, and (2) emotive intensity: whether a belief system is held with low or high intensity. He identifies four types of belief systems: Adamant (closed cognition, high affect), Resilient (closed cognition, low affect), Firm (open cognition, high affect), and Flexible (open cognition, low affect). This typology reveals certain inherent weaknesses when he tries to apply it to the decline of ideology hypothesis.

Sartori argues that the requirement of the decline hypothesis would not be met by a decline in the emotive intensity of ideology, i.e., by a passage from Adamant to Resilient. "A real decline of ideology is safely indicated" by a trend toward relative openness in the cognitive dimension, i.e., by a passage from Adamant to Firm (p. 406). A literal *end* of ideology, he adds, would require the complete transformation of ideology, i.e., a passage from Adamant to Flexible.

In making this argument Sartori introduces an unnecessary disjunction between the cognitive and emotive dimensions, for, although analytically distinct, the two are in fact closely interrelated. Thus, for example, if the emotive intensity of a belief system is high, that belief system is almost by definition "closed." Similarly, if a belief system is "closed," it is probably because it is held with high emotive intensity.

This suggests that cognition and affect may be inadequate or insufficient criteria for a typology of ideologies. The objective or goal of ideology, for example, may provide a more relevant dimension—a dimension, in fact, that would yield a threefold typology of ideologies as "instrumental," "expressive," and "consummatory." In this context, a decline of ideology hypothesis might refer to any one of three propositions: (1) the transformation of a consummatory belief system into an expressive one, (2) the transformation of a consummatory belief system into an instrumental one, or (3) the transformation of an expressive belief system into an instrumental one. (The idea of an "end of ideology," by the way, is simply a euphemism. No one has suggested a literal ending of all ideologies, but an ending of "apocalyptic" or "extremist" ideologies—that is to say, a decline of ideology.)

2. Professor Sartori's "major and most comprehensive point" (p. 410) is that, since mass belief systems appear to be dependent on elite belief systems, the elite publics are in a position to mobilize and manipulate mass publics. He states: "ideologies are the crucial lever at the disposal of elite for obtaining mass mobilization and for maximizing the possibilities of mass manipulation. That is . . . the single major reason that ideology is so important to us" (p. 411).

Insofar as the mobilization-manipulation potentials of ideology have been insisted upon by most writers on this subject since Karl Marx, one cannot help wonder whether Sartori's logical operations, though admittedly impressive, were necessary for reaching this conclusion. And having reached this conclusion, has one used ideology to "explain" politics?

Mostafa Rejai
Miami University, Ohio

To the Editor:

Professor Mostafa Rejai's comment is a welcome opportunity for clarifying my views. He takes issue with me on three points.

1) The disjunction between the cognitive and emotive dimensions of ideology is unnecessary: "although analytically distinct, the two are in fact closely interrelated." However, "interrelation" does not imply that a distinction is "unnecessary": the premise does not warrant the conclusion. The point simply is, therefore, that Professor Rejai takes the view that the emotive and cognitive dimensions vary together. As he puts it, "if the emotive intensity of a belief system is high, that belief system is almost by definition closed. Similarly, if a belief system is closed it is probably because it is held with high emotive intensity." If this were the case, then my distinction would be unnecessary. But is this the case? I very much doubt it. Indeed my experience with marxists and clericals (with which my country is well supplied) is that no matter how feeble the *passion* may become, the *mentality* remains constant. So the point simply is that Rejai takes for granted the covariance that I challenge.

2) Professor Rejai goes on to suggest that the "objective or goal of ideology" may provide better criteria for a typology of ideologies than the criteria suggested in my article. This may

well be the case, even though I happen to be very skeptical about the typological validity of the distinction between instrumental, expressive and consummatory ideologies. In principle, however, there is no reason why two criteria—one of these being teleological—would necessarily turn out to be mutually exclusive rather than complementary and additive.

3) Professor Rejai's concluding point is that "Insofar as the mobilization-manipulation potentials of ideology have been insisted upon by most writers . . . since Marx, one cannot help wonder whether Sartori's logical operations . . . were necessary. . . . And, having reached this conclusion, has one used ideology to explain politics?" Quite frankly I do not see why the last question should be addressed to me, for throughout my article I asked whether "ideology" still has a scientific use, and my conclusion was that the concept retains an explanatory value only if used far less cosmically and ambitiously than we tend to do. And I feel that the other points touched in the above quoted paragraph equally imply a misreading of my text.

The suggestion conveyed by Professor Rejai is that my logical operations break through an open door. We must be knocking, however, at very different doors. An illustrative, if cursory, selection of the current conceptualizations of ideology reads as follows: "an organization of opinions, attitudes and values—a way of thinking about man and society" (T. W. Adorno, 1950); "the systematic and explicit formulation of a general orientation to politics" (G. Almond, 1956); "A verbal image of the good society and of the chief means of constructing such a society" (A. Downs, 1957); "an application of particular moral prescriptions to collectivities" (D. Apter, 1964). The most recent work I have read on ideology is Nigel Harris, *Beliefs in Society—The Problem of Ideology*, where it takes the author 266 pages of debunking to reach this thrilling insight: "Ideology can mean both the views of any and all social groups, and the views of social groups other than my own." Whether my article lacks originality or not, the fact remains that it does run counter to the prevailing mood of the discipline.

Professor Rejai links my argument with Marx. This linkage appears to miss my major point, which was to separate ideology conceived as a doctrine (and an epistemological issue) from ideology conceived as a perception of politics (the ideological mentality), and to show that the argument can be neatly pursued from beginning to end in a contentless fashion, with regard to *how* one believes, regardless of content, i.e., regardless of *what* one believes. If anything, therefore, my insistence has been upon disentangling what Marx entangled for himself and posterity.

GIOVANNI SARTORI
Visiting Professor, Yale University

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REVIEW SYMPOSIUM

Report of the National Advisory Commission on Civil Disorders (New York: Bantam Books, 1968); and *Supplemental Studies for the National Advisory Commission on Civil Disorders* (New York: Frederick A. Praeger, 1968): (Angus Campbell and Howard Schuman, "Racial Attitudes in Fifteen American Cities;" Peter Rossi, Richard A. Beck, David P. Boesel, Bettye K. Eidson, and W. Eugene Groves, "Between White and Black—The Faces of American Institutions in the Ghetto;" and Robert M. Fogelson and Robert B. Hill, "Who Riots? A Study of Participation in the 1967 Riots.")

Aside from Gary Marx and a few other scholars, few Americans have subjected the Kerner Commission report to careful scrutiny. Nor, in all probability, will many others do so in the foreseeable future. The widespread protest against the Vietnam War has diverted social criticism from domestic policies to foreign affairs and military appropriations. The intermittent outbreaks of black terrorism have channeled official concern from long-term questions of reform to short-term problems of repression. And the recurrent uprisings at the nation's universities have distracted many Americans from the issues raised by the 1960's riots. This situation, though understandable, is most unfortunate. For the report, while not, as the New York *Times* observed, "a major turning point in the history of the nation," epitomized the liberal response to the riots and embodied the liberal approach to the ghettos. Better than any other report, better than the report on the Newark riots and better by far than the report on the Los Angeles riots, it posed the question whether liberalism has the capacity to resolve the racial crisis. And if for no other reason than to consider this question an evaluation of the Kerner Commission report is in order.

It was fairly easy for the commission to choose between a liberal and a conservative interpretation. Most of the commissioners (including the chairman, vice-chairman, and most of the more influential and knowledgeable commissioners) were, broadly defined, liberals. So were most of the staff and the consultants. What is more, virtually everyone on the commission knew (or, after a while, learned) that the basic assumptions of a conservative interpretation, namely the outside agitator theory and the riffraff theory, were already partially discredited. The outside agitator theory, which blamed the rioting on irresponsible extremists and, in particular, left-wing radicals and black militants, was sharply at odds with the reports of the F.B.I., C.I.A., and other investigative agencies and the findings of the commission's own field terms. The riffraff theory, which traced the rioting to a small and unrepresentative minority of unemployed, ill-educated, and uprooted Negroes, was incompatible with survey research reported by social scientists and arrest statistics compiled by police and correctional authorities. And the commission, to its credit, sponsored the studies which further discredited these misleading assumptions.

But it was much harder for the commission to devise a consistent liberal interpretation. For, as Allan Silver has shrewdly pointed out, the commission was caught in the same bind as the Chicago Commission on Race Relations, the Mayor's Commission on Conditions in Harlem, and other liberal riot commissions. Although the commission was committed to racial equality, it was also committed to public order; and though it was appalled by ghetto conditions, it was also appalled by violent uprisings. From the very outset, then, its overriding objective was to reconcile these conflicting convictions—or, in other words, to retain sympathy for the rioters without conferring legitimacy on the riots. To this end the commission, much like other riot commissions, adopted the perspective of what Professor Silver aptly calls "diagnostic sociology," a perspective from which collective violence appears the inevitable response, as opposed to the deliberate choice of an oppressed group.[1] By so doing the commission viewed the riots as reactions rather than protests, the rioters as victims rather than demonstrators, and racial unrest as a manifestation of problems which can be resolved within the existing institutional structure.

Given this perspective, the commission's interpretations fall neatly into place. At the core of the riots, the commission argued, are racial discrimination, economic deprivation, family disorganization, personal insecurity, consumer exploitation, residential segregation, and the Negroes' other grievances. Combined with the black migration from the south and the white exodus from the city, these conditions

[1] Allan A. Silver, "Official Interpretations of Racial Riots," in Robert H. Connery (ed.), *Urban Riots: Violence and Social Change*, Proceedings of the Academy of Political Science, XXIX, 1 (July, 1968), 149–154.

have created a situation of exceptional degradation in the black ghetto and unprecedented disorder in the entire society. Underlying this situation, the commission insisted, is white racism. Also, by virtue of inadequate communication in the cities, ineffective organization of local government, and inequitable representation of the Negroes, many blacks find it hard to redress grievances in legitimate ways. And by virtue of the rising expectations of most Negroes, the increasing legitimacy of violent action, and the growing influence of black militants, many blacks tend to regard violence as feasible alternative. Under these circumstances a minor incident or a series of minor incidents, often involving the police or other visible symbols of white authority, can easily trigger full-scale rioting in the black ghettos.

This was a formidable interpretation of the riots. It was much more sophisticated than the McCone Commission's conception of the Los Angeles riots and the Cuyahoga County Grand Jury's conception of the Cleveland riots, two well known documents with a conservative viewpoint. And it was far more elaborate than the conception of the Governor's Commission on the Newark riots, the Chicago Riot Study Committee, and the other official commissions with a liberal perspective.[2] This was a plausible interpretation of the riots too. The findings were not sharply inconsistent with available information on the ghettos: by any criteria unemployment rates are excessive, educational practices are defective, welfare regulations are humiliating, and housing facilities are dreadful. Nor were the conclusions inconsistent with the available information on the riots: the Negroes' grievances and frustrations are closely related to the recent violence, and white racism is inextricably connected with ghetto conditions. Given the sensitivity of the topic, the prominence of the commissioners, and the impatience of the public, this was no mean accomplishment.

But it is one thing to be plausible and another to be credible, one thing to offer an explanation and another to prove it. And here, as Gary Marx has observed, the report left a good deal to be desired.[3] By classifying the riots simply as "major," "serious," and "minor," the report overlooked the specific differences and general similarities which might have revealed the critical patterns. By separating the accounts of the riots from the analysis of the ghettos, the report failed to described how the Negro's grievances and frustrations combined to precipitate the violence. And by applying its main concepts too broadly, the report neglected to document (though ample documentation was available) precisely how white racism has engendered black grievances and frustration. This critique could be extended to the report's desultory and sometimes misinformed surveys of black history and racial protest and to its mistaken impression of the riot's impact on the black ghettos and white suburbs. Rather than do so, it is enough to point out that these criticisms (which probably reflect the commission's tight schedule and deserve the scholar's judicious sympathy) do not prove that the report was wrong. Only that its findings cannot always be taken at face value.

Other criticisms are more damaging. Consider the report's approach to the Negro's grievances. In line with its conviction that at the core of the rioting are racial discrimination and many other grievances, the commission attempted to discern and measure black discontent. To this end it analyzed twelve hundred interviews conducted soon after the disorders (and sponsored two public opinion surveys in nineteen communities, which were finished afterwards). What is objectionable about this approach is not the technique but rather the presumption that the grievances could be found just by interviewing the blacks. Had the commission looked more closely at the riots, and particularly the violent acts and precise targets, it would have uncovered rather different grievances with rather different intensities. And these findings might have persuaded the commission to reconsider its position that ghetto grievances were a reflection less of misguided policy than of widespread apathy. To do so, however, the commission would have been obliged to acknowledge a direct relationship between violence and grievances. And though there is much evidence to support this position, the commission could not subscribe to it without regarding the riots as protests and the rioters as demonstrators. Which, in view of its liberal perspective, it could not do.

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[2] Governor's Commission on the Los Angeles Riots, *Violence in the City—an End or a Beginning* (Los Angeles, 1965). Cuyahoga County Grand Jury, "Special Grand Jury Report Relating to the Hough Riots" (August 9, 1966); Governor's Select Commission on Divil Disorder, *Report for Action* (February, 1968); *Report of the Chicago Riot Study Committee to the Hon. Richard J. Daley* (Chicago, 1968).

[3] Gary Marx, "Two Cheers for the Riot Commission Report," *The Harvard Review*, IV, 3 (Second Quarter, 1968), 3–14.

Consider next the report's treatment of the Negro's frustration. In line with its conviction that many blacks find it hard to redress grievances in legitimate ways, the commission attempted to locate the source of the blacks' powerlessness. To this end it reviewed the hearings, field reports, and academic papers, examined recent histories of the stricken cities, and surveyed black attitudes toward local government. And from these studies it concluded that the Negro's frustration is essentially a function of inadequate communication, ineffective organization, and inequitable representation. What is objectionable about this treatment is not so much the conclusion as the implication that these barriers are accidental or technical. Had the commission studied the evidence more carefully, it would have learned that these barriers are intentional and political. They are employed by individuals and organizations, including ethnic minorities and municipal bureaucracies, to prevent blacks from influencing decisions. But the commission could not accept this idea without admitting the possibility that the racial problem cannot be resolved within the existing institutional structure. Which, in view of its underlying assumptions, it could not do.

Consider also the report's concept of white racism. Although the commission shied away from a precise definition, it applied this concept in two distinctive ways: first, as if all whites, regardless of age, class, or region, are racist, and racist to the same degree too; second, as if individual prejudice rather than institutional rigidity is the major reason for racial inequality in urban America. According to recent studies of prejudice and discrimination, the first application is plainly wrong. It is also misleading: for as Hannah Arendt has recently remarked in the *New York Review of Books*, "Where all are guilty, no one is; confessions of collective guilt are always the best possible safeguard against the discovery of the actual culprits."[4] The second application is misleading too. Not only is individual prejudice much on the wane and institutional rigidity much on the rise nowadays, but, even more important, rigid institutions have a more pervasive and deleterious effect on the everyday life of the ordinary black today than do prejudiced individuals. But the commission could not incorporate these objections without seriously undermining the credibility of its interpretations.

These criticisms raise doubts about the validity of the recommendations too. For the commission drew from its interpretations the conclusion that American society could not prevent future rioting by resorting to outright repression or abject capitulation. Nor by pursuing present policies, which would perpetuate subordination and segregation and probably culminate in black rioting, white retaliation, and eventually a police state. And not by yielding to separatist pressures to enrich the ghetto and abandon integration, which would culminate in affluent suburbs, impoverished ghettos, and permanent inferiority for Negroes. Rather the commission insisted that American society could prevent future rioting only by adopting a forthright policy to enrich the ghetto and at the same time encourage integration. To this end the commission recommended large-scale and long-term private and public programs to alleviate ghetto grievances, reduce Negro frustration, and eliminate white racism. Only by implementing these programs, the commission warned, could American society halt the movement towards two societies and, in the report's resounding peroration, end the violence "not only in the streets of the ghetto but in the lives of people."

This was an admirable approach to racial unrest. Few responsible Americans would challenge the commission's conviction that American society cannot maintain public order by resorting to outright repression or by pursuing present policies. Nor would many responsible Americans challenge the commission's conclusion that American society must alleviate ghetto's grievances, reduce Negro frustrations, and eliminate white racism. This was an ambitious blueprint for public policy too. The commission took advantage of its special mandate to recommend policies not only to the states, the cities and their respective bureaucracies, which many riot commissions had already done, but also to the Congress, the President, and the American people. And whatever the immediate problem, whether employment, education, welfare, housing, or law enforcement, the report proposed not only broad national programs, which was what most public officials probably expected, but also specific legislative action. Given the tight schedule imposed on the commission, the sharp divisions among the commissioners, and the popular demand for law and order, this was no mean achievement.

But it is one thing to be admirable and another to be relevant, one thing to be ambitious and another to be feasible. And thus the recommendations must be subjected to more careful scrutiny. To alleviate the ghetto's

[4] Hannah Arendt, "Reflections on Violence," *The New York Review of Books*, XII, 4 (February 27, 1969), 28.

grievances, the commission recommended that the authorities increase employment by expanding training programs, consolidating recruitment activities, creating additional jobs, developing impoverished regions, and encouraging ghetto enterprise. Improve education by attracting first-rate teachers, developing new curricula, reducing class size, offering supplementary services, and reinforcing pre-school and college-preparatory programs. Reform welfare by setting minimal benefits, increasing the federal ogvernment's share, eliminating residency and other onerous requirements, and providing due process and other legal safeguards. Upgrade housing by granting low-interest loans to builders, enlarging rent-supplement funds, amending building codes, diversifying public housing, reorienting urban renewal, and expanding the model cities program. These recommendations, which epitomize the liberal approach to domestic problems, suggest how strongly the commission believed that the ghetto's grievances were a reflection of wide-spread apathy, which required new will not new porgrams.

But if the ghetto's grievances are also a reflection of misguided policy, these programs are of questionable value. In view of the costs of the Vietnam War, the demands of the defense establishment, and the conservative reaction in the country, it is highly unlikely that the authorities will pass the legislation or appropriate the funds. Even if the money is made available, these programs may increase employment, improve education, reform welfare, and upgrade housing a bit, though job training, head start, A.F.D.C., and public housing have not exactly been unqualified successes. They may even enlarge the welfare state enough so that, in terms of income, housing, health, and other vital services, the public sector will provide for depressed blacks what the private (or quasi-private) sector provides for affluent whites. But these programs will not affect the inequitable distribution of wealth which underlies the ghetto's grievances; this requires not minor changes in existing programs nor modest extensions of welfare capitalism but drastic changes in income distribution. And by failing to include an effective mechanism for redistribution, these programs will probably not alleviate the ghetto's grievances.

The commission realized that in view of the likelihood that ghetto grievances will still be a severe problem in the immediate future other recommendations had to be devised to reduce the Negro's frustration. And to this end it recommended that the authorities open channels of communication by organizing Neighborhood Action Task Forces, joint community-government agencies which would discuss and redress neighborhood grievances. Enhance governmental efficiency by establishing Neighborhood City Halls, which would institutionalize the task forces, and Multi-Service Centers, which would expedite the delivery of health, welfare, legal assistance, and other municipal services. Increase Negro representation by appointing blacks to high-level policy posts, employing them at all levels of local government, curtailing at large representation on city councils, and encouraging participation by representative community organizations. These recommendations, which were probably modeled on the Lindsay administration's response to black militancy in New York City, reveal how strongly the commission believed that the Negro's frustration was essentially a technical problem. And one which did not require radical institutional change.

But if the Negro's frustration is essentially a political problem, the commission's recommendations are beside the point. Neighborhood task forces and city halls may give blacks a greater sense of participation, and multi-service centers may provide them a more convenient arrangement for acquiring public services. Black civil servants and community organizations may also wrest occasional concessions from mayors and other officeholders whose political futures are closely tied to public order in the ghetto. But these programs will not end the bureaucratic irresponsibility which underlies the Negro's frustration. This requires not only adequate communication, efficient organization, and equitable representation but also a redistribution of political power, which is underway in many cities, and a transformation of urban institutions, which is meeting furious resistance in the same cities. About these issues the commission said virtually nothing. Thus by neglecting to include an effective mechanism for participation, its programs will not reduce the Negroes' frustration; and by raising their expectations, they may even increase it.

The commission also realized that these programs will not eliminate white racism, that, if anything, white racism might eliminate these programs. Thus it also recommended that the authorities remove racial barriers to employment by extending federal anti-discrimination legislation to government agencies, granting the F.E.P.C. strong cease and desist powers, and supplying the E.E.O.C. additional staff and resources. Curtail de facto school segregation by providing financial bonuses to inte-

grated school systems, creating exemplary schools and educational parks, opposing racial discrimination by school boards, and, if necessary, cutting off federal funds to segregated school districts. Abolish racial restrictions on housing by enacting a national, comprehensive, and enforceable open occupancy law for private (and not only federally-assisted private) housing and, by financial inducements, rent supplements, and direct construction, enlarge the stock of low-income housing in the suburbs. These recommendations, which extend the civil rights legislation of the Kennedy and Johnson administrations, suggest how strongly the commission believed that the major reason for racial inequality was individual prejudice, which could be effectively controlled by federal anti-discrimination legislation.

But if the major reason for racial inequality is institutional rigidity, these recommendations are wide of the mark. Fair employment practices may remove certain barriers to employment, but, if the E.E.O.C. reports are a reliable indicator, they will not guarantee equal opportunity to Negroes. Legislative action may prompt many schemes to eliminate de facto school segregation, but, in the face of bureaucratic procrastination, it will not generate many integrated schools. Open occupancy laws may inconvenience realtors and property owners, but, if recent studies are an accurate guide they will not inhibit residential segregation. To put it bluntly, federal anti-discrimination legislation may benefit some Negroes, and especially middle-class Negroes, but, no matter how strong and thorough, it will not end racial inequality. For, if individual prejudice once made the Negroes inferior, institutional rigidity, notably in the police, schools, welfare, housing, and other institutions, now keeps them so. And not only because of racial antipathy, a serious problem to be sure, but also because of the bureaucratization, professionalization, and centralization of these institutions. About which the report made no recommendations.

These criticisms not only undermine confidence in the report; they also raise the possibility that by holding to its liberal assumptions the commission might have overlooked more relevant and feasible approaches to the ghetto's problems. Doubtless the black militants thought so. They regarded the disorders not as riots but as rebellions and racial unrest as a manifestation of problems which cannot be resolved within the existing institutional structure—a position the commission casually dismissed. Now the militants' interpretations are probably not worth extended analysis: a thorough examination of the riots, and especially the violent acts and precise targets, leaves little doubt that they were protests, even highly articulate protests, but not rebellions. But the militants' recommendations are another matter entirely: a careful examination of the commission's recommendations leaves a good deal of doubt that the ghetto's problems can be resolved within the existing institutional structure. And if only to determine whether the commission overlooked a more promising approach an evaluation of black power is in order.

According to the commission, black power is an outgrowth of two closely-related developments. First, the failure of civil rights legislation and non-violent direct action to overcome segreation in the rural South and subordination in the urban North. And second, the resurgence of racial pride among blacks of all regions and particularly among the upcoming generation of blacks in the northern ghettos. The commission conceded that black power does more than simply reflect the Negroes' growing alienation from white America. It also expresses the increasing pressure for black autonomy, ranging from economic self-sufficiency and independent political action to retaliatory violence, and underlies the demand for community control of schools, police, welfare, housing, and other institutions. But the commission also contended that black power is nothing but a modern version of the nationalist (or separatist), as opposed to the assimilationist (or integrationist), tradition of Negro protest. An old program encased in a new rhetoric—or, as the report put it, "old wine in new bottles"—black power is a revival of the "separate but equal" notion, a retreat from racial integration, a victory for white racism, and a defeat for black equality.

Much can be said for the commission's indictment. Black power does belong to the nationalist tradition of Negro protest which extends beyond Elijah Muhammed, Marcus Garvey, and Booker T. Washington well into the ante-bellum period. It emerges out of the disintegration of the civil rights movement of the 1950s and 1960s much like the Atlanta Compromise developed out of the breakdown of the civil war reconstruction in the 1870s and 1880s. Its ideology of racial solidarity, self-reliance, and autonomy is a variation on traditional nationalist themes; and so is its program of economic self-sufficiency, independent political action, and retaliatory violence. Black power also reflects the revival of the "separate but equal" notion. It expresses an increasing disparity between expectations and achievements and a growing frustration with the as-

similationist approach to ghetto problems. It regards racial integration as undesirable and, in view of white attitude, impossible; and it considers white racism, individual or institutional, ineradicable. And given the Negro's economic and political position, it is possible that black power will, as the commission argued, rationalize segregation and subordination in urban America.

But more can be said against the commission's indictment. The militants' conviction that integration and equality are not the same thing is not unreasonable; nor is its belief that at this moment the concern for integration may set back the struggle for equality. It is open to question whether, as the commission contended, integration is a prerequisite for equality. The militants' contention that the community control of ghetto institutions (and not an extension of welfare capitalism or an appeal to white conscience) is the only relevant program for equality is defensible too. It may well be that, as the militants contend, participation is a prerequisite for redistribution. From this prespective the militants' demand for community control is fundamentally different from the Garveyites' quest for an independent African nation and the Black Muslims' demand for a separate American state. It is more realistic and yet more radical; it is, above all, more political. So much so that, notwithstanding the militants' revolutionary rhetoric and military posture, there is little doubt that with the emergence of community control black nationalism has come of age politically. And if, as the commission suggested, this is "old wine," it is heady stuff.

So, too, are its implications. Community control rejects the Kerner Commission's distinction between grievances and frustration; it insists that they are inextricably connected and that, if anything, grievances are a function of frustration. It argues that integration is irrelevant and, even more important, that meaningful participation is a prerequisite for effective redistribution. Community control also rejects the commission's commitment to the existing institutional structure; it demands a thorough overhaul of welfare capitalism (and particularly the institutions which administer it). It insists that the buerauctratization, professionalization, and centralization of urban institutions be halted, if not reversed. Hence community control repudiates the progressive view of local government which has prevailed since the turn of the century and the liberal notion of federal authority which has prevailed since the start of the New Deal. And it calls instead for a return to public authority which emphasizes political responsibility, citizen participation, and administrative decentralization.

Now community control is not as promising an approach to ghetto problems as the black militants argue. As a device for redistribution, it may be feasible, but it is probably not relevant (or at any rate not without radical changes in the American economy which are inconceivable in the foreseeable future). And as a device for participation, it may be relevant, but it is probably not feasible (or at any rate not until the blacks gain control of the cities, which will not happen for at least another generation). But community control is not as trivial an approach to ghetto problems as the Kerner Commission concluded. It emphasizes the connections between institutional rigidity and racial injustice and thereby serves as a valuable corrective to the commission's focus on individual prejudice. It also questions the conventional wisdom about the relationships between integration and equality and redistribution and participation. And for these reasons alone the Kerner Commission should have given community control (and black power) a more extensive hearing and more thorough evaluation.

But the commission could not do so without reconsidering its fundamental principles, namely, the integrationist approach to race relations, the progressive view of local government, the liberal version of welfare capitalism, and the traditional commitment to the existing institutional structure. It could not do so without contemplating whether integration is the prerequisite for equality or, as the militants contend, integration is irrelevant to equality (and, if anything, equality is a prerequisite for integration). Whether the bureaucratization, professionalization, and centralization of municipal institutions has enhanced social justice or, as the militants argue, perpetuated racial subordination and segregation. Whether distribution and participation are two different issues or, as the militants claim, equitable distribution is inconceivable without effective participation. And whether the racial troubles can be resolved within the existing institutional structure or, as the militants declare, community control is the only relevant and feasible approach to ghetto problems. In other words, the commission could not give community control (and black power) due consideration without inquiring into the conventional wisdom of American liberalism.

All things considered, the Kerner Commission did a good, but not splendid, job. As I have observed, its approach to ghetto grievances, its treatment of Negro frustration, and

its concept of white racism leave a good deal to be desired. So do its recommendations to extend welfare capitalism, increase citizen participation, and restrain individual prejudice without changing the existing institutional structure. Its casual dismissal of community control and black power, not to speak of more radical proposals for social change, is particularly disappointing. But, in view of the public opinion polls, the Kerner Commission did a better job than the country deserves. It offered a comprehensive and plausible interpretation of the riots when it could have explained them away as the product of outside agitators and irresponsible riffraff. It also offered elaborate and reasonable recommendations for the ghettos when it would have written them off with vague phrases about private enterprise and local initiative. And had the commission abandoned its liberal perspective and submitted a more original interpretation and more radical recommendations, it would probably have been rejected outright by most Americans.

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It seems fairly clear, even now, that in the mid 1960's we entered a new era in race relations in the United States. The nonviolent marches and passive civil disobedience of earlier years have, during the past five years, given way to newer, and sometimes more violent, forms of black militancy, and the coalition politics of the Civil Rights Movement is now forced to share the public stage with the Black Power Movement and black separatism. Although the Civil Rights Movement is far from dead, it has lost ground with the emergence of a new "pluralism" in the black community, a "pluralism" that ranges from those who would pursue social changes through traditional legal and institutional reform to those who have abandoned the political system altogether and who would force change through violent attacks on the existing institutions.

As everyone knows, the National Advisory Commission on Civil Disorders (generally referred to as the Kerner Commission after its Chairman, Otto Kerner) was constituted by President Johnson with the mandate that the Commission should explore the causes of the riots and make recommendations for dealing with the problem of racial discontent. Unhappily for many people, the Commission did just that and the result was a document that was surprisingly strong in its condemnation of white America. The Commission suggested that we didn't have simply a "black problem," but that the real villain was the "white problem" of racism, both individual and institutional. At the root of the "white problem" lay a barrier of white attitudes, values, and practices that has, for three hundred years, successfully prevented most black Americans from sharing the benefits of freedom and prosperity.

Two of the three supplemental studies in this *latest publication* for the Kerner Commission are concerned with the attitudes of both the black and the white citizens of fifteen large cities. The first study, by Campbell and Schuman of the Survey Research Center at the University of Michigan, is based on 5,759 interviews with the residents of the fifteen cities and two suburbs. The second study, by Rossi, *et al.*, of the Department of Social Relations, Johns Hopkins University, is based on 1,953 interviews with individuals in six occupational categories that have regular contact with Ghetto residents in these fifteen cities. The occupational categories included social workers, primary and secondary school teachers, police, retail merchants, political workers (all of them black), and the personnel managers for large firms in the riot cities. The third study, by Fogelson and Hill, is not concerned with attitudes, but it focuses on the social, economic, and other characteristics of the rioters.

It should be stressed at the outset, as all of the authors emphasize, that these reports are preliminary in character; they represent only a brief overview of the data collected and they contain only a few relevant cross tabulations beyond the simple "marginal" results. In this form, the reports tend to be descriptive and speculative in tone, and the authors justifiably end up asking more questions than they answer. Nevertheless the reports contain a great body of information, and some of the results, even in a generally under-analyzed form, are of considerable importance. My comments, however, will have to be limited to a few high points because there is far too much in these reports to be covered in a few paragraphs.

One of the more important findings is the extent to which the white residents of these cities underestimate the amount of discrimination in their cities. Nearly 40% of the white respondents believe that "only a few" or "none" of the Negroes in their communities "miss out on jobs and promotions because of racial discrimination," and about a quarter of the respondents believe that "only a few" or "none" of the Negroes "miss out on good housing" for the same reason. More importantly, over half of the respondents believe that the inferior social and economic conditions of Negroes are due "mainly to the

Negroes themselves." At the same time, police mistreatment, which is one of the main complaints of blacks, is thought not to occur by a majority of the white residents.

If white recognition of the racial dilemma of black Americans is a precondition for eliminating poverty and discrimination, these results are not very encouraging. There are, however, two additional pieces of information which modify these results somewhat. One is that evaluations of the "black condition" are rather strongly related to the age of the respondents with the younger respondents more inclined to appreciate the discrimination that confronts Negroes. The second consideration is that a majority of the respondents are willing to see the government assume an active role in improving the conditions for blacks. This support for governmental action was strong enough that a majority of the respondents were willing to see their taxes increased 10% in order to fund the necessary programs.

Although one can overestimate the support for governmental intervention among whites, the findings by Campbell and Schuman, when seen in the context of the numerous other studies of white attitudes, provide some basis for a sense of cautious optimism. White resistance to black advancement has decreased over the past two decades, and the more sympathetic views held by young people may indicate that an even more dramatic reversal in the distribution of attitudes is approaching. In addition, virtually every study of white attitudes indicates that the least resistance is found among the more highly educated, and this segment of our society is increasing in size.

At the same time, however, one of the main points of irritation between the black community and the larger society is still with us, and that is the relationship between the black community and the police. A large segment of the black community expresses very great discontent with police practices in the cities. A majority, for example, believe that the police don't come quickly when called (27% for whites), nearly 4 out of 10 believe that the police are disrespectful and insulting (16% for whites), and over a third believe that the police frisk and search without reason (11% for whites).

And parallel to these findings, Rossi, et al., report data that suggest that the police are the least sympathetic to blacks of the six occupational groups that they interviewed. Police, for example, are about three times as likely as educators and social workers to believe that Negroes are "as well off" or "better off" than whites with respect to employment, housing, and police treatment, and they are about twice as likely to believe the same with regard to education, the treatment of public officials, and recreation. In addition, over 60% of the police believe that "Negro gains toward equality" are either "much too fast" or "too fast." Moreover, they are the only group in which a majority disagrees with the findings of the Kerner Commission, perhaps because of the criticisms voiced in the Commission's report of police practices.

These findings, when coupled with the Commission's report that nearly every riot began with a "police incident," suggest that the composition of urban police forces is a critical problem for race relations. This is particularly apparent when the attitudes of white policemen are compared with those of black policemen. Although black policemen may be considered "Uncle Toms" by many black militants, these data show them to be much more sympathetic to the problems of their race than are their white counterparts. In addition, they report a greater acquaintance with the people of the ghetto, and they are far more likely to have relatives, friends, or themselves live in the neighborhoods where they work. The overall import of these results seems to be that the white policeman is, in many respects, the alien agent of the "white power structure" that the Negro tends to believe he is. His black counterpart, on the other hand, fits more an older model of the policeman as the agent of the neighborhood from which he comes. The policeman is one of the principal agents of the society with whom neighborhood residents have contact, and the probability of continued exacerbation is this contact seems very high. For that reason the logic behind the black demand for "community control" of the police seems very understandable.

A third important aspect of these studies is information they provide on the "causes" of the urban riots. The findings almost universally portray a black community that is extremely discontented with the conditions in which they live, discontented with the institutions that exclude them, and discontented with the primary agents of the white society with whom they are in contact. Although only a minority indicate a willingness to directly participate in the riots themselves, a majority are sympathetic with those who do join the riots and a sizeable minority of the young indicate a willingness to use violence to obtain social justice. Under these conditions, the direct "cause" of a riot is somewhat less important than the general set of conditions upon which an "incident" feeds.

The alienation of the majority of urban

blacks, whether due to their "rising expectations," relative deprivation, or the restriction of their social and economic possibilities, creates a condition where the normal restraints of social life are removed. Although many blacks may not be willing to join a riot, they are also clearly not willing to condemn those who do. It has always been the case that social order rests on the condition that most people do not have to be restrained; in fact, the most general and effective control over disruption is probably the informal sanctions exerted by those who stand to lose because of disruption. What white racism has fostered in the black community is the breakdown of this normal sanctioning process. This is coupled with the condition that the riots may be beneficial to a sizeable minority in the community, a minority that includes those for whom the riot represents an opportunity to express anger and hostility, those who can obtain desired goods through looting, and those who can use the riots for political gain.

The continued existence of these conditions, however, does not necessarily lead to the conclusion that more rioting is in prospect. Riots also exact a cost, and that cost may have been sufficient to deter blacks from following this course of action in the future. Although the studies discussed here do not give us any indication of how high these costs have been, it appears on the surface that the costs may outweigh the benefits such that the period of mass riots is passed. What is more ominous, however, is that the discontent does not appear to be decreasing, and the demise of the riot may foreshadow a turn towards even more militant forms of protest, particularly since it remains to be seen whether the militant demands for "community control" will be successful.

All this is in the way of conjecture, however, but it does suggest that we have a long way to go in our understanding of either the riots or how to deal effectively with a deprived minority. These studies, along with many aspects of the Kerner Commission Report, suggest that social scientists can be of considerable use in the search for understanding. These studies are all preliminary reports and are not specifically intended for an academic community, but, as the authors note, we can expect more detailed and scientifically interesting publications at a later date.

At the same time, these data and many of the preceding studies only skim the surface of the research that needs to be done. We have, for example, almost nothing on the development of attitudes over time in the area of race relations. For this, we need longitudinal data from panel studies of both blacks and whites. If we do not obtain this data, it will be very difficult to discern what experiences are associated with attitudinal modifications among blacks and whites. We also have little or no research on inter-racial bargaining and negotiation. If the drive for community control continues to develop within the black community, it would be useful to know what contributes to or detracts from a successful bargaining process among racial groups.

These concerns are secondary to the more paramount problem of bringing blacks into the mainstream of American economic life. The Kerner Commission proposes a vast number of programs with which to confront this problem, but they offer very little in the way of research or substantive knowledge that would permit us to choose between the various approaches to poverty and skill deprivation. The economic pie is, as we all know, limited, and if we have to choose between programs, it seems more appropriate to choose with insight, not ignorance.

These comments point to a concluding note that is perhaps the most severe criticism of the Supplemental Studies and of the Kerner Commission in general. These studies and the larger Commission report were a response to a severe "crisis" in the American political system. As such, they represent an admirable attempt to understand certain aspects of the intricate and complex problem of race relations in the United States. But the process of science and the process of understanding these phenomena requires a more systematic and orderly approach than is displayed in these efforts. A survey of over 7000 respondents represents a major investment in social research, and one would hope that such investments in the future are preceded by the development of theories, propositions, and hypotheses to be tested. We need, as in so many areas of social research, more theory development and proposition testing rather than simply more data.

Given the "crisis" atmosphere in which these studies were developed, however, the preliminary reports represent a surprisingly high quality of social research, but they also suggest a number of important questions about the whole research enterprise. Could we, for example, have learned more by extending the examination to a broader range of American cities? Could we have learned more by reducing the number of respondents while interviewing them at a later date? These comments are not intended simply as *post hoc* second guessing. The researchers had choices to make and they were operating under the constraint of pro-

viding information for policy makers. At the same time, "crisis research" would be less necessary if research support were available in this area on a more continuous basis.

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Reviewing the Supplemental Studies for the National Commission on Civil Disorders (Kerner Commission) provides an opportunity to examine some specific questions about the approach of riot commissions to their subjects, and their use of social science.* Our attention is first drawn to these considerations by the apologetic tone appropriately adopted in two of the three supplemental studies. This is suggestive of the time pressures under which the Kerner Commission operated. Campbell and Schuman provide an analysis that "is a very preliminary statement of the findings that will eventually flow from this study." For the most part they present their findings only by race and by sex, although in a few tables they provide age and educational distributions. They are painfully aware that they illuminate "very little . . . about the social locations of the basic attitudes with which the study is concerned." Similarly, Rossi and his colleagues in introducing their study, also stress that their "report contains [only] the first sounding of our data." It provides "an overall picture," although the findings presented here are not directed to questions which are of greatest interest to the researchers.

The prominence of such caveats is clear and understandable. Campbell and Schuman provided a report written within a few weeks of the end of data collection. Interviewers for the Rossi study were still in the field when Martin Luther King was assassinated in April, 1968. In this volume only the study by Robert Fogelson and Robert Hill, an effective critique of the "riff-raff" theory, appears complete for its purposes. Even here the potential richness of the study appears untapped.

Two general, related questions are raised by the hurried nature of the presentations. First, why did the Commission consider it essential to provide these research reports under such time pressures? Second, what is the relationship between social science inquiry and the writing of reports in the riot commission process?

A number of answers may be proposed to explain the premature publication of the Supplemental Studies. From interviews with key commission strategists, it appears that the commission staff took extremely seriously President Johnson's original charge. They also needed to "cover" the commission from potential criticism that it had not provided answers to questions that were asked of it by the President. Additionally, they recognized that they could not even begin to answer some of the questions authoritatively (if then), except through sophisticated contemporary methods of social science inquiry. Futher, they wanted to evoke the structure of social science investigation in order to: 1) demonstrate that the analysis was founded as much as possible upon "hard data"; 2) make it appear that "hard data" were employed even if they were not; and 3) deflect the potential criticism of academics and others who were lobbying for the Commission to deploy the arsenal of social science on the problems of riots.

Because the commission staff felt that their effort would be imcomplete without these investigations, and in order to deal with some of the problems of political legitimacy mentioned above, the Kerner Commission "farmed out" several key studies: an attitudinal survey of white and black attitudes; a more intensive, 15 city survey of policemen, teachers, and others who provide services to the ghetto; an analysis of riot arrest data; a content analysis of riot coverage in communications media; and survey research in Newark and Detroit ghettoes, among others.

Another insight into the Commission's concerns for developing political legitimacy may be obtained by asking why the Commission contracted for sophisticated outside research at a period when it could not possibly have made use of the findings in developing its analysis and recommendation. Analysis and recommendation are supposed to flow from commission research. But it is clear that this was not the case in the Kerner Commission's use of material presented in the Supplemental Studies and could never have been the case, given the time constraints under which the Commission operated. The studies presented here were commissioned so late that the findings were not available to inform the analysis. It would have been difficult to use these findings when the Commission still intended to produce both an interim report, around January 1, and a final

* These notes have been developed as part of the writer's continued collaboration with David J. Olson in a study of the politics of riot commissions in American cities. Our extensive research on the Kerner Commission, the New Jersey (Lilley) Commission, and post-riot politics in Newark, Detroit and Milwaukee has provided the underlying perspective for these comments. I am grateful to Murray Edelman for comments on the first draft.

report in July. The decision to produce only one report, in March, made these studies irrelevant for use in the final report. In some cases the data were not even collected until after the commission had reported. In other cases, such as the Simulmatics content analysis of television riot coverage, the Commission prepared its recommendations on communications media without the benefit of this research.

There is considerable justification for arguing that the Kerner Commission, given the time constraints under which it operated, acted appropriately in utilizing the best available research and consultants to write its report, concurrently commissioning studies to advance still further knowledge of selected factors affecting riots. At the same time, however, we should clearly acknowledge the extent to which the Kerner Commission Report was not based upon these studies, and should further inquire into the relative importance of empirical research in writing such reports.

Does it make a difference? Careful comparison of the Kerner Commission Report with the Supplemental Studies suggests that in some cases the Commission's report would not have been affected particularly by the research findings. This is the case with the study by Fogelson and Hill of riot arrest records. Although Fogelson and Hill effectively and pursuasively demolish the riff-raff theory, evidence in the Kerner Commission is consistent with, and supportive of, their analysis.[1]

In other cases, it seems that the Kerner Commission might have profited from the availability of some of its commissioned research. Not only would individual findings have been useful for the Kerner Commission deliberations, but some of this research might have directed the attention of the Commission to critical (and in the Report neglected) areas of investigation. It is instructive to compare some aspects of the Kerner Commission Report with the findings developed in the Supplemental Studies. While such comparisons are somewhat speculative, they are illuminating for investigating whether the Kerner Commission might have developed different conclusions, or pursued different directions in analysis, if it had had at hand its own commissioned research.

Take, for example, findings related to ghetto education. The Kerner Commission asserted that "school administrators, teachers, . . . regard ghetto schools as inferior." It concluded that this was "[a]nother strong influence on achievement." (K427) Rossi and his associates, however, as reported in the Supplemental Studies, found that ghetto teachers generally considered their schools to be average or better when compared to other schools in the city. Only in the area of adequacy of physical plant did the ghetto teachers in Rossi's survey approach agreement that their schools were inferior. (SS135)

Although much is made in the Kerner Commission of increasing the number of blacks employed in various occupational roles, the Kerner Commission fails to discuss racial differences in teacher attitudes. If, somehow, the Kerner Commission had been able to develop its analysis of education on the basis of research on teacher attitudes, it might have been compelled to discuss racial differences in teacher attitudes because of the discriminatory power of this variable. Rossi and his associates discovered that black teachers are less likely than white to conclude that their ghetto schools are adequate, but more likely to regard their black students as having potential for education. (SS137)

The disadvantages of not being able to write the Report with empirical studies at hand are again illustrated in the Kerner Report treatment of ghetto teacher attitudes toward schools. Apparently for want of pertinent research, the Kerner Commission cites a 1963 Chicago survey of public school teachers as evidence that "the condition creating the highest amount of dissatisfaction among teachers was lack of adequate provision for the treatment of maladjusted, retarded and disturbed pupils." (K429) This contrasts with the Rossi findings that, on the whole, ghetto teachers are relatively satisfied with their schools except over the adequacy of physical plant. (SS135)

Omission of teacher attitudes in the Kerner Commission analysis is also pointed up by examining Rossi's findings of ghetto teacher attitudes toward their pupils. The Kerner Commission explains negative teacher attitudes toward ghetto schools by a chain of development which starts when the least experienced teachers are assigned to disadvantaged schools. According to the Kerner Commission, teachers thus start out with negative attitudes towards students: "This process reinforces the view of ghetto schools as inferior." These attitudes are "aggravated" by: discipline problems; high crime rates in the surrounding areas; greater difficulty in teaching students from disadvantaged background; higher proportions of emotionally disturbed and other problem children

[1] See Fogelson's earlier statement, "White on Black: A Critique of the McCone Commission Report on the Los Angeles Riots," *Political Science Quarterly*, LXXXII (Sept., 1967), 217–47.

in schools because of neighborhood school districting; greater health problems; inadequate facilities relative to the greater needs for supplementary instruction and counseling; and serious mental and physical health problems that are found with greater frequency in poverty areas. (K429) High student turnover in disadvantaged areas is also mentioned as a cause of teacher dissatisfaction and difficulty. (K430)

Appropriately, the Kerner Commission takes note of the fact that negative teacher attitudes function as self-fulfilling prophecies wherein low teacher expectations result in low student achievement. (K429) But nowhere in the section on teachers in the Kerner Commission Report does the Commission comment on teacher attitudes toward students as students. Problems that teachers have in ghetto schools, listed above, are all "objectified" so as to relate to community characteristics independent of teacher attitudes toward ghetto kids.

Rossi and associates, in attempting to get at this consideration, developed some interesting findings, although the data may be somewhat ambiguous. They found, for example, that more than half of the teachers interviewed (56 percent) agreed that "many communities provide such a terrible environment for the pupils that education doesn't do much good in the end." Over 80 percent of the teacher repondents thought that parents tried to help children get a good education, but that too many other influences distract the pupils. Rossi and associates conclude that the educators surveyed adhere to a theory of "cultural deprivation" which "finds the reason for bad education primarily in the student's environment rather than in the schools." (SS136)

Similar comments may be made in comparing materials developed by Rossi and his associates in the Supplemental Studies on police behavior and attitudes, and the section in the Kerner Commission Report dealing with police practices. Generally speaking, the Kerner Commission fails to deal with the deeply rooted attitudes concerning Negroes and lawlessness which are revealed in the Rossi survey of police attitudes. While the Kerner Commission Report does cite the research of Albert Reiss concerning the high degree of prejudice toward Negroes expressed by police officers (K306),[2] the Commission does not deal with the implications of these findings. It is content to recommend that officers with poor reputations in ghetto areas be reassigned, that screenin procedures be developed to assure that superio officers be assigned to minority group areas and that incentives be developed to attrac outstanding officers to ghetto assignments.

Rossi found that police feel overwhelmingl that riots are caused by criminal elements an agitators, holding this view even more tha ghetto merchants (the other occupational grou which subscribed to such views in simila proportions). Police also believe, again in con trast to the views of other civil servants (e.g. teachers, social workers), that Negroes hav achieved equal treatment in the city and parit in terms of services. These views undoubtedl are reinforced by the fact that police hav virtually no personal acquaintances amon ghetto residents. (SS113) These findings sug gest that police overwhelmingly hold view which deny that social factors may cause civ disorders, deny the relationship of racism t Negro attitudes, and explain ghetto violence i terms of criminal agitation. Equally important these findings suggest that the police hold self-justifying theory of riot causation which i impermeable to falsification because rioters ar defined as criminal; hence, riots are caused b criminals, and it is unnecessary to searc further for causal explanations.

The Kerner Commission recognized tha there is "no easy solution to police communit relations." (K320) But in the Report the as sumption prevails that the police problem i amenable to solution. Perhaps the Kerner Com mission was unable to develop a discussion o these perceptions because of the time con straints under which it worked. But the Kerne Commission consistently omits discussion o the attitudes and feelings of various segment of the population in viewing various aspects o riot causation. Thus, it most blatently failed t discuss what the riots meant to the black pop ulation, although some such materials wer available. Thus the Commission failed to dea with questions of the problems of perceivin riots as protest. Thus the Commission faile to talk about the inferior and worsening qualit of police recruitment and training that Skolnic outlines in his recent report.[3] Thus at the sam time that the Commission was discrediting th riff-raff theory, police appear to be holding o to it quite firmly. (SS110–111) The Commissio does not deal with sources of continued suppor for various alternative interpretations of rio causation.

Allan Silver has written intriguingly tha

[2] See Albert Reiss, "Police Brutality—Answers to Key Questions," *Transaction* (July/August, 1968), 10–19.

[3] Jerome Skolnick, *The Politics of Protest* (Ne York: Ballantine, 1969), ch. 7.

riot commissions in the United States have generally adopted a perspective in which social causes and conditions are seen to underlie civil disorders to the exclusion of other factors. Silver argues that this perspective functions to permit the various publics of commission reports "to maintain broad political sympathy for the urban Negro 'underclass' and simultaneously come to terms with popular unruliness, riot and violence even though these things are especially disturbing to them."[4] This perspective also permits elites to avoid confronting the revolutionary challenge of civil disorders. By attributing disorders to remote social causes, riot commissions may avoid raising fundamental questions about the nature of the political system. The perspective is the opposite but functional equivalent of the riff-raff theory, which also serves to deflect attention away from the practical content of violent protest by attributing riots to criminals, the uneducated and the socially dislocated.

Having compared some of the Supplemental Studies with the Kerner Commission Report, this analysis may be extended by recognizing the function played by a social causal analysis in permitting the Commission, and those sympathetic to it, to demand changes and advocate radical reforms without calling names. The Kerner Commission viewed the political system as *insufficient*. Drastic program changes and additions are advocated. But just as Silver argues that the perspective often adopted by riot commissions tends to "objectify the causes of violence, to show their roots in environment and social structure, to depersonalize the connection between particular violent groups and the specific content of violent acts,"[5] we may argue that there is a depersonalization, and an objectification in the Kerner Commission's analysis *which permits the Commission to ignore purposive actions in creating preconditions for violence*. There is little or no discussion in the Kerner Commission Report of the ways in which institutions and bureaucracies are structured so as to sustain racism.

As suggested by our review of the Supplemental Studies, attitudes are not scrutinized individually. No one is to blame. In the Kerner Commission Report, police departments have great problems and have difficulty being effective; but policemen are under great stress, too, and their racist attitudes are said on the basis of one study not to affect their work behavior. (K306) Teachers are said to have many reasons why they think ghetto schools are inferior and their status is challenged by being assigned there; but teachers' attitudes toward children in the ghetto are not discussed. Labor union discrimination is barely mentioned in the Report. Politicians are not responsible for lack of progress, and so on. The problem is identified and solutions are proposed. But no one is responsible, and no one is blamed, or urged to act, as an individual, any differently.

This characteristic of the Report, as other illustrations presented here, suggests some insights into the operation of riot commissions. The objectivist and depersonalistic tone of the report may be attributable to the commission's concern for maximizing its political legitimacy (thereby trying to avoid divisions and conflicts among commissioners, some of whom are protective of certain institutions or values). It may be attributable to the desire not to arouse opposition among groups whose future cooperation would be required for implementation of recommendations, or it may be attributable to the genuine convictions of the commissioners. While we cannot finally provide authoritative answers in this note to questions of commission motivation, we can suggest that it would be useful to continue to raise questions about the uses to which government agencies put social scientists and the social sciences.

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[4] Allan Silver, "Official Interpretations of Racial Riots," in Robert Connery (ed.), *Urban Riots: Violence and Social Change*, Proceedings of the Academy of Political Science, XXIX, 1 (New York, 1968), 151.

[5] *Ibid.*, 150.

BOOK REVIEWS

The Government of Science. By Harvey Brooks. (Cambridge: The M.I.T. Press. 1968. Pp. 343. $10.00.)

Science and the Federal Patron. By Michael D. Reagan. (New York: Oxford University Press, 1969. Pp. 346. $7.50.)

It is only a few years since Don K. Price pointed out that science had become an American "establishment." Already, however, as Price has lately noted ("Purists and Politicians," *Science*, 163, Jan. 3, 1969), it has come under attack from a motley army of New Puritans aroused by what they see as an unholy alliance of science and the state. President Eisenhower (appropriately an admirer of Cromwell) sounded the tocsin with his farewell warning against "the scientific-technological elite" and "the military-industrial complex." Others have described the active orders of the establishment as a "new priesthood" (Ralph Lapp) wielding sinister influence in the "cardinal decisions" (C. P. Snow) and only too willing, "in the name of science" (H. L. Nieburg), to lend their talents to the cause of temporal power. Even the contemplative orders have been accused of corruption. They are said to tolerate every species of evil, from holy wars to social misery, in exchange for splendid cathedrals (housing accelerators and radio telescopes rather than altars and chantries)—from which presumably only the gargoyles are missing to comment once again upon human folly.

Dangerous as it is to stretch even the best of analogies too far, it is tempting to apply this one to the books under review. Harvey Brooks might be cast as a modern judicious Hooker, writing a twentieth century *Laws of Ecclesiastical Polity* in defense of establishment, convinced that it is the rational course to follow and that its abuses stem from human frailty and external maldirection. Michael Reagan could be described as a latter-day Leveller, less radical than the establishment's utopian-anarchist critics, agitating for a constructive reform to secure greater popular control under an "agreement of the people" laying down explicit guidelines.

At best, alas, the analogy is imperfect, since neither Brooks nor Reagan has undertaken a defense or critique of the establishment *in toto*. Both concern themselves almost exclusively with the science end of the "science and technology" spectrum—with basic and applied research, rather than with development, which accounts for over two-thirds of the federal budget for "R and D." Brooks does not doubt that *development* should be planned and controlled in accordance with social goals, but in the papers assembled in this volume he concentrates on the problems of science policy narrowly construed. Similarly, although Reagan occasionally touches on other and larger issues, he is mainly concerned in this book with the justifications, criteria, and administrative organization for the support of research and scientific education.

Brooks and Reagan point out that as of now decisions to allocate public funds for science are made in accordance with the pluralistic, decentralized structure and the pressure group style typical of American decision-making generally. Funding for research and training is supplied to support agency missions, to advance knowledge in certain defined areas of special interest (such as atomic energy, outer space and the diseases studied by the National Institutes of Health), to subsidize projects in the various disciplines, and to promote scientific education. The National Science Foundation, which was originally expected to develop overall policies, has actually functioned as the special patron of university science and as a "balance wheel" correcting for deficiencies in areas inadequately funded by other agencies. What planning and coordination there is has been the work of the Budget Bureau, the Office of Science and Technology, and the interagency Federal Council for Science and Technology. These agencies have drawn upon data collected by the NSF, recommendations by the task-force panels of the President's Science Advisory Committee, and studies of needs and opportunities in the disciplines by the Committee on Science and Public Policy (COSPUP) of the National Academy of Sciences. Congressional oversight is performed by a diffuse and overlapping network of committees and subcommittees.

Reagan argues that under the present arrangement, support for science is in continual jeopardy because there is no clear appreciation of why and how it should be provided. He would prefer to see national objectives in science spelled out and the machinery of coordination and review overhauled so that these objectives could be pursued according to some rational plan. Congress could begin by enacting a law similar to the Employment Act of

1946, setting forth overall aims. Although no fixed percentage of the federal budget or of G. N. P. should be prescribed as the "science budget," it would be advisable to establish priorities. First priority, in Reagan's admittedly personal opinion, should be attached to projects serving major social goals; second, to science-related educational needs; third, to undirected small-scale research ("little science"); and last, to large and costly projects ("big science").

Coordination would be made the responsibility of an enlarged OST to be paralleled by a single Joint Congressional Committee for Science and Technology. Like Brooks, Reagan doubts the advisability of a Department of Science, among other reasons because too many of the activities it would have to house would be incompatible with each other and because some are better located in the mission-oriented agencies they are designed to serve. Instead, he suggests that it might be a better idea to associate the NSF, the NIH, the National Foundation for the Arts and Humanities, the Office of Education, and the proposed National Foundation for the Social Sciences, perhaps in a Department of Research and Higher Education. Interfield comparisons could be made by COSPUP (which has so far restricted itself to reviews by discipline) and by an interagency committee of administrators.

Brooks has no great enthusiasm for the effort to set priorities. He points out that none of the proposed rankings—and Reagan's is no exception—offers quantitative guidance. The trouble with all such efforts is that "the problem of scientific choice is multidimensional." (45) "From the standpoint of research allocation, it is difficult to compare atomic energy with pollution control, or space with the removal of urban blight. . . . " (64) How can the value of chemistry to agriculture and industry be measured against the value of astronomy to man's understanding of his place in the universe? (50) It is virtually impossible to assign a meaningful priority to a field as a whole because "there is too large a variation in the quality and relevance of projects within a broad field. . . . " (50) Nor does it make sense to rely on such indices as "proposal pressure," output of publishable papers, or the availability of qualified researchers since, when it comes to absorbing funds, even little science can be open-ended. On the other hand, how is it possible to decide that a given field has attracted all the talented manpower it could conceivably recruit if it were offered unlimited funding? (73)

Categories, as distinct from priorities, are necessary and useful. Brooks offers several sets of these, but he points out that they too are fluid and sometimes misleading. In fiscal 1964, the NSF reported total federal expenditures for basic research of $1.6 billion. Nearly half this sum was accounted for by NASA; but nearly 80 per cent of what NASA classified as "basic research" went into the design and procurement of space vehicles, tracking ranges, etc. (131) Similar ambiguities arise in other areas where "last year's basic research is often this year's applied research" and "the basic research of one field may be the technology of another." (180) Virtually all fundamental research in biology may be classed as applied science "since in our rudimentary state almost any advance is likely to find applications rather quickly in medicine or agriculture." (180) Even presumably mission-oriented and presumably basic research projects funded by the military services have resulted in such practical applications as the laser and maser, the plastic cornea, and a technique for rapidly freezing blood. (113, 124) If disciplines are treated as categories, how can due account be taken of hybrids like biochemistry and geophysics that have lately appeared in rich profusion? (181)

In view of all these subtleties—of which Brooks seems to have an inexhaustible supply—the best way to govern something as inherently ungovernable as science is by "providing over a sustained period the best possible environment for decentralized initiative, and for individual and institutional creativity." (51) The most useful way to classify research is according to "the primary purpose of the institutions or organizational subdivisions that perform it." (184) "The criteria," in other words, "are considerably less important than who applies them." (160)

Although he claims to be avoiding the "unrealistic" extremes of complete autonomy or centralized control, Brooks is actually expounding the ideology of establishment: instead of trying vainly to construct and impose political directives, just hand over the alms box to those who understand the mysteries and let them decide how to divide the collection. The trouble with this ideology is that it is as unacceptable to politicians as dictation by government is unacceptable to scientists. As Reagan puts it, echoing Congressional sentiment, "the funds are not to be left in a hollow stump in the middle of the night, as some researchers would like." (109) Brooks' solution is no more immune to criticism than Reagan's. Again, it provides no quantitative guidance. It relies on the exercise of responsibility by institutions, notably universities, even though Brooks him-

self points out that the academic climate "generates strong pressures for equal distribution of support, regardless of merit, especially to senior people whose earlier productivity may have declined." (75) Worst of all, instead of confronting the admittedly thorny but vital problem of setting priorities, it fragments and relocates the problem, in effect asking public and private organizations to do separately what the government presumably cannot manage to do collectively on behalf of the taxpaying public.

Brooks' skepticism is nevertheless an effective counterweight to Reagan's reformism. Brooks, who is Dean of Engineering and Applied Physics at Harvard, is probably the most widely experienced and the most thoughtful member of the establishment's hierarchy. Each of his papers is studded with brilliant insights and caveats. The rare flaws crop up only when he ranges far afield or when he is overly defensive. In an otherwise illuminating discussion of similarities between scientific thought and other facets of modern culture, he mistakenly claims that the philosophic concept of progress is derived from the natural scientific theory of evolution and natural selection. In fact, of course, it is the other way round. In an essay on "the scientific adviser" he argues that criticism of PSAC for failing to give due represensentation to certain of the scientific disciplines and to the diversity of scientists' political views is either exaggerated or beside the point, since PSAC is not supposed to be a legislative body—a response to a perfectly reasonable complaint which is, to say the least, disappointing. Reagan's accurate and readable study also merits high praise, especially for its creative approach to the practical tasks of reform. The main limitation of the study is one that the author has himself imposed by confining his attention to policy for "pure" science. This is a serious limitation because it makes his recommendations less clear and less convincing than they might otherwise be. Would an expanded OST and the proposed JCST be expected to handle development as well as research? How would a reform of science policy affect the links between industry and government? Would it change the present role of scientists in policy-making? Such questions are neither asked nor answered. Another shortcoming of both these books is that at a time when the U. S. should be thinking seriously about meshing its domestic policies with its foreign policies, especially in science and technology, neither author gives more than passing attention to the problem of formulating domestic science policy to take account of international commitments and constraints.

Regardless of their limitations, however, both these works make valuable contributions to the more comprehensive study now well underway of the interaction of science and society. In time, to revert to the historical analogy, this study may help make it possible for some future Locke to put the interaction in proper perspective. From a later point of view, the controversy over the establishment of science may come to be regarded, much as we now regard the controversy over the establishment of religion, as the forerunner of a great social transformation, fully as significant as the earlier passage from feudalism to liberalism and industrial society. David Apter hints at the character of this transformation when he suggests (*The Politics of Modernization*, 433) that in an age of science "the application of knowledge by political means—and not the responsiveness of government to private wants—becomes the test of good government."

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Television in Politics. BY JAY G. BLUMLER, AND DENIS MCQUAIL. (Chicago: University of Chicago Press, 1969. Pp. 361. $13.25.)

This book should be of interest to political scientists for at least two reasons. First, and most important, it is a major contribution to the literature on the political impact of television. This literature oscillates between impressionistic accounts that are uninformed by data and small-scale analyses that are relevant to the Big Issues only by strained inferences; *Television in Politics* presents hard-data analysis that *is* relevant to the Big Issues. Second, it gives valuable insights into British electoral processes during a period of political change.

The book's analytic strength comes from its focus on the O part of the S-O-R paradigm. In the author's words, its point of departure was "the expectation that in the persuasive process sheer amount of exposure to television might matter less than the precise motives, and the different degrees of motivation, underlying the reception by viewers of political materials." (p. 6) Accordingly, they adopt a "uses-and-gratifications approach" which asks about the reasons why people watch political broadcasts, the uses made of the broadcasts, and preferences for different formats. This lets them develop their findings about the *impact* of broadcasts in terms of an interaction between motivation and exposure.

The basic fact which stimulates concern about T.V. is its wide reach. The authors show that almost three quarters of all voters watched at least one party broadcast in 1964 compared

to two-fifths in 1955. But even more important is the wide reach among voters who would not otherwise be paying much attention to the campaign. This part of the population (about one-half of those tuned-in) watches out of *toleration* for party broadcasts ("It never gets switched off. I was waiting for Frank Sinatra Jr.") in more or less the way one watches the news; it is best described as surveillance of the political environment. These people are *reached* by political information but do not seek it out for political ends.

The two most important motives for seeking information, on the other hand, are a desire for reinforcement of political predispositions and a need for help in the voting decision. About fifteen percent of viewers were "pure" vote-guidance seekers, and twenty-three percent were "pure" reinforcement seekers; a further eleven percent had some mixture of these motives. This distinction is important (as the authors point out) because of various attempts to describe the "typical voter" as *simply* reinforcement-seeking or *simply* collecting information in order to help with a rational vote-calculus. More accurately, there is a large body with no political motive for watching and, beyond this, a continuum of electors stretched between the reinforcement-seeking end and the vote-guidance end. Party loyalty is the motivating factor for the former, and party choice the motivating factor for the latter.

The central theoretical variable in this study is a measure of the strength of a viewer's motivation to follow the election campaign on T.V. Some such variable is necessary because exposure to T.V. is so widespread; high exposure can be explained by habitual use of the set or by deep concern for the political subject matter of broadcasts. It is also necessary because of the theoretical expectation that motivation is related to susceptibility to influence. In short, it is likely to be an important intervening variable between exposure to the stimulus and the attitude-change response.

As expected, exposure to the T.V. campaign varied positively with motivation to attend the campaign on T.V. (although, interestingly enough, there was a negative relationship between such motivation and general use of T.V.). The interaction of exposure and motivation, however, is best demonstrated by the fortunes of the Liberal party during the campaign. It is clear that the Liberal Party gained most from the campaign. The panel data show the Liberals gaining votes from Conservative and Labour defections, but losing very little themselves, and pro-Liberal attitude change was strongly (and positively) related to exposure to Liberal campaign broadcasts. But when motivation is held constant this latter relationship becomes erratic for the high motivated group and very strong indeed for the low motivated group. Attitude change, in other words, is a function of exposure *when viewer motivation is low but not when it is high.*

From the perspective of trends in British politics, the important question this raises is why the campaign should have so helped the Liberal Party. The authors suggest that its central position in a bi-polar party system helped it: a shift to the middle is easier than a shift to the other extreme. They also make two suggestions which, in the light of their main theoretical thrust, seem to deserve more attention than they get. The first is that the politically indifferent section of the electorate (those who are less motivated to attend to broadcasts but who, nevertheless, are exposed to them) were particularly receptive to Liberal propaganda because of generally lukewarm feelings about the other parties. The second is that the election campaign coverage boosts the Liberal Party more than the others because of its relative obscurity in the period between the elections. The average citizen "who is not actively seeking political information but takes it as it comes" (i.e.: the low motivated citizen) will be strongly affected when he is exposed to campaign-levels of transmission. As the authors point out, this happens more with T.V. than with the press because T.V. is obliged to devote a definite time allocation to the Liberal case. These are speculations of some significance; if they are correct, the fortunes of all third parties are considerably boosted by the T.V. revolution in political communications.

Since voting theory generally points to "issue factors" and "candidate factors" as the main components of the vote (after "party factors") their findings about the role of T.V. in shaping issue awareness and candidate-related attitudes are important. Exposure to the T.V. campaign, as expected, is related to information gain about party issue positions, but motivation again plays a central role: information gain falls off at the highest levels of exposure, *unless* such exposure goes with high motivation to attend, in which case "motivation takes over".

Their data also help to resolve (or at least make more sophisticated) the debate over the role of T.V. in perceptions of candidates. In a nutshell, they find that *factual* knowledge about the candidates—for example, about personality characteristics—changed considerably during the campaign, and that the change was "stimulus determined" rather than "perceiver

determined". On the other hand, *evaluations*—moral characteristics—tended to be "perceiver determined" rather than "stimulus determined". Thus it is not sufficient to say, as many have, that what one gets out of T.V. depends on what political prejudices are brought to the viewing situation; this might be true for evaluations, but it is not true for more substantive information.

One might have asked the authors for a somewhat more systematic attempt at formalizing their theory; as it is, the reader sometimes has to do the job himself. However, the theory is there for the picking. It is a classic in the field.

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Communist Party Membership in the U.S.S.R., 1917–1967. BY T. H. RIGBY. (Princeton: Princeton University Press, 1968, Pp. 573, $15.00.)

Professor Rigby has written a sterling monograph of heroic proportions, which is precisely limited to a dynamic profile of the Communist Party of the Soviet Union during its half-century period of power. The book is studded with charts and statistical tables, unencumbered with irrelevant quantitative prestidigitation. The apparent unsophistication of his statistical computations may disturb some readers, but his data will probably provide years of employment to other scholars as they mine the rich lode of numbers he supplies.

The book is organized very simply. It includes a conceptual introduction which appears to be an afterthought that seems to have little organic connection with the body of the study. The work is largely descriptive and bears little relationship to the author's claim that "the framework we shall employ here is a fairly radical adaptation of that proposed by Gabriel Almond in his introduction to *The Politics of Developing Areas* (p. 18)." As a meticulously documented work of descriptive analysis, the study can stand sturdily and independently on its own merits and hardly requires the conceptual crutch provided in the introduction. After 54 pages of paying ritualistic obeisance to the cult of concept-manipulation, the author dispenses with his framework until page 510, where it is resurrected for another 15 pages. "This framework," Rigby writes, "is implicit in the body of the book, but we have avoided rigid pigeon-holing of data in terms of abstract concepts or constant explicit reference back to our analytical framework, since this would overload the text with jargon and exasperate readers who did not happen to be students of comparative politics or political sociology. In the Conclusion we attempt to summarize our findings in terms of this framework." (p. 50).

The body of the work is not cast within the framework described in the first 54 or last 15 pages of the book, nor indeed is it cast within any systematic conceptual mold, aside from that of traditional descriptive analysis. This does not in any way detract from the significance of his research or its usefulness to others, for Professor Rigby has produced a monumental study which will remain definitive for many years. Previous readers of Professor Rigby's articles on the Soviet Communist Party need no introduction either to his methods and approaches, which might be characterized as traditional-innovative, or to the significance of his research, which has always been rigorous in its precision, meticulous in its documentation, modest in its claims, thoroughly reliable in its presentation, perhaps a little too prudent and judicious in its conclusions, and nearly always heuristic in its exfoliation. The book under review, which is an extrapolation and refinement of his previous research, reflects equal care and discrimination in the employment of data and reportage of conclusions.

The substance of the book is divided into two unequal parts. Part One covers "the main lines of CPSU recruitment policies and trends in membership, particularly social and occupational trends . . . in historical sequences." The second, somewhat shorter part, deals with a "number of special aspects . . . isolated for special treatment," *i.e.*, it deals with the overall character and composition of the Party in terms of turnover, sex, age, nationality, education, representativeness, and geographical distribution. Part One is thus essentially a chronological history of the Party as it was alternately "proletarianized," and "de-proletarianized" during the vicissitudes of the Stalinist era and its transformation into an instrument of the multi-faceted ruling bureaucracy. The periodic attempts to establish a balance between the imperatives of a proletarian party and an effective or competent party is also covered, culminating in the emergence of Khrushchev's All-People's Party, *i.e.* a quasi-mass political organization, within which is secreted the "ruling elite." Part Two, which is divided into six chapters, also contains much useful information of a statistical nature, although it, too suffers from insufficient analysis.

Although it was clearly not the intention of the author to produce a work on the Soviet political system as such, his reluctance to deal with the dynamics or "stuff" of Soviet politics

in a book of more than 500 pages on the CPSU inevitably imparts to his study an eviscerated quality. This becomes particularly apparent when Professor Rigby, in his most determined effort at analysis in a chapter entitled "A Representative Elite?," fails to answer satisfactorily the tantalizing question he poses. In this chapter, the author devotes considerable attention to the socio-occupational composition of the Party and the Party composition of socio-occupational groupings, but nowhere does he deal with the institutional patterns of interlock and interaction at the Summit of the Soviet system, which he must do if he wishes to provide an answer to his own question. The last paragraph of the chapter begins with the promising words, "We are now in a position to refine our proposed concept of the CPSU as a 'representative elite,' " but characteristically, he leaves the reader pleading for a judgment which does not materialize. "The CPSU is certainly an elite in Soviet society," he observes, "and it certainly consists of representatives of all sections of that society." Yet, one would conclude from his own preceding descriptive account that a 13,000,000 member organization, over half of whom are nobodies, can hardly be called an "elite," although it might be described as "representative" (p. 453).

But because the author did not examine the interlocking membership and interacting character of the important decision-making organs of the Soviet political system, he is not really in a position to define or delineate the Soviet elite or elites, much less to determine whether the Party itself is in fact an "elite." As Professor Rigby's data itself demonstrates, the CPSU cannot be an elite organization, but rather is an organization which incorporates three elites almost completely (the armed forces, the police, and the Party Apparatus, although the author mystifyingly omits the latter) and "links up the various [other] elites of Soviet society . . .by overlapping with them, not by incorporating them." Hence, the Party emerges quite clearly, not as an elite, but as an arena within which the various elites in Soviet society articulate their interests and make their demands known upon one another as they compete for power, influence and resources. And how this is executed cannot be fully understood without an examination of the structural and institutional relationship between the Party and other structures and institutions in Soviet society A single chapter devoted to this aspect of the Soviet system would have made an excellent study even more rewarding

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Mr. Justice Murphy: A Political Biography. BY J. WOODFORD HOWARD. (Princeton: Princeton University Press, 1968. Pp. 578. $12.50.)

Revelations of judicial life in the Supreme Court of the United States from the time Frank Murphy took his seat on February 5, 1940 to his death on July 19, 1949 make this an important, fascinating study. The book also holds riches for students of personality and of institutions. Murphy was born in 1890, went to the University of Michigan Law School, practiced law, was a Captain in the A.E.F., a losing Democratic candidate for Congress in 1920, a big electoral winner in Detroit for judge of Recorder's Court in 1923 and 1929, for mayor of Detroit in 1930, and for Governor of Michigan in 1936. He won national attention as judge in the 1925 Ossian Sweet racial defense case, as compassionate depression mayor and cool, adroit governor in the climactic sit-down strikes. Murphy was For-Roosevelt-Before-Chicago, and F.D.R. named him chief American representative to the Philippines in 1933, Attorney General in 1939 and, the final reward, Associate Justice. Each episode in Murphy's career is treated in such rich detail that the book encompasses legal education, urban politics, colonialism, federalism, parties and patronage, the President's cabinet, and policy-making in the Justice Department. The treatment is personal and specific, however, so the focus is on Frank Murphy in each setting and there are no elaborate hypotheses of office or reviews of the specialized writing on these topics. The book is well informed on each point and it is thorough; it simply offers little novelty in biographical method.

Howard is the first to tap the magnificent Murphy manuscript collection in the University of Michigan Library but the sparsity of other judicial papers and Howard's rules in using those of Murphy keeps the book from being a full-fledged study of the Roosevelt Court. Four brethren (Black, Reed, Frankfurter and Douglas) preceded Murphy to the Court and were there after he died, serving all nine terms with him. The coincidence of terms of service with others was Jackson, eight; Stone, C. J., and Rutledge, five each; Roberts and Burton, four each; Vinson, C. J., three; McReynolds, Hughes, C. J., and Byrnes, one each. I do not take exception to Howard's discretionary rules in tapping the available confidential papers but wish those rules were more completely spelled out along with an account of other available papers. This would show the possibilities open to future students. This task

is not done by the impeccable footnotes and brief preface.

Consider the problem of confidential sources further. Howard rules out "embarrassing individuals or the Court," abhors quotes that "might be misconstrued as an effort to discredit a Justice," and "chooses not to undertake extensive treatment of a Justice on the bench." For the living, also, he has not quoted from the papers of any Justice without permission. He acknowledges "the cost is to present Justices Black and Douglas as relatively wooden characters." (p. ix) Essentially the same result holds for Reed and Byrnes, who today are living retired justices; Roberts, Vinson and McReynolds, who seem to have left no manuscript collections; and Burton, whose papers in the Library of Congress are dull, though helpful in corroborating votes in conference and the like. The Library of Congress manuscript collection for Hughes was used in the Merlo Pusey biography, that for Stone in Alpheus Mason's notable and exhaustive biography. The Rutledge papers, not very well exploited in the Fowler Harper biography, remain in family hands and were apparently not available to Howard. There seem to be no papers for Robert H. Jackson and his long oral history reminiscence will not be open in the Columbia Special Collections until June 1, 1980!

What remains, then, is very largely a study of two justices, Frank Murphy and Felix Frankfurter. The Murphy papers are full of notes from Frankfurter, beseeching, praising, belittling, mocking. They are stuff for a playwright, like Albee perhaps, and are tantalizing to the straight scholar who sees in them the possibilities for analysis but lacks the tools and the daring. Howard presents them as part of descriptions of decision making by the Court and of the anxieties Justice Murphy felt in reaching outcomes. At points this book becomes a dual biography of Frankfurter and Murphy but even with Frankfurter damning himself by tart, mean notes and persistence connivance to win votes to his views he seems to come off better in Howard's estimation than does Murphy.

Murphy is praised for his instincts and for his courage in the *Korematsu* and *Yamashita* cases for "he seldom muffed the big ones." (p. 487) Yet Howard concludes that Murphy was not a "great" jurist, had no "well-rounded philosophy of the judicial function" but rather exemplified "the native tradition which distrusts 'legalism,' scholastics, and theoreticians." (p. 486). This was the kind of assessment Frankfurter made in his note to Justice Murphy addressed "Dear god" where he says "it must be wonderful to be a god" for, after all, "a god does not have to read or study things in order to understand them." (p. 396) By themselves, the spiciness of the Frankfurter notes makes this biography of Murphy fascinating. Mostly the material comes from the Murphy papers at Michigan. The Frankfurter correspondence in the Library of Congress is now open and was also used to an extent. When the Frankfurter judicial papers now at Harvard Law School are meshed with other related materials there will be work for an army of scholars to piece his career together and for dramatists to bring it to life.

Just as the Frankfurter characterization is implicit and simply keeps pushing itself upon the reader in the context of this study of Murphy, so, too, is the theme of irrationality and political by-play put down by Howard's assertion that appellate courts "probably achieve greater rationality in their decision-making than legislative or executive organs, whose processes of resolving disputes necessarily require negotiation, unprincipled behavior, and secrecy which judges must eschew." (p. 483) Howard shares Chief Justice Hughes' belief that "knowledge of how courts operate would vindicate popular respect for the judiciary." (p. viii) It would be interesting to operationalize the meaning of "vindicate" and "popular respect" and test this hypothesis by apprising and polling different publics about 1) Justice Murphy asking his Budget Bureau friend and roommate, Edward G. Kemp, to comment on drafts of his opinions; 2) colleagues hiding doubts from Murphy, while as a freshman justice he wrote the *Thornhill* opinion, because they hoped he would join them as an ally later; 3) how Black and Douglas shed extreme equivocation in the decision-making process and set forth conclusions in their opinions which were stripped of even a hint of this difficulty; 4) Frankfurter's caucus of justices he hoped would vote with him in the *Everson* case; 5) Frankfurter's Sunday letter of appeal to Murphy to decide that case in the liberal Catholic tradition of Cardinal Gibbons; 6) Murphy's outrage at being publicly denounced by the clergy for his position in the *McCollum* case and his action in planting rebuttals among friendly priests and journalists; 7) Murphy being reviled within the Court for his decisions which, it was said, gave him a constituency of aliens, crooks and communists.

Apart from the sensational items that appeal to us tabloid readers we get a picture of decision-making which seems to me to be definitely believable. Howard has done a beautiful

job of capturing the complexities of that process in a whole series of cases. I like the book. It contributes mightily to understanding, especially when read with Herman Pritchett's *Roosevelt Court* and the *Vinson Court and Civil Liberties*. Yet I am not convinced that Howard's *Mr. Justice Murphy* demonstrates that the Supreme Court of the 1940's achieved greater rationality than Congress or the President. I await the polls to learn whether Howard's nicely wrought inside story will "vindicate popular respect for the judiciary."

CLEMENT E. VOSE
Wesleyan University

Obscenity and Public Morality: Censorship in a Liberal Society. By HARRY M. CLOR. (Chicago: The University of Chicago Press, 1969. Pp. 315. $9.50.)

Here is a book that sets out to "develop a philosophy of censorship," by an author who, addressing what is probably a largely libertarian audience, recognizes that the trends of the times seem to be "contrary to the thesis of this book." Clearly, we are in the presence of a brave man. Here, also, is a book of breadth and sophistication, well-researched, closely argued, and moderate in tone. In short, this is one of the most effective statements of the value of obscenity control to be made to date, and one that deserves a full hearing, especially from libertarians. It is a book not without difficulties, but first, Professor Clor's argument, or at least a compression of it, should be set forth.

The author, presenting a systematic review of arguments and evidence on the effects of obscenity, concludes that though the evidence on whether obscenity is a cause of anti-social behavior is inconclusive, there is good reason to believe that it "can contribute to the debasement of moral standards and ultimately of character." Asserting that these standards are a legitimate concern of government, he argues that not only have all societies dealt with depiction of the physical and sensual side of life by establishing binding public moral standards, but also that such standards are, in fact, *essential* to a civil society. Yet the community cannot rely solely on non-governmental institutions—the family, church, public opinion—to maintain these moral standards, since these agencies are, today, more often shaped by the standards than they are the shapers of them. Government and the law, then, have a necessary and legitimate function to perform in preventing the most corrupt influence and in holding up an authoritative standard for the guidance of opinions and judgment. However, ordinary notions and legal formulas of obscenity are not suitable guides for censorship, because they do not sufficiently explain what is wrong with obscenity. Obscenity should be seen in moral perspective, as a "way of looking at man which dehumanizes human purposes and human beings." Its harm lies in its reductionist isolation of sex, death, and suffering to the animal or subhuman level.

In actual obscenity control, these moral considerations must be joined with both the community's interest in protecting its "common beliefs" or "moral consensus," and its interest in the circulation of literature. Clor would exempt from control works of acknowledged excellence or seriousness. For more difficult cases, he calls for a "recognition" (as opposed to "blanket imposition") of community standards, and suggests four "public opinions," beginning with the lowest common denominator of shared attitudes and prescriptions, which can be utilized to discern community standards. This is too important a matter to be left to literary critics, whose function does not ordinarily include a careful weighing of "those elements of the public interest which may be found on the philistine's side of the controversy." Instead, this responsibility must be assumed by a "judge, or a jury under a judge's guidance." Clor believes that such a system, though leaving untouched much that might otherwise be considered obscene, would be a barrier to the "worst instrusions." Our obscenity jurisprudence should reflect a concern for public morality at least in its rhetoric, if not always in its decisions. He concludes that censorship of obscenity, though not a liberal-democratic measure, can still serve a liberal democracy by "helping it maintain ethical and political proportion," in being a "counterweight to those evils or excesses which are a by-product of its virtues."

I shall leave ideological criticism of this book to those who see any kind word for governmental censorship as Armageddon, in order to consider some practical aspects of Clor's analysis and thesis. His critique of the Supreme Court's work in obscenity cases and of the libertarian position generally, is painstaking and impressive. In fact, it is hard to see how any further examination of the obscenity decisions, already a well-worn ground for scholars, can improve upon this searching analysis. Probing the libertarian mind, however, is a much less frequently assumed task, and Clor, examining the works of a number of modern writers, finds the libertarian position to be a "complex combination of truths, partial truths, fallacies, and unexamined assumptions." Its ethical relativism and absolute commitment to

individual liberty combine to create a climate of irresponsibility. This assertion will raise hackles, but if it succeeds in inducing any self-examination at all, it will sharpen future value debate. Clor also shifts the burden of proof, asserting that libertarians have not established that *all* censorship is bad, nor that obscenity is *totally* without harmful effects. In times less super-permissive than those of the present, this would be a less defensible tactic. Actually, in relying upon the availability of constitutional checks and the fact that "censorship is not our normal way of governing," Clor displays greater confidence in the viability of the free speech system than do many libertarians. However, at these and various other crucial points—the requisites of a civil society, the ineffectuality of nongovernmental institutions, etc.—we are mightily in the realm of conjecture, where philosophers thrive and policy-makers founder.

Clor's arguments are less impressive as he turns from critique to prescription, and it is exactly here that the considerable distance between general value guidelines and operational concerns becomes most apparent. It is not clear, for example, whether his "contemporary community" is the nation or some lesser social entity (a reflection of the same ambiguity for which he has earlier criticized the Supreme Court). In his prescriptive ambitions for it, the concept seems to refer to the former, but as a society that would yield up a discoverable "moral consensus," it appears to be decidedly local. In fact, the entire notion of a national "moral consensus," one that is more than just the most conservative opinion and yet sharp enough to be a judgmental instrument, is a highly dubious one, to say the least. The role Clor sets out for his judge is equally improbable. It appears that this key public servant must be equal parts man of letters, moral philosopher, and public opinion analyst, besides being blessed with the political isolation of a monk. Some triers of some obscenity cases have perhaps approached this ideal, but a *system* of such judicial supermen is a little too much to expect. When and if the more conservative disposition in local communities or in the nation makes itself felt on obscenity questions, its vehicle will likely be that of political pressure, variously applied, rather than the juridical discoveries outlined here. Clor also says nothing about the prosecutor, who has much formal as well as informal discretion in any governmental obscenity censorship. Likewise, he excuses himself at the outset from dealing with either the procedural aspects of obscenity censorship or with informal censorship of a governmental or group character and its relationship to formal censorship, though he does recognize the importance of these questions. Yet it is on just the latter question that Clor's ideas about community standards have much relevance, and it is from that problem area that we find empirical studies of conflict management which would support some of his arguments.

A philosophy or rationale for censorship cannot rest on the interest of public morality alone. Before we can feel at ease with governmental obscenity censorship, we need to know more about the effects of obscenity itself, the effects of various censorship devices and methods, and something about the relationship of informal censorship to governmental controls. Though Clor has told us nothing new about these problems, he has impressively analyzed, evaluated, and clarified what we do know, and has in addition, made a forceful and skillful case for giving renewed attention to the concerns of public morality.

RICHARD S. RANDALL
New York University

The Precarious Republic: Political Modernization in Lebanon. By MICHAEL C. HUDSON. (New York: Random House, 1968. Pp. 364. $8.95.)

This single-nation case study is an example of the small but growing body of scholarship on contemporary Middle Eastern politics that reflects the continuing revolution in the field of comparative politics. Professor Hudson addresses himself to many of the questions and problems posed in recent years by students of political development. The influence of the conceptual and theoretical approaches of Karl Deutsch, Samuel Huntington and others is apparent; also apparent is the perspective on data-making and data-analysis associated with users of nation-level aggregate data.

Professor Hudson organizes his study around two questions: (1) how has Lebanon's shallowly rooted liberal democracy managed to survive the formidable challenges of the first two decades of independence; and (2) what are its chances for survival in the forseeable future? He attempts to answer these questions through an analysis of the relationships between the changing capabilities of the Lebanese political system and the changing burden of demands emenating from its changing environment, domestic and external. As he sees it, such an analysis requires study of the nature, velocity and amplitude of change in both the political system and its environment.

Professor Hudson's major and most provoca-

tive proposition concerns the relationships between democratic instrumentation, political stability and societal development. Standing the prevailing theory of stable democracy on its head, he suggests that in the Lebanese case "there is reason to think that . . . democratic institutions have been a requisite for political stability, not a result of it." (8) Strong sectarian sentiments and a deeply rooted pattern of "traditional pluralism" deny Lebanon the political consensus and highly legitimate machinery of state that are presumed to be the bases of order and stability in the modern nation-state. Hudson draws a suggestive analogy between the domestic environment of Lebanese politics and the classic international balance of power system: in each case, a number of both highly autonomous and interdependent actors seek security and attempt to advance their interests in a setting marked by mutual suspicion and hostility. According to Hudson, democratic instrumentation—albeit limited by situation and intent—has provided the institutional framework in which Lebanon has managed to avoid sliding into the Hobbesian "state of nature." Flexible and porous, Lebanon's oligarchical bargaining system has been remarkably successful in maintaining and incrementally adjusting the delicate balance upon which order and stability have depended. In this fashion, the political system has managed to survive in an environment made inhospitable by recurring sectarian tensions and external influences.

This, however, is only half the story. Lebanon's quasi-democratic system has a much less adequate performance record in matters of national integration, modernization and social justice. What lends urgency to this situation is the fact that private (and some public) initiatives over the past two decades have produced a pattern of extremely rapid, uneven economic development and social mobilization. As a result, the stability of the Lebanese system is increasingly threatened by the demands of the recently mobilized for fundamental reforms. The Lebanese political establishment, concludes Hudson, is confronted with a potential dilemma: "To adjust to the new social justice and the newly politicized elements may lead to a derangement of the traditional balance of power, with its attendent instability; not to adjust may invite the total destruction of the political system." (12)

Professor Hudson draws upon a rich variety of data to support his thesis that democracy has been more a blessing than a curse to Lebanon. In addition to a wide range of secondary sources, he makes good use of interview data, census-type aggregate data and electoral statistics. His skillful use of the available aggregate data to construct a picture of the rate and scope of social mobilization is particularly noteworthy, as is his resourceful manipulation of electoral data to determine changes in the patterns of political participation, competitiveness and organizational development. These varied materials are woven together and interpreted in ways that are always interesting and intelligible. The different patterns of presidential power and institutional performance of the Khoury, Chamoun and Chehab regimes are presented against a rich environmental backdrop: the personalities, factions and social bases of the "establishment" and the "radical outsiders;" the ebb and flow of parochial strains and external influences; and the changing patterns of social mobilization, development and political participation.

Lebanese politics is, among other things, exciting. By including ample portions of narrative history, Hudson is able to subject two decades of Lebanese politics to systematic analysis without sacrificing this excitement. What results is the best macropolitical study in English of contemporary Lebanon.

Despite its undeniable virtues—or perhaps because of them—this volume is a sharp reminder of the gap between aspiration and realization in the study of comparative politics. Hudson's study reveals notable disjunctions among theory, data and mode of analysis. For example, he seems to consider the "shared doctrine" (126) or "common code" (147) of the ruling elite as an intervening (or independent) variable explaining Lebanon's capacity to survive. Nevertheless, the specific content of this elite culture is not studied (or at least not reported) systematically; nor is the fragmented political culture of the non-elites related to that of the elite. To tap these dimensions, of course, would require extensive survey or content analysis. An important disjunction also exists between the mode of analysis on the one hand, and theory and data on the other. Single-nation studies of so time-oriented a theoretical concern as political development and mobilization may be more appropriately approached by longitudinal techniques. Multiple time-series analysis, which orders and relates data in terms of precise units of time, may be the best way to handle questions in which rate and sequence of change are crucially important and to integrate quantitative and non-quantitative materials in the same historical study. Admittedly, the data constraints are formidable. Nevertheless, in the case of Lebanon, sufficient data seem to be available to attempt to relate series of "event

statistics" to data on an interesting range of quarterly, semi-annual or annual social indicators. In this fashion, a tighter fit might be achieved between changing system performance and stability on the one hand, and changing demands from the domestic and external environment on the other hand.

Nevertheless, Professor Hudson does carry us a number of steps towards an understanding of the dynamics of Lebanese politics. Moreover, his volume whets the appetite. Lebanon recommends itself as a laboratory for the close study of political development and modernization. Perhaps Professor Hudson will find time in the future to seek answers to the questions that his volume raises and leaves unanswered.

ROBERT BURROWES
New York University

Democracy in the Administrative State. BY EMMETT S. REDFORD. (New York: Oxford University Press, 1969. Pp. 211. $5.00; paper, $2.25.)

This is a careful, thoughtful and systematic effort, characteristic of its author, to assess the actualities and potentialities of democratic performance in the workings of the "administrative state"—by which he means the institutions and processes of administration considered in the context of the total political system. He does not try for quantification in the assessment, but he is clear about standards of qualitative judgment, and he comes up with some analytical categories that mark, I think, a substantial advance over previous thinking on this subject.

The first chapter states his three tenets of democratic morality—a premium on "individual realization" as a goal of public policy; the equality of men in their claims for attention; and the widest practicable provision for participation—along with three ways of arriving at policies, by elaboration of consensus, by majority vote and by brokerage of interests. It proceeds to suggest typical "quandaries" inherent in attempts to meet democratic standards as well as other tests of government such as wisdom, the reduction of tensions and order. Humane policy, open access to those who make it, and a share in the processes of developing it, are his touchstones.

The second chapter reflects on significant features of administrative institutions and notes the influence of the official roles, the personal stakes and the professional norms of the decision makers. The third chapter sketches a generalized but realistic model of institutional relationships.

With this much by way of groundwork the fourth and fifth chapters develop three generic categories of administrative action—the author calls them "micropolitics", "subsystem politics" and "macropolitics", respectively—and examines each in turn to see how the tenets of democratic morality fare. A sixth chapter considers the protections and remedies available to citizens as subjects, in connection with such topics as redress of individual grievances, invasions of privacy and participation. The seventh chapter treats the problems of public employees and ways of ameliorating the divergences between their personal interests and the public goals of the agencies they work for. The final chapter strikes a more philosophical note and ends in a judicious balance of satisfaction and concern for the prospects for "workable" democracy in the processes of administration.

Five years ago, Theodore Lowi, in a notably original essay (in *World Politics*, July 1964) distinguished three kinds of what he called policy "arenas" for domestic legislation—"distributive", "regulatory" and "redistributive". They were particularly suggestive because each was associated with a type of politics said to be characteristic of it—patronage, pressure group and class politics, respectively—and therefore was explanatory as well as descriptive. The distinctions, however, were clearer in concept than in practice and broke down at the water's edge.

Redford notes (p. 84n) the parallel between his "micropolitics"—the satisfaction of an individual claim, the award of a contract or license, the location of a particular federal installation, the avoidance of a pending threat; more generally, a decision in which one or a very few persons, firms or localities have a very heavy stake in a matter that no one else cares about—and Lowi's "distributive" arena. But where Lowi was content to throw out a suggestion, Redford was gone on to analyze its operation and to show not only the grounds for its durability but also how the application of democratic tenets of morality to a repetitive group of cases tends to reduce the process from the methods of patronage and favoritism to an administrative routine conducted according to prescribed standards.

Beyond this, as it seems to me, Redford's two further categories, subsystem and macropolitics, are both more realistic and more explanatory than Lowi's. The parallel between "regulatory" and "subsystem" is close. But the cosy arrangements that writers from Pendleton Herring to Leiper Freeman have made familiar, the triangular arrangements that link cognate bureaus, congressional committees and outside

interest groups, have more than regulation in view. Redford's extensive and intensive previous work in regulatory administration puts him on home grounds in treating this topic. Moreover, his "macropolitics" is a concept at once more inclusive and more serviceable than "redistribution". Every transfer is some sort of redistribution, and it is not only class conflict such as produced the Wagner Act that broadens the scope of political involvement beyond the capacity of a subsystem to contain the ensuing controversy. Redford itemizes no less then seven distinct grounds for appeal to or intervention by macropolitical forces, to upset subsystem arrangements.

On other aspects of his general subject, he views sympathetically but without elaboration the demands for more participatory democracy in urban anti-poverty programs. He finds that the basic problem in improving executive-congressional relations, from the standpoint of advancing the democratic ideal, "lies in the pockets of narrow responsiveness in the leadership posts in Congress" (p. 131). And in anticipating the continued expansion of the administrative state, he offers the hypothesis that "*comprehensive, democratic jurisdiction attracts power*" (p. 184).

It is a book to cherish, from the hand of a scholar and statesman.

HARVEY C. MANSFIELD
Columbia University

Political Socialization. By RICHARD E. DAWSON AND KENNETH PREWITT. (Boston: Little, Brown & Co. 1969, Pp. 226. $1.95.)

Political Socialization has a comprehensible plan in developing an approach to political life, the processes by which new members of a polity learn about politics, and the agencies (family, education, media, peers etc.) that transmit popular political culture at various stages in the life cycle. This is competently and professionally done with the distinction between direct and indirect political learning (Chapter V), discontinuities in the socialization process (Chapter VI), and the discussion of peer group influence (Chapter VII) of particular value. Furthermore, in the introductory and concluding chapters, Dawson and Prewitt go to lengths beyond the usual call for more research to say how much we (and they) do not know about comparative socialization, relations among socialization agencies, collective processes of political change, and other critical matters.

However, the intellectual uneasiness of authors and reviewer ought to be articulated. Here are empirical propositions drawn from Dawson and Prewitt's volume, propositions that the reader can test for himself:

1. Political Socialization is much more useful in exploring and understanding the attitudes and behavior of children and adolescents than of young adults and mature citizens.
2. Political Socialization is more significant to processes of political continuity and consensus than it is to processes of political discontinuity and conflict.
3. Political Socialization is more useful in dealing with mediating agencies and institutions (schools, families, political parties) than with the influence of central political agencies (government bureacracies, the military, corporations).
4. Political Socialization has a greater explanatory and ideological affinity for the politics and outlooks of middle-class socializers (parents, teachers, party activists) than it does for the politics and outlooks of cumulatively advantaged and disadvantaged strata in the United States. Consequently, it deals with electoral and voluntary group politics more often than with the politics of policy-makers and protest.
5. Political Socialization is most useful in dealing with personality and cultural dimensions. It is least useful in understanding political contexts in which direct, unique, and significant struggles for power and values are going on.
6. In striving for organic explanations (system theory, functionalism, agency reinforcement) mainstream Political Socialization research is less sensitive to political processes and purposive striving that determine distributive policies and the rules of the game within socializing agencies themselves (universities, political parties, control of community and poverty agencies).
7. If valid, these propositions yield an important generalization: Political Socialization research as reviewed by Dawson and Prewitt can explain a very limited range of politics under very limited conditions with very limited applicability to political phenomena.

Dawson and Prewitt do observe that "the influence of political socialization includes more than the formation of the citizen's political views. Every dimension of the polity public order, justice, legitimacy, policy, stability, leadership is affected by the processes of political socialization and the structure through

which this process occurs." (p. 201). Why, then, are these central political matters not explained through the Political Socialization construct?

Consider that the authors adopt a building block premise in which individual patterns must be explored before one can develop more politically comprehensive and cumulative explanations (see pp. 201–2). Consider further that the scope of politics under discussion tends to be circumscribed by citizen experience with the ballot box:

> Democracy is no longer just a vague symbol but one which specifically connotes competitive elections and control of political authorities by constitutional means. The individual learns how his world is structured and where he fits into it. Government officials are elected and he, the citizen, gets to cast his ballot along with everyone else." (p. 23)

Experiences with military induction, the policeman's mace, the informal power structure of the ghetto and the corporate board fall outside the parameters of "getting to cast his ballot along with everyone else."

Moreover, political socialization occurs among diffuse and potentially similar agencies:

> In the United States the political culture is transmitted from parent to child, from teacher to student, from den mother to cub scout . . . there is no institution to oversee the entire process. There is no way to guarantee that all children will be similarly informed about the adult, political positions they will someday fill. (p. 30)

It is as if the government had been abolished by scholarly decree. What is one to make of a book in which the den mother and Selective Service are merely two socializing agencies, and in which changes in the Presidency and ABM policies are no more important than an influx of young Democrats into the teenage peer group on the block?

> Changes in the political world also make people revise their political outlooks. Political leaders come and go. The fortunes of political factions rise and fall. Old policy supports disappear and new policy demands come into being. (p. 54).

While socializing goes marching on! Dawson and Prewitt pepper their work with politically relevant examples from urban politics, black activism, and foreign policy issues. Yet they are unable to meaningfully integrate these illustrations within the text because these political examples are merely 'asides' to the central concept they are developing. Some of the uneasy feeling conveyed by the authors may indicate their awareness that they are fatally stuck with a concept that is applicable to the political norms of a milieu (essentially white, middle-class, suburban America). Herein, ongoing socialization norms must from time to time encounter and deal with irritating 'problems; racial conflict, wars, changes in political leadership and policy. Instead of being explained as public positional issues, these 'problems' become the problems of improperly socialized groups and individuals. *Ultimately, genuine political controversy and allocative processes get in the way of socialization theory.* Therefore, Dawson and Prewitt simply cannot explain the 'aside' issues of policy, justice, legitimacy, and power.

The difficulties are not the sole responsibility of the authors who have mined the professional literature at their disposal. The only question remaining is whether students of Political Socialization, and Political Science, wish to explain the critical issues of our time to their students.

EDGAR LITT

University of Connecticut

A Theory of Political Exchange: Economic Reasoning in Political Analysis. By R. L. CURRY, JR. AND L. L. WADF. (Englewood Cliffs: Prentice-Hall, Inc., 1968. Pp. 130.)

Although both are concerned with the processes of government, political science and economics are disparate enterprises. The concern of political scientists focuses on recording, correlating, or otherwise analyzing empirical data in the belief that rigorous theory grows from refinements in methodology or the application of relatively simple statistical techniques. Economics proceeds more directly to the development of rigorous deductive theory so that today few political scientists comprehend the research of these colleagues. Separating the economic from the political, however, is dangerous to the extent that the activities of governments have economic implications, and to the extent that economic activity affects the behavior of political institutions. Similarly, this separation, the success of economic theory, and the relative failure of political science to develop equally satisfactory theories leaves political science at a distinct disadvantage with respect to economics in its ability to conjecture about, advise on, and devise policy.

A prospective solution to this problem, however, is afforded by recent theorizing about individual decision-making which concludes that fundamental principles of behavior developed for economics are applicable to political activity. The "new" political economy asserts that the economic definition of rational man is con-

sistent generally with individual behavior, and that theories derived from this definition should assist in the explanation and prediction of noneconomic activity. The research of political and economic theorists such as Downs, Olson, Riker, Buchanan and Tullock, and Davis and Hinich demonstrates the relevance of economic theory to problems traditionally posed as political. Professors Curry and Wade adopt this perspective because, in their words, "empirical political research can profit . . . from attempts to move from a preoccupation with sociopsychological correlates of individual and group action to a more concerted concern with interaction." (p. 115)

This book commences with a general introduction to the economic concepts of utility and preference, and proceeds to a consideration of political processes as a form of market activity. The authors review the basic ideas of supply and demand and the derivation of these ideas from assumptions concerning preferences, budget constraints, and technical rates of substitution. The particular market considered is the demand and supply of administrative elites, with the conclusion that "The government intervenes into the market to coerce an equilibrium quantity of appointments when the reservation demand of the administrative elite group [the maximum demand permitted by established elites] prevents attainment of equilibrium." (p. 65)

The authors then consider briefly the problem of attaining an equilibrium when the assumption (of pure competition) that the behavior of an individual consumer or firm cannot affect other participants in the market is not satisfied (monopoly and duopoly). The authors provide a review of some elements of the relevant economic theory and suggest some political interpretations of these elements. For example, two party electoral contests exhibit many of the characteristics of duopolistic markets, where parties allocate policies in order to secure votes. The approach in this example, however, is symptomatic of the difficulties of this book. No theorems are derived from the analysis, and few testable propositions are presented. Since the authors fail to conduct any serious rigorous analysis, many of their conclusions fail to be supported by the more rigorous and thorough treatments in existence (e.g., the authors' conclusion that "From the perspective of exchange theory . . . it is impossible to specify the 'best' decision for either party," (p. 87), is clearly contradicted by the analyses of Davis and Hinich).

These difficulties derive from the confusion of two dimensions in studying politics from the economist's perspective. The first dimension concerns the concept of exchange, and the necessary and sufficient conditions for exchange to occur. The second dimension concerns the institutional and organizational environment of exchange relationships, and the theoretical properties of the commodities exchanged. As the authors correctly point out with reference to the first dimension, the concepts of economic and political exchange become synonomous once the perspective of behavior as utility maximization is adopted; only the commodities exchanged may differ. But this difference can be of significant theoretical importance with respect to the second dimension, and thus the market models constructed in economics may not be relevant for describing political exchange processes.

The theoretical generality of the concept of exchange insures that demand and supply relationships exist in all social processes, but an understanding of these relationships as they pertain to political processes requires an understanding of the concept of externality and the distinction between public and private goods. Briefly, externalities are said to exist whenever the utility of one or more individuals is dependent on the activity of other individuals. Public goods, as opposed to private goods, exhibit externalities in that if one or more individuals consumes such a good other individuals benefit from this consumption. An example of a public good is an automobile pollution control device; if some individuals purchase such a device the level of pollution which other members of the community must tolerate is decreased, and the externality associated with such purchases is the general reduction of air pollution. Theoretically, however, market mechanisms inefficiently allocate for public goods consumption; although everyone might agree that all cars should be equipped with anti-pollution devices, an individual is unlikely to purchase such a device because the incremental benefit of his purchase is exceeded by its cost. Decisions concerning the demand and supply of such goods, and the regulation of externalities, therefore, are frequently centralized (in governments).

The central difficulty associated with public goods and externalities, however, is that their benefits are not easily, if at all, measurable. Because the cost of providing a public good or the regulation of an externality must be allocated within the community, an individual may find it advantageous to disguise his true preferences from a government or any supplier of a public good. Any individual can claim that he doesn't value this provision, and, knowing full well that once the provision is made he will

benefit from it, claim that he should finance none of its costs. Governments, nevertheless, must allocate the rewards and costs of public goods, and the manner in which they do so is at the center of all political processes. A prerequisite to a satisfactory theory of political exchange, therefore, is a more explicit discussion than the authors provide of the conditions under which political exchange takes place, and a discussion of how specific factors such as elections, interest groups, constitutional structure, and leadership affect the allocation of benefits and costs.

The theorist might first attempt to postulate a specific process of exchange, and to deduce the patterns of exchange from the nature of the commodities involved. The authors' case, therefore, would have been stronger if they derived testable hypotheses in specific institutional environments. An alternative approach is to attempt to predict the institutional structures that emerge from the exchange of theoretical classes of commodities. Until these problems are solved theories of political exchange remain inaccurate descriptions or vacuous tautologies.

One completes a review such as this, however, with the conviction that the authors have been criticized for not accomplishing what the reviewer has not accomplished himself. This comment is not the gratuitous tip of the hat prescribed by etiquette. The problems encountered, and the insights and suggested interpretations of economic relationships provided by the authors undoubtedly will feed the development of deductive political theory. And it is this reviewer's conviction that the basic theoretical perspective advocated by Professors Curry and Wade is the proper one for realizing this development.

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Tangle of Hopes: American Commitments and World Order. By ERNST B. HAAS. (Englewood Cliffs: Prentice-Hall, Inc., 1969. Pp. 295. $7.95.)

Ernst Haas' many admirers have learned to expect much of him. He must therefore labor under their perhaps unrealistic expectations that he will give to every subject he touches the intellectual power of elegant theory, rigorous empiricism, and common sense. The present book is no exception. And for one Haas admirer, this *is* a generally persuasive, sometimes brilliant, and unfailingly stimulating reappraisal of the conventional wisdom that has dominated U.S. policy toward international organization for 25 years.

The second expectation with which Haas' reputation encumbers him is that he will successfully integrate into something approaching a general systems theory any given political sector to which he turns his hand. His look a few years ago at ILO was the occasion for a seminal theory-building exercise in integration, functionalism, and regionalism. Scholars and students hungry for some new theoretical magic that will elevate their own sometimes pedestrian descriptive efforts may be frustrated with this new work. But I don't think theory was the author's objective; his book could in fact have started on page 219, and it would have still been worth the price to watch a social scientist known for his analytical rigor articulate his ideals in a way at least one reader found both comely and professionally rewarding.

Professor Haas' avowed purpose is to:

> . . . recapitulate what happened to American expectations of the network of international organization and to American objectives in joining it; (p. viii)
>
> . . . throw into relief, and submit to our judgment, aspects of evolving international systems that clash with our aspirations; (p. viii)

and

> . . . criticize current American policy toward international organizations as probably destructive of the chief values we cherish, and . . . demonstrate that the continuation of this policy will not meet the challenges and demands of future international systems. (p. ix)

If Haas' disciples have their hang-ups, so naturally has the master. In Part I, the neo-Linnaean compulsion to taxonomy produces seven systems of international relations, with four characteristics, six types of nations, three styles of thinking, and three models of democracy and foreign policy. Still, the sections on international systems and American democracy keep social science jargon under reasonable control, and in places actively advance understanding of the role of elites.

I myself found him weakest in dealing with military power, strategic concepts, and the role defense analysts played a decade ago in sorting out the arms-controlling elements in strategic deterrence. On the latter he uncharacteristically does not credit those he might, perhaps because he is least at home in that literature. On the former, he proposes to discuss changing perceptions "without complicating our analysis with the elusive notion of power," but ten pages later this of course leaves him unable to analyze comprehensively the questions he raises there about the working of security systems. Still, in this the most abstract section of the work, one can profit from the rigor an

imaginative social scientist brings to a complex policy sector.

By the time Part II begins, it is established that Haas sees the relationship between national actors and the international milieu as a "learning process" with feedback both ways. It is important to understand however that he does not swallow whole the earlier belief in UN procedures as "habit-forming," nor the later "reform school" interpretation of the organization's "civilizing" influences. (We however must swallow something slightly less digestible—that the world is "multipolar heterosymmetrical"—but becoming "multibloc asymmetrical").

Against this background Haas defines the chief strands in the web of interdependence, and here in the main his work is a useful redefinition for the 1970's of that buzzword of the 1960's—interdependence. I will (or rather will not) refrain from recalling to him the several official exegeses by the pre-Vietnam Dean Rusk and Walt Rostow of the same themes of pluralism, diversity, and democratic values.

My own biases were appealed to most forcefully by Part III, where Dr. Haas the observer drops his stethoscope, pronounces the diagnosis, writes a padful of prescriptions, and so to speak jumps into bed with the patient. The most appealing thing of all to me is Haas' willingness to assert clearly and cogently his own intensely humanistic system of values, and on that basis to take policy stands of courage, vision, and in most cases common sense, based precisely on his personal and political commitment to individual worth and political freedom. For the 1970's—indubitably a new time for political and social man—this seems every bit as good a way to redefine the national interest as the Pentagon's classic (and sometimes maladroit) "definition of the threat."

In the political-security-ideological area, his prescription for curing U.S. "over-enmeshment" is a slimming diet of "prudential withdrawal" and a longer term regime of "selective recommitment." His ideas will be called neo-isolationist and even irresponsible by those who still believe in containment and the "responsibilities" of great power. Until, that is, they notice that even while joining the popular cry against unilateral interventionism, he proclaims a positively Wilsonian commitment to "defend the institutions and territorial integrity of democracies everywhere, not only in Europe and Australasia." If I understand him correctly—and I hope I do not—this is a call for precisely the kind of ideological crusade he finds fault with in at least the rhetoric of four Presidents.

But on many another front he says new, fresh, and often exciting things about the proper role of international institutions at this juncture in history. In the process he poses as open questions virtually all the revealed liberal theology of development, trade, aid, self-determination, the UN, peace itself. Hard-hitting analysis is accompanied by imaginative proposals in the vital "functionalist" areas, bearing on the technological encroachments on freedom and the environmental encroachment on life.

On the assumption that my reviewer's union card is in jeopardy unless I pick at least a handful of nits, I do not think: that the Uniting for Peace Resolution has totally "languished from disuse"; that any serious argument has yet been advanced (including here) for a "standing" UN peacekeeping force; that what happened in the disarmament dialogue in 1955 isn't worth mentioning; or that the U.S. Dominican Republic landings took place in 1964. I also wish he hadn't (or rather, I wish he had) tried to square his desire that the UN "enforce" (what?) against South Africa with his earlier assertion that the UN's "primordial task [is] . . . to keep conflict localized."

Ernst Haas gets full marks for yet another work of originality, great power, and high worth. His publisher gets high marks as well for readable type, no obvious typos, and attractive abstract drawings preceding each chapter.

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BOOK NOTES

POLITICAL THEORY, HISTORY OF POLITICAL THOUGHT, AND METHODOLOGY

Power and Leadership in Pluralist Systems. By ANDREW S. MCFARLAND. (Stanford: Stanford University Press, 1969. Pp 273. $7.50)

Pluralists and elitists continue to debate the issue: "Who has power?" This is another way of arguing about who causes things to get done or keeps them from being done; for, McFarland contends, power is really intended causation, and

theories of power are in fact explanatory systems about such phenomena. Explanatory systems, like the corresponding realities they seek to render intellectually manageable and intelligible, may be *complex* or *simple* depending upon 1) the number and variety of components belonging to the system, 2) the amount of relational interdependence among these components (to what degree a change in the state of one component is accompanied by additional changes in the states of other components), and 3) the variability of separate components and their relationships through time (p. 16). Pluralists contend, as McFarland convincingly shows, that the answer to the question "Who Governs?" is *complex*, that "power structures," i.e., systems of intended causation, in most American communities are *complex* and therefore require *complex* systems of hypotheses for their explanation and understanding. Elitists on the other hand are said to view power, i.e., intended causation, in more simplistic terms requiring only *simple* models for successful explanation. To carry this basic contrast further, pluralists view conflict as multilateral, elitists see it as dualistic; pluralists characterize power relationships as symmetrical and see leaders as negotiating brokers, while elitists depict power relationships as asymmetrical, thus permitting dictation and manipulation by leaders. Most importantly, perhaps, pluralists focus on the empirically verifiable activities of different individuals making a variety of choices; while elitists, employing more an historical and macro-sociological approach, focus on the nature and shaping of the structural context that determines the scope of alternatives to which the individualistic choice-making behavior studied by the pluralists refer. On the basis of this latter distinction, it may not be inappropriate to suggest that in fact the pluralists and elitists really are not talking about the same thing, that each is focusing his attention at a different level of analysis and coming up with a correspondingly different set of conclusions that do not so much as clash with one another as bypass one another.

There is something else at issue here besides "who has power?" More to the point: "Who has power over *what*?" This question the author suggests is important to testing for the presence of democracy. Complex systems of intended causality that in fact are restricted in their operation to the level of routine decisions rather than critical ones, while pluralistic are not democratic, for the membership does not possess sufficient power to keep control over the really important decisions. Because popular control over the making of important decisions is the basis for equating pluralism with democracy, according to McFarland (p. 226), where complex systems do not provide popular control over important decisions, we encounter only "spurious pluralism," as the author argues in his own pains-taking review of the literature on decision-making in the U. S. Forest Service and in decentralized Soviet industrial management. But how does one go about deciding on a truly scientific basis what decisions are innocuous and which important? Not just "who causes?" but, again, most importantly, "who causes *what*?" The middle part of the book takes up this issue of distinguishing between incidental and important decisions. Here the discussion is only suggestive, not at all conclusive, and the author does not pretend his effort to be any other than a tentative and exploratory one. At times the going seems to get a bit tedious, but the reader quickly recognizes this as a small price for the author's rigorous exercise in conceptual clarification and stock-taking and soon develops a charitable attitude. Here McFarland draws from the literature of the theoretical fields of games and sets, organization behavior, and structuralism in order to identify relevant concepts bearing on the task of distinguishing important decisions from the unimportant; and giving close attention to their high degree of argeement, he relates these concepts heuristically to one another.

The survey found in the middle part of the book gives the impression that the closer a choice is to determining the range of alternatives that in fact will govern subsequent choices and the closer a decision is to influencing the structural context that will contain future decision-makers, the more important it is. At first glance it might seem that the implications that emerge from the terms of the author's own analysis are more congenial to the position of the elitists who, emphasizing the limiting role of ideology and social structure, contend that control over the important decisions is not widespread at all. But McFarland resists this suggestion. The elitists, he argues, over simplify; they assume before the fact their own conclusions, as when they approach conflict as inherently dualistic. But I do not see that the pluralists too are not guilty of this same behavior from time to time. What, after all, really is the difference between assuming conclusions before the facts and hypothesizing? The really important questions is: Do the facts support the conclusion informed by the assumption that makes the facts interesting and worth collecting in the first place? The difference between assuming conclusions and hypothesizing lies in one's respect for *all* the relevant facts and their *limitations*. On the basis of this test, I am not convinced by McFarland's view of the pluralist model as having a better fit. Whenever I engage in radical activity at the issue boundaries of the system, the "establishment"

does acquire a sudden visibility and cohesiveness, or, at the very least, I suddenly become very much aware of my impotence over the critical decisions affecting my own life. The conditions of activity located well within these boundaries, however, more often conform to the expectations of the pluralist. As the author himself notes, "one can find complexity (and simplicity?) almost anywhere from one perspective or another." (p. 222) But if the test of democracy is widespread control over the *important* decisions, and the important decisions are structural or inclusive in their import, boundary decisions as McFarland implies, then do not the critics of pluralism (Walker, Kariel, Bachrach, Lowi, Bottomore, *et al*) stand on solid ground when they argue against the claim of democracy made in its behalf? McFarland is not unmindful that this question might occur to the reader, and he deals with it in his discussion of leadership in a pluralist system in the last part of the book. There he suggests that the cumulative momentum of the routine or incremental bargaining decisions of pluralist politicians may have long-run boundary or issue scope implications; therefore, these decisions are as critical or important as the innovative decisions advocated by charismatic, anti-establishment leaders (p. 173). In attempting to save pluralism's democratic bone fides, however, McFarland fails to satisfy one very important test of power as causality that he himself insists upon, viz., *intent*. Pluralist politicians do not always *intent* their decisions to possess that cumulative innovative thrust necessary to concluding pluralist politics do adequately organize the people to give them control over those making important decisions. In fairness to McFarland it must be emphasized that he in fact admits structural innovation usually is *not* the goal of politicians negotiating in a pluralist setting, but this concession hardly serves to change the facts of the matter.

McFarland's emphasis on democracy as individual control over *political* decision-makers and his preoccupation with leadership phenomena are also reflective of what might be considered serious shortcomings of a discussion of pluralism, elitism, and democracy. For to some, democracy involves the making not only of political decisions, but of other kinds as well, with participation valued not only as a mechanism of control, but more importantly as a means to individual growth and change through the discovery and development of the public self. Pluralist theory examined through these perspectives on democracy would focus not on leadership but rather on *citizenship*. In the absence of these perspectives, the likely possibility that pluralism is a variant of elitism remains unexplored.

Notwithstanding these criticisms, this book *does* contain some valuable theoretical stock-taking, conceptual clarifications, and many analytically rich and illuminating illustrations. In regard to these attributes and the spirit of balance and fairness that pervades the author's formulations, it sets a standard of scholarly sophistication pluralists and elitists would do well to observe as they continue their debate.—DARRYL BASKIN, *Windham College*

Beyond Nihilism: Albert Camus's Contribution to Political Thought. By FRED H. WILLHOITE, JR. (Baton Rouge: Louisiana State University Press, 1968. Pp. 225. $6.50.)

Professor Willhoite admires Camus and therefore places the latter's political views in the most favorable possible light. The result is a book which is useful and interesting even to those who, like this reviewer, feel that the author has assessed "Camus's contribution to political thought" too generously.

By referring in the subtitle to Camus's "political thought," Professor Willhoite seeks to sidestep the question of whether Camus deserves to be called a political philosopher, but it is clear that he is tempted to answer that question affirmatively. Thus he does not hesitate to attempt what may be called a philosophical reading of all of Camus's works—including the plays and novels. There is certainly much to be said for such an attempt, for it would be arid pedantry to argue that one cannot find wisdom about things political among the poets. In the case of Camus, however, one may well wonder whether it might not be equally fruitful to begin by reading all of his Works poetically—including *The Rebel*, which seems to present itself primarily as a kind of noble rhetoric, a passionate call to political decency.

Whether or not Camus can be called a political philosopher depends in part on the extent to which his thought is more than simply derivative from earlier thinkers. It would therefore be worthwhile to determine how much Camus took from, and understood of, Rousseau, Marx, and especially—in a book called *Beyond Nihilism*—Nietzsche. Professor Willhoite has not attempted to do this fully. He has, to be sure, summarized Camus's estimates of his illustrious predecessors, but basically the author has chosen to illuminate Camus's political thought by comparing, confronting, and contrasting it with the ideas of contemporaries like Buber, Sartre, Freud, Marcuse, Arendt, Oakeshott, Michael Harrington, George Kateb, and others.

The author employs this procedure skillfully enough to permit a fair and fairly comprehensive picture of Camus's politics to emerge. He views Camus as a thinker who employs a profoundly

existential philosophical method—he eschews "*a priori* concepts and categories"—to "attempt to overcome the contemporary threat of nihilism by reestablishing norms for human conduct in the political realm." Such an attempt presupposes that contemporary man confront "the world of absurdity," which exists because of "the radical disjunction between the natural world we perceive every day and the inescapable perspective of modern science," as well as because of "the disjunction between man's desire for unending happiness and the inescapable fact of death." But "the world of absurdity" is merely the starting point of Camus's analysis. While it cannot be fully overcome, it can be transfigured; to contend against absurdity is not itself absurd. The genuine rebel will fight against all that degrades and alienates man. His rebellion will be in the name of man's common humanity and for the sake of "what Camus considered the highest human good—dialogical life in communion." Camus, of course, was well aware of the fact that few revolutionaries and radicals are genuine rebels in this sense. Therefore, he engaged in a trenchant critique of ideologists who wish to play God in human affairs, absolutists who certainties do violence to the fundamental ambiguities of the human situation, terrorists who perpetrate unspeakable cruelties in the pursuit of unattainable (or even attainable) ends, and utopians who fancy that all human problems are amenable to political solutions. Camus was a steadfast opponent of all totalitarianism, whether it emanated from the Right or the Left, but he never blinded himself to the grave shortcomings of the Center. Above all, he was very much his own man in politics, as is perhaps best exemplified by his courageous refusal to adopt a simplistic position in regard to France's involvement in Algeria.

Such a bare summary scarcely does justice to Professor Willhoite's exposition. Suffice it to say that the author dwells with suitable enthusiasm on the assets of Camus and that, conscientious as he is, he enables one to see Camus's limitations as well. Unfortunately, there are a number of limitations to see, only two of which can be mentioned here. For one thing, Camus's thought appears as curiously abstract, coming as it does from someone who opposed the abstractions of traditional political philosophy. What, for example, is one to make of the central concept of dialogical communion? One cannot be sure that it is anything more than a vague compound of friendship, love, and the fact that man is by nature a political animal. A more fundamental objection is that it is hard to see how Camus got "beyond nihilism" in any real way. Nietzsche might well have accused him of having recourse to moral principles rendered groundless by the death of God and the concomitant loss of all transcendent standards; it is hard to frame an effective reply on Camus's behalf to this hypothetical charge. Thus Camus spoke of the need of *creating* values; apparently he was insufficiently aware of the fact that the meaning men must create is a paltry substitute for the meaning which, while it may be discovered, is beyond creation by man.

Nevertheless, Camus was an admirable man. One cannot but agree with Professor Willhoite when he insists that today's social science, and social scientists, can learn a good deal from this man.—WERNER J. DANNHAUSER, *Cornell University.*

John Locke: Problems and Perspectives. A collection of new essays. EDITED BY JOHN W. YOLTON. (New York: Cambridge University Press, 1969. pp. 278. $9.50.)

This volume of essays is remarkable for its high standard of scholarship. Most of the contributions reveal an awareness of the fact that Locke wrote several drafts of his more important works and made revisions in later editions of them. This attention to the internal detail of Locke's intellectual development is complemented by a familiarity with the context of that development in terms of his correspondence, associations and the significant philosophical and political issues of his time. The result is not a series of interpretations which attempt to establish that Locke was a philosophical or political radical driven by an *idee fixé*. Rather, the Locke that emerges from these studies seems much more conventional and traditional—a significant thinker and actor in his time who articulated certain views which transcended the particular situation for which they were formulated, but by no means a devious writer who consciously set out to be a mover and shaker on a world-historical scale. It is this quality of these essays which perhaps justifies the editor's description of them as "new". For after the numerous recent interpretations and reinterpretations of Locke, it is refreshing to read a volume which for the most part, is not reinterpretation but re-presentation of his life-world.

The contributors address themselves generally to three areas of concern. Yolton's essay, "The Science of Nature" and von Leyden's on "What is a Nominal Essence the Essence of?" are concerned with the tenets and problems of Locke's epistemology. There are several historical essays, dealing with events and intellectual currents of Locke's time, such as Axtell's "Locke, Newton and the Two Cultures" and a contribution by Esmond De Beer in which he discusses the contemporary social and political issues and concludes that Locke's political views antedated

their publication in the *Two Treatises* although they were expressed in more topical form in the published draft due to the revolutionary setting. An essay by John Dunn argues for the relatively diffuse influence of Locke's ideas on English and American politics in the eighteenth century and Peter Laslett deals with a specific historical event in his essay "John Locke, the Great Recoinage, and the Origins of the Board of Trade: 1695–1698."

Of most interest to political theorists, however, are those contributions which deal with Locke as a political philosopher. In this category, the first essay by Raymond Polin on Locke's concept of freedom, is rather disappointing in terms of the fruitfulness of a close textual analysis. Polin first considers metaphysical freedom and then analyses Locke's views on social and political freedom, concluding that for Locke freedom is politically meaningful only within the context of morality, rationality and plurality. Moreover, such a view is said to lie "at the bottom of any true and efficient liberalism". Such a conclusion is both obvious and banal; it is a point of departure for the critical examination of the extent to which Locke's specific political recommendations give a distinct content to this doctrine so that it is truly made politically meaningful. Did Locke, for example, propose great freedom differentials correlated with rationality differentials? Such questions are not considered and therefore the essay is inadequate for the amplitude of its topic. Another essay by Seliger on "Locke, liberalism and nationalism" suggests that Locke's liberalism adumbrates a rationale for nationalism and that the most important aspect of this is Locke's treatment of the phenomenon and form of consent. This essay is well written and the connection between liberalism and nationalism—two doctrines often considered antithetical—is well documented and soundly argued.

There are two essays, by Gordon Schochet and Hans Aarsleff, which are exceptionally valuable. Schochet discusses the relationship between paternal and political authority in Locke and makes a strong case for the view that Locke saw a historically contingent coincidence of them which was quite distinct from Filmer's genetically based theory of obligation. Locke saw the necessity for the presence of consent and trust between ruler and citizen. This essay is very well done and shows how an apparent similarity of approach between Filmer and his critic can easily tend to obscure the fact that, for Locke, a patriarchal derivation of political obligation is normatively irrelevant in addition to being scripturally questionable." Aarsleff's essay on "The State of Nature and the Nature of Man in Locke" is concerned to argue that there is reciprocity between a prudential and a moral basis of obligation. The basic point is that Locke's account of man in a state of nature is based on a view of man as essentially rational but requiring the use of pleasure and pain by a providential God to ensure that man learns the law of nature as well as observing it as best he can. Since God is the author of man, of the law of nature and of the pleasure and pain man experiences, there is no dual account or justification of human behavior in Locke.

Richard Ashcraft's essay on the relationship between faith and knowledge is an excellent documentation of Locke's fundamental commitment to Christianity. Ashcraft disputes and effectively disposes of the view that Locke's *Essay Concerning Human Understanding* and *The Reasonableness of Christianity* were both intended, although in apparent opposition, to discredit the gospel. Rather, Locke emerges as a devout believer for whom faith is required to remedy the defects of philosophy. This conclusion is related to an egalitarian imperative which Ashcraft mentions but the implications of which one wishes he had pursued.

The most delightful essay in this volume is by Rosalie Colie. Analysing the style of Locke in his *Essay*, Professor Colie utilizes a phenomenological methodology which is sensitive to the nuances of words and phrases as they mirror and focus the process of thought of which they are a part. This essay, partly because of its subject, is an esthetically pleasing high-fidelity representation of Locke.

The concluding piece by Hans Aarsleff considers the quality of recent Locke scholarship and contends, among other things, that such scholarship has been much too concerned with psudeoproblems. Once can heartily agree. But this leads one to note the deficiency of this volume of essays: it lacks critical political thrust and relevance. As an academic exercise it is generally superior to most collections and many of its contributions are intellectually stimulating. One suspects, however, that many political theorists will not be completely satisfied with its offering. There is a notable omission of consideration of problems such as those raised by C. B. MacPherson. Who are the people? Who are rational? What is property? Some serious consideration of these problems might have livened up the action.—KEN RESHAUR, *University of Manitoba.*

Technological Man, BY VICTOR FERKISS. (New York: George Braziller, Inc., 1969. Pp. 336. $7.95.) *Values and the Future.* BY KURT BAIER AND NICHOLAS RESCHER. (New York: The Free Press, 1969. Pp. 527. $14.95.)

These two books provide excellent examples of what happens when, on the one hand, as in the Ferkiss book, a single individual attempts to

encompass all that is significant about a topic in the work of a great number of writers, and on the other editors, such as Baier and Rescher, set out to build a theory which, it is hoped, will be exemplified in the work of a selected set of experts each of whom presents his own findings.

Ferkiss demonstrates a mastery of a very wide range of literature and is able to distill the major contributions of its authors. He presents no methodological or theoretical framework upon which others may build. Baier develops a theory of the ways values may be studied so as to bring them into the purview of science. Some of the writers seem to have utilized his framework in preparing their own chapters, while others show little or no apparent need to resort to it in making their presentations.

The extent and variety of the materials presented show wide concern about what technological change has done or is doing to our culture and to our future values. The student is provided with excellent bibliographies in both books.

Ferkiss is intent on demonstrating that Technological Man is both real and mythical. He shows how new techniques of this age have had a major impact on some elements of modern life. Juxtaposed also is the evidence of continuity in much that men feel and think. Conflict may occur at the level of social interaction or be internalized, but conflict is not regarded as being necessarily pathological. For Ferkiss the central danger is not that man will, in response to the demands of technology, abandon old values and institutions, but rather that in the effort to preserve elements of the past he will prevent the use of technology which would make it possible to achieve a life far superior to that which men shared at the time when many of our institutions and our values were developed.

He holds that the rise of the nation-state was accompanied by that of science and technology and the two were mutually dependent. It was feedback from technological achievement that energized the nation-state and produced the power to overcome its agriculture based forerunners. The new civilization produced a rootless, nomadic man, often alienated from loyalty to the major institutions through which he must survive. This freedom from the ecological limits which characterized earlier man requires the creation of a new discipline, new institutions, and a different set of priorities than that sustained by the culture of the past. Some see the new system as totalitarian, destroying freedom. Ferkiss emphasizes the checks and balances which grow out of the diverse sources of power as a means through which the freedom technology makes possible might in fact be realized. However, one great barrier must be overcome: the expansion of population must stop or only a few, if any, of the world's peoples will ever turn the myth of technological man into reality. The probability that this stability in population will be achieved remains moot.

The title of Baier and Rescher's work is "Values and the Future" and they are concerned not only with such value change as can be traced back to technology but with all of the sources of such change. Baier heads a project designed to examine the roots of change, using a theory which he has developed. Rescher utilizes it extensively. The work of other contributors reflect its use in varying degrees. The position taken is that the prediction of values in the future is not only possible but absolutely necessary. Man can choose whether or not to use science or to depend on faith, hope, or blind trial and error, but in any case he has to assume that values will continue to have real effects.

Baier abandons hope of finding in any object "intrinsic" value. Rather value is an expression of relationship between various objects and human beings (whether treated individually or collectively). Sets of objects are imputed to have value because they contribute to some large generalized conception of the good life and can therefore be objectively measured on the basis of their contribution to or interference with the achievement of these generalized goods. To discover values then we need to observe people and the responses they give (putatively or in real time) when confronted with given sets of objects in given sets of conditions. To test the degree to which a particular object or activity serves in this way, we must resort to the findings of psychology, sociology, anthropology, economics and numerous other disciplines. Presumably, we can, when the evidence is in, make an objective assessment of which values ought to persist or be enhanced and which should be given a lower priority. The rest of the chapters in this book provide evidence of various kinds from some of the necessary disciplines, but there are others which will have to be included if the model is to be explored fully. Even these chapters demonstrate the paucity of hard facts that can be used to show just how specific technological changes led to specific alterations in the values men held or pursued.

Rescher develops the theory further by showing that "value change" may mean several different things, and the means of demonstrating them may require the use of different instruments. Both the primary authors demonstrate far more familiarity with "values" than with the numerous kinds of technological change to be related to value change. The consequences which follow from altering the biological and physical

worlds constitute hard facts which must be given a place in any assessment of the future. We can hope that the promise which the use of the proposed theory holds out will be sufficient to attract the means to support or reject it.

Two kinds of evidences of the substantive changes man is likely to encounter in the future are also presented in this work. One uses the "Delphi" method in which outstanding authorities are asked to present lists of what each considers to be the major changes man will face in the future. These lists are then compared and a summation drawn showing the trends upon which the experts have evidenced a given degree of agreement. The other method utilized here required the various contributors to participate in a simulation, with the computer helping to "predict" what would be the effect of various decisions. Both these methods have been and are being used by others and need no further comment here.

Finally there are a series of chapters in which each contributor makes his own assessment of what has been going on in the world and what is likely to result from it. As usual these vary in their quality and in the means used for assessment. Almost all the authors have written extensively and the positions they take here are not significantly different from the stance a reader would expect. The advantage for the student is that their findings are presented side by side in summary form so that he can more easily compare them. Both books are stimulating and thought provoking.—W. FRED COTTRELL, *Miami University.*

Zwischen Reformismus und Bolschewismus. Der Austromarxismus als Theorie und Praxis. BY NORBERT LESER. Europa-Verlag, Wien-Frankfurt-Zürich, 1968. Pp. 600.)

Within the almost limitless literature on Marxism there has been little systematic discussion of that peculiar and fascinating variety of it, Austromarxism. In fact the question has been raised whether the term itself has any merit and whether there is anything specifically "Austrian" about the phenomenon concerned. Leser's book tackles this phenomenon with knowledge and insight. The author is a young Austrian who knows it only from historical studies and recent conversations, but this precisely gives him the advantage of perspective. He plans to supplement this work by a second volume on "Austromarxism as a Conception of the World and of Life."

The organization of the present volume is rather unusual. Part I deals at length with the two pivotal figures in Austromarxist theory and practice, Karl Renner and Otto Bauer. Part II, entitled "Unity at Any Price", discusses the formative processes of Austromarxist thought and policy from the 1860's to the 1930's. Part III is concerned with the social philosophy of Max Adler, that lonely figure on the fringe of Austromarxism. The three parts do not hang together too convincingly, yet add up to an overall view as a result of the author's many fine psychological insights.

His own position and conclusions are made clear almost from the beginning: "not that Austromarxism played, in the destruction of Austrian democracy, a part equal to that of the forces active in such destruction: but that it merely facilitated, rather than hampered, these activities through its own tactics" (p. 21). Austromarxism succumbed time and again to "the temptation to project desired and accepted values into the analysis of a given historical situation and thus to prejudice its analysis, even to put into question its guiding function, and at the same time to supply with the help of a political theory a later justification for specific judgments and decisions" (p. 26).

Leser's sympathies are clearly, though by no means uncritically, on the side of Karl Renner with his belief in socially-minded democratic processes, tolerance, human rights, an application of the democratic principle to the economic scene, an affirmative interpretation of the state, and a pragmatic approach to socialism. Nevertheless Renner considered himself a Marxist who merely wanted to humanize the class struggle; but Leser feels that Renner actually had more in common with Hans Kelsen than with Karl Marx.

Otto Bauer, on the other hand, took over from the founder of the Social Democratic Party, Victor Adler, the vision of a revolutionary final struggle at some future date along with hesitation in concrete crisis situations to make full use of the powers of the movement. To some extent Leser traces the contradiction between revolutionary theory and a hesitating practice back to Marx and Engels themselves, but to his mind all these earlier contradictions were compounded and systematized by Bauer. His strong humanistic and libertarian feelings prevented him from accepting the Bolshevik interpretation of revolution and proletarian dictatorship, yet he considered Bolshevism a great revolutionary phenomenon. As political leader he himself showed a "tendency to non-action which sought a refuge in Marxian theory" (p. 135). He neglected the subjective factor in politics in favor of objective influences which invariably impeded action. Here again he merely brought out a belief in historical necessities which basically was present in Marxism itself. From this there followed a pattern of self-deception which was bound to result in defeat.

A brief section on the origins and changing

meaning of Austromarxism brings out the fact that only before the first World War was there real intellectual cooperation of its leading thinkers. Leser gives only passing attention to Rudolf Hilferding, a more adequate treatment to Friedrich Adler. Helene Bauer, Otto's wife and an important influence in her own right, is not mentioned at all. Leser points out intelligently the non-revolutionary personality traits of most of the leading figures even when they used revolutionary phraseology, the legacy of the unity urge resulting from the initial split in the 1880's, and the quasi-religious nature of many Austromarxist beliefs.

We can only mention here the lengthy historical part of the book, which is not aimed at factual completeness but at analysis of the various political and ideological phases. Leser discusses especially the elements of individualism and collectivism in the movement, the frequent tensions between reform and revolution, the expectation of an inevitable crisis of class society when the time is ripe and not before, and the strange mixture of genuine convictions and tactical considerations. Leser claims that "the existence of a tactical reason supporting non-action was used with relief as an opportunity to evade action and to prevent the revolution professed as a principle from ever becoming a serious matter in society" (p. 216). "The fixation upon the future relieves one psychologically from the necessity to act in the present" (p. 237). There was a "socio-psychological circuit": the leadership waited for the masses to act, but when they did they were told that the objective conditions were not right.

When the Republic was established in 1918, more as a result of the military and ethnic disintegration of the old empire than as a result of Social Democratic action, the initial coalition with the Catholic forces did not last long. The 20's were characterized by the political contrast between the Marxist Otto Bauer as leader of one party and the Prelate Ignaz Seipel as leader of the other, and the contradictions discussed gave Seipel the opportunity to increasingly weaken the Social Democratic position. Dollfuss' creeping Fascism and the civil war of February, 1934, marked the end of the old Social Democracy, though it was to emerge in new forms after 1945 in an essentially non-Marxist version.

In attempting an immanent critique of Austromarxism Leser comes to the conclusion that "Austromarxism in all of its forms was not really a revolutionary theory and practice and cannot therefore be characterized as a Marxist force in its basic meaning" (p. 496). But "even though it has perished as a phenomenon of reality, it is indestructible and effective as *Idealtypus*" (p. 507).

The last part of the book discusses the social philosophy of Max Adler who tried to combine and re-interpret Kantian and Marxian thinking, especially in his concept of a "social *a priori*" in the human mind. Leser points out the religious element in Adler's thinking, his "materialistic interpretation of history without materialism" (p. 529), and his combination of all these elements with extreme left-wing politics within Austromarxism.

This is an important book for the study of political philosophies and their implementation. It is interesting to observe the influence of the non-Socialist Kelsen on Leser's brand of socialist thinking. In a future English edition, which would be very desirable, the "endless" German sentences will undoubtedly be broken up and repetitions will be eliminated. More important, the psychological roots of the great self-deception under discussion, and the differences in basic personality between Renner and Bauer might be treated more fully. Leser might also reconsider his interpretation of Bauer's mentality and doctrine as the real villain in the piece; why did the others follow his leadership to the bitter end? Perhaps we shall find some answers in the second volume promised.—ALBERT LAUTERBACH, *Sarah Lawrence College.*

Democracy and Totalitarianism: A Theory of Political Regimes. BY RAYMOND ARON. TRANSLATED BY VALENCE IONESCU. (New York: Frederick A. Praeger, 1969. Pp. 257. $7.50.)

This volume is a collection of 19 lectures delivered by Professor Aron at the Sorbonne in 1957–1958, and published in France in 1965 under the title *Democratie et Totalitarianisme.* None of the essays has been revised or updated and, as the author himself concedes in the introduction written for the original French edition, the book "bears the inevitable traces of the lecture room and of improvisation" (p. vii).

Readers who are well acquainted with Aron's general works on politics and society, particularly his *The Industrial Society and Progress and Disillusion,* will therefore find little that is new or interesting here, except for what is contained in the few essays devoted explicitly to his "theory of political regimes." On the whole, this is a fairly representative sampling of a wide-ranging, stimulating and polemical mind. As usual, Aron is at his best in synthesizing the ideas and concepts of other writers; he glides effortlessly—though often too effortlessly—over the writings of diverse theorists, from Aristotle to Montesquieu to Pareto. And, characteristically, he is at his weakest when it comes to political analysis, which is what the core of this book is supposed to be. A major part of the trouble stems from the author's inability to calm his passion for the blessings of the new

technocratic liberalism which has presumably come to Western Europe for good, destroying in its wake the old, anachronistic left-wing"utopias."

The basic task which Aron sets for himself is to analyze political regimes in modern industrial societies. After defining politics as the arena in which individuals or groups, each having divergent policies, aims and interests, come to grips, he plows through three lectures of discourse on nearly every general aspect of political life before he gets down to the business of constructing a typology of modern political systems.

All regimes operating in highly industrial settings are divided by the author into constitutional-pluralistic and monopolistic types, a scheme roughly equivalent to the once-prevalent cold war dichotomy of democracy and totalitarianism. (Despite the title of his book, Aron never gets around to discussing, let alone defining, the concept of totalitarianism until page 193.) Pluralistic systems are those in which "the peaceful rivalry for the exercise of power exists constitutionally" (p. 41); the "spirit" or logic (montesquieu) of such regimes is legality and compromise. Monopolistic regimes, of which the Soviet Union is the clearest and most advanced type, are characterized by the monopoly of power by a single party and by the absence of tolerance for opposition; their "spirit" is faith and fear. The "antithesis" between pluralistic and monopolistic systems is explained according to four criteria: (1) competition vs. monopoly; (2) constitution vs. revolution; (3) pluralism of social groups vs. bureaucratic absolutism; and (4) state with parties (secular state) vs. party-state (ideological state). Of course, these are highly normative categories. The major difficulty, however, is Aron's failure to provide us with some set of empirical indices for distinguishing existing regimes according to these categories. For example, how do we know when a given political system ceases being monopolistic and revolutionary and becomes competitive and constitutional? We are given no clues for this, primarily because Aron is not interested in formulating propositions that are amenable to empirical verification. Other problems arise when we consider the fourth criterion. Here we are asked to believe that monopolistic regimes are the only ones that behave in accordance with "ideological" precepts or, as Aron puts it, that "the renunciation of violence becomes, so to speak, the ideology of the non-ideological [i.e., pluralist] state" (p. 236)—an astounding statement for a Frenchman to make at the peak of the Algerian war.

The vague and haphazard quality of Aron's framework becomes all the more visible as we examine more closely the respective types. Pluralistic systems can be compared in terms of five "main variables"; constitution, party system, functions of parliament, pressure groups, and leadership. How these variables are derived, and the way in which they relate to the four criteria, is not made clear. At other points, Aron mentions the role of "social infrastructure", "historical environment," and "national psychology" in the analysis of pluralistic regimes, but these are actually residual categories which are never integrated into any analytical scheme. Aron does not attempt to construct subtypes of pluralistic regimes based upon these or any other categories, though he does wrestle with the problem of developing subtypes for monopolistic systems. The Soviet Union, Nazi Germany, and Portugal are seen as three main variants of monopolistic regimes, which vary according to doctrine, scope of goals, violence of methods, and ideal image of society. There is some ambiguity here also—Aron is not really sure how or even whether the third type actually fits in—but at least he shows less inclination than most theorists of totalitarianism to lump together Fascist and Communist regimes.

Perhaps the strongest section in *Democracy and Totalitariansim* is the one that is furthest removed from Aron's formal scheme: his study of the "corruption" (degeneration) of the French Fourth Republic. The corruption of any regime develops where there is too much oligarchy, and hence the loss of legitimacy, or too much "democracy," and hence loss of efficiency. In postwar France, of course, the balance persistently swung toward the latter, toward immobilisme, which Aron silhouettes with some very good historical analysis that has little to do with his framework.

The chapters on monopolistic regimes are almost exclusively devoted to Soviet politics. Included are essays on the Soviet constitution, terror and ideology, totalitarianism, and an assessment of various theories of the Soviet system. Particularly annoying is Aron's tendency to make generalizations about Soviet politics, Communist systems, or monopolistic regimes that are based upon the historically specific phase of Stalinist development in the USSR. And then there is the old and tiresome game of pointing up the inconsistencies between classical Marxist theory and Soviet political practice. Certainly there is nothing any longer puzzling about the primacy of politics in Soviet development; but rather than dwell upon the triumph of voluntarism over the "doctrine of determinism," Aron might have proceeded ahead with an analysis of the evolution of the party within the larger context of the transformation of Soviet society. Here, as with the pluralistic regimes, we find a very cavalier attitude towards conflict and change, and there is never more than a very cursory attempt to link up economic, social and cultural conditions with the processes of political development.

The subtitle to Aron's book is misleading. What we have is not a "theory of political regimes," but rather a simple typology of modern political systems that is supplemented by a series of loosely-connected and theoretically unrelated essays.—CARL E. BOGGS, JR., *Washington University, St. Louis*

AMERICAN GOVERNMENT AND POLITICS

Party, Constituency and Congressional Voting: A Study of Legislative Behavior in the United States House of Representatives. BY W. WAYNE SHANNON (Baton Rouge: Louisiana State University Press, 1968. Pp. 202. $6.50.)

In terms of number of studies undertaken, the literature on congressional voting based on roll-call analysis is already extensive. But in this new roll-call analysis of four House sessions in the Eighty-sixth and the Eighty-seventh Congresses (1959–1962), Professor W. Wayne Shannon provides a substantial addition to our knowledge in this area. Although based on relatively straightforward research techniques, the book reflects throughout a meticulous attention both to method and to research strategies designed to produce useful substantive findings. And more than most such studies, Shannon has made a genuine effort to explore, confirm, or reject hypotheses raised by earlier roll-call studies, and to build cumulatively on the established findings of earlier work.

Shannon first reviews four major congressional voting studies—A. Lawrence Lowell, Julius Turner, Duncan MacRae, and David Truman. (His examination of the methodology Turner employed in *Party and Constituency* is one of the best such treatments to appear thus far.) He then explores much of the existing congressional voting literature in search of "strands of continuity"—cumulative and generally consistent findings concerning: (1) descriptions of the congressional parties, especially their voting cohesiveness, the frequency with which the parties differ, how much, and on what kinds of policy issues; and (2) factors such as constituency characteristics that are associated with congressional voting.

Shannon then examines party differences in the Eighty-sixth and the Eighty-seventh Congresses. The roll calls were divided into three categories: "modified party votes" (75% or more of one party versus 75% or more of the other party); "majority opposition votes" (majority of one party versus a majority of the other); and party agreement votes (majorities in each party voting the same way). He then explores the most common policy content of the differing types of roll calls. The modified party votes, those exhibiting the sharpest inter-party cleavage, occurred overwhelmingly on domestic economic issues. Party opposition votes also were concerned predominantly with domestic economic, regulatory, and welfare measures. Most major foreign policy roll calls fell into the party agreement category, as did all important civil rights bills and civil liberties votes during this period. Seventeen Guttman scales, constructed for nearly all Congressmen and covering a variety of issue areas, supplement and expand the policy analysis.

Shannon then attempts to examine the potency of the "conservative coalition" (votes on which a majority of southern Democrats and Republicans voted against a majority of northern Democrats). His major finding is that in 1959–1962 southern dissidence from policy positions taken by the majority of congressional Democrats had increased markedly since the four sessions (1933, 1937, 1941, and 1945) analyzed by the late V. O. Key.

Shannon next explores the relationships between constituency characteristics and party control of House seats, and between constituency characteristics and intra-party voting differences. To what extent do special constituency characteristics seem to "explain" the votes of Democratic or Republican Congressmen who do not go along with the majority of their party? As earlier studies by Turner, MacRae Truman, and Froman have suggested, Shannon concludes that this type of analysis indicates only that there are "some relationships at some times" between constituency characteristics and intra-party differences. The findings are complex and sometimes conflicting. They suggest very few straightforward relationships between intra-party differences and constituency characteristics; but at the very least, he concludes, they indicate that the congressional parties are far more than simple reflections of constituency differences.

Shannon then analyzes a problem that has attracted considerable attention in past studies of congressional voting behavior—the relationship between electoral margins and the Congressman's party voting and/or policy choice. He relates his findings to earlier work by Froman and Huntington, and concludes that outside the South in 1959–1962 the differences between the parties were greater where electoral competition was less severe.

The foregoing summary gives some indication of the terrain Shannon has explored, though it by no means does justice to his actual findings. By building cumulatively and comparatively on other work, he has made a number of firm sub-

stantive contributions. He has also pointed up some areas where the data, and findings, are confusing and sometimes conflicting. Perhaps there are several ways out of this dilemma which confronts roll-call studies generally. First, greater attention to the time dimension. Shannon shows, even using only the fragmentary, disjointed evidence currently available, that patterns of party cohesion and party opposition have varied widely over time since the nineteenth century. As he aptly suggests, earlier roll-call vote studies, like early electoral behavior studies, may have been timebound. He makes the interesting suggestion that there may be systematic relationships between major electoral realignments and congressional party voting cohesion. Clearly a strong case can be made for a comprehensive study of congressional voting patterns over time, something existing research technology now makes possible.

Second, Shannon advocates closing a gap that is sometimes noticeable in varying approaches to the study of the legislative process. In effect, he calls for a common meeting ground between "congressional insiders" and those researchers who stress relationships between Congress and the political system as a whole. "Congressional insiders" become knowledgeable, by detailed observation and sometimes by direct participation in legislative work, in the day-to-day intricacies of the Congress. They stress log-rolling, the personal influence of colleagues, other forms of bargaining behavior, the norms of the legislature, actions of formal party leaders, activities of the executive, the perceptions and attitudes of the members themselves, and many other factors—all elements of the legislative process that can be important to legislative outcomes, *and* are very difficult to quantify.

The "congressional outsiders" have relied more heavily on roll-call votes, and, while coming up with some conflicting findings, have also made major contributions to our understanding of Congress in the last two decades. Shannon relies heavily on roll-call analysis. But he also shows sensitivity to the importance of other factors that could sharpen and expand the insights that roll-call analysis, as it has usually been conducted, can provide.

There is "little doubt," Shannon declares, that "some of the most important questions dealing with the congressman, his party, and his constituency can only be settled by other modes of inquiry." He calls for a more sophisticated concept of constituency, embodying the notion of "the constituency" as the Congressman sees it. He also advocates a survey approach to Congress that focuses attention on the representative, his own perceptions of his constituency and issue preferences, and his concept of his role in the representative process—conceptual concerns that may in part be answered by the forthcoming book on *Representation in the American Congress* by Warren Miller and Donald Stokes.

It is Shannon's hope that further inquiries that blend the concerns of the congressional insider and the congressional outsider will help "explain much that remains mysterious in the aggregate data-roll call studies." If his book helps stimulate further research approaches of this kind, it will contribute even more than it already has as a highly creditable example of roll-call analysis.—MILTON CUMMINGS, *The Johns Hopkins University.*

Pedagogues and Power: Teacher Groups and School Politics. By ALAN ROSENTHAL. (Syracuse: Syracuse University Press, 1969. Pp. 192. $3.50, paper.)

Professor Rosenthal has written a book which is both "timely" and of more lasting significance. The "facts" in the book should (although, unfortunately, they probably will not) come to the attention of school board members, teachers, and administrators. The theoretical underpinnings and the interpretation of the facts should (and, fortunately, probably will) come to the attention of social scientists. In short, this is a good book which is a pleasure to read and to review.

The book is concerned with the role of teacher organizations in the formation of educational policy. Given this particular objective, Rosenthal could have labored through the wealth of propositions which exist in the literature of interest groups. Instead, he relys more upon a common sense approach which, while making the book less pretentious, does provide some tentative answers to the questions which the author seeks to answer. He wants to know how effective teacher associations are and, if they are effective, why. Rosenthal suggests a variety of ways of exploring the question. He analyzes interviews with leaders of various teacher organizations and he provides an anthropological-type description of particular educational settings in New York, Boston, and San Francisco. These cities are a sub-set of the larger sample from which the interviews with leaders of teacher organizations were drawn.

Before discussing some of the more intriguing findings of the book, let me say something negative—so that the traditional responsibilities of the reviewer may be justified. It seems to me that the devices employed by Rosenthal to explain the effectiveness (or lack thereof) of teacher organizations are not especially helpful in organizing the data or in explaining what they are supposed to explain. Briefly, he suggests that organizational strength, organizational opportunities, organizational behavior, and organizational influence are the independent variables upon which he intends to rely. In assessing the net contribution made by these variables, I am forced to conclude that the

book could have been just as well done without them. But, so what? Even if the explicit formulation of a theoretical framework does not satisfy this reviewer, the implicit assumptions which guided the research seem quite adequate. In other words, I think Rosenthal has done an excellent job in explaining the relative strength of educational associations.

He begins his description of teacher organizations with a brief history of the clash of doctrine between the dominant National Education Association and the smaller, but more rapidly growing American Federation of Teachers. He is especially concerned with what has become to be known as the "new militancy" among teachers and addresses himself to the question of which organization is, in fact, more representative of this new militancy. His conclusions are that the American Federation of Teachers, as we would expect, is more militant but that, probably as a consequence of the growing strength of the AFT, the National Education Association has revised its conservatism to the point of encouraging political activity among teachers. However, even if the official ideology of the two major teacher organizations is becoming more compatable, Rosenthal's data suggest that the attitudes and behavior of leaders of the organizations has not caught up with the party lines.

In an especially well done chapter, Rosenthal examines the motivations of the leadership of the AFT and NEA local affiliates in New York, Boston, San Francisco, and Atlanta. He correctly asserts that the question of the extent to which teachers shall share in the determination of educational policy is at the heart of the new militancy. On this particular dimension, Rosenthal concludes that "leaders belonging to AFT are far more disposed than their Association counterparts to prescribe for their own groups a major or equal voice in the formation of public school policies." Why are AFT leaders more militant? Traditionally, a variety of explanations has been offered. Others have argued that AFT affiliates are more attractive to men and younger teachers, for example. However, Rosenthal asserts (by the inclusion of appropriate controls) that the most important factor contributing to a militant outlook is organizational affiliation, irrespective of other confounding variables. Organizational socialization, we might conclude, is sufficient to overcome whatever personal or sociological factors might contribute to or detract from a militant attitude. Consequently, a shift in the NEA ideology has had virtually no impact upon its local leadership. Basically, the NEA emphasizes the unity of the educational enterprise and allows administrators to hold membership in the organization. The AFT, on the other hand, stresses the inherent conflict between teachers and administrators. The traditional differences between the organizations appear to have been so sufficiently established that recent modifications in NEA ideology has simply not filtered to the local level.

When Rosenthal addresses his efforts toward an assessment of the effects of the new militancy upon the government of education, he concludes that teacher organizations are at the bottom of the rung with regard to power in educational decision-making. No matter what the ideology of the leadership, the organizations play a negligible part in determining school policies. They are substantially weaker than mayors, school boards, superintendents, or "whoever happens to dominate in a particular policy domain."

Interestingly, there seems to be some association between the relative strength of teacher organizations and the strength of the superintendent. In Chicago, where the superintendent clearly dominates, teacher organizations are weakest. There is some support, then, for the idea of an inherent conflict between teachers and administrators, which will no doubt become exacerbated as the militancy of teachers increases. For instance, the single exception to the rule of the impotence of teacher organizations is the United Federation of Teachers in New York in the single domain of salary negotiations. However, no teacher organization seems to have been able to achieve much in the area of curriculum, which is especially ironic in view of the fact that teachers assert that they are professionals. If they are professionals, then surely they ought to be able to influence if not control the substance of the courses they teach. However, they cannot. If Rosenthal is correct in his conclusion that organizational affiliation is the single most important factor contributing to militancy, the crucial problem becomes one of assessing the extent to which the American Federation of Teachers will continue to grow and the extent to which it will be able to develop strategies for dealing with competitors for power who have traditionally been able to dominate the resources of the political struggle.

In his concluding chapter, Rosenthal addresses himself to the problem of explaining the limited effects of teacher militancy as consequences of the behavior and attitudes of other participants in the educational system. This seems to me to be an especially fruitful way to attack the problem. The practices of educational authorities, particularly school boards and administrators, seem to structure organizational opportunities and provide militant groups with opportunities which, given appropriate leadership, they can exploit. In virtually no case have educational authorities reacted with acquiescence to demands by teachers for an extension of their authority. In some cases there

has been overt resistance and in others there has been a policy of benevolent concession granting. Of course, the behavioral response of school authorities is determined to some extent by the political style of the community in which the competition takes place. If the example of New York can be taken as an illustration of relatively intense conflict, the steadfast opposition of the educational establishment to UFT demands has certainly proved ineffective and suggests that in the politics of education, as in political life in general, head-on confrontations do not necessarily decrease the power of either participant. Rosenthal's conclusions that the impact of teacher groups on public education will increase—which is presumably based upon his finding that organizational affiliation is the crucial variable—might be subject to some qualification, depending upon the varieties of behavior which Rosenthal has described on the part of administrators.

In summary, the book is well researched, well written, and well thought out.—HARMON ZEIGLER, *University of Oregon.*

Social Insecurity: The Politics of Welfare. By GILBERT Y. STEINER. (Chicago: Rand McNally and Company, 1966. Pp. 270. $6.00.)

Gilbert Steiner's opening words, "This book has been written because I could not find one like it. . . ", are descriptive of the paucity of political science literature discussing much of the social welfare activity of government. There has recently been a resurgence of attention paid by political scientists to the substance of policy. However, despite this, the political scientist, politician or citizen who wants to learn about the politics of medical care, of public assistance, of social insurance, or of other social services (with the exception of education), can very quickly exhaust the political science literature available.

Steiner's book is an excellent introduction to the politics of public assistance, the second-largest federal grant program in the United States, and one which is generally conceded to be a failure. Steiner explains the reasons for this failure by describing the process by which public assistance policy is made and explaining the relationship between this process and the policy that results. In the process, he provides a history of the program, a discussion of Congressional attitudes toward and supervision of public assistance, a critique of the attitudes of the social work profession, a description of many of the basic problems and issues in the program, and some proposals for change. Although most of the book has a national focus, a chapter is devoted to public assistance politics in Illinois in the early sixties.

One of Steiner's principal themes is that the nature of the program's clientele and the public image of that clientele have changed, while at the same time the legal outlines of the program have remained the same. The present public assistance program began as an ad hoc effort to deal with the "deserving poor"—those whose poverty was due to no fault of their own—the elderly, the blind, and widows with children. Public assistance recipients were expected to dwindle in number as more and more of the population attained coverage under social security. However, the number of recipients has increased, rather than diminished; at the same time, the image of the recipient has changed from that of the white elderly respectable person to that of the black ADC mother with illegitimate children. With the change in image and the increase in number has come hostility from Congress and the public. Interestingly, this hostility has been largely directed towards the public assistance recipients, rather than those running the program. The hostility has been expressed by means of payments that are often inadequate to meet the state's own minimum standards, constraints imposed on the recipients' behavior, and loss of privacy. The recipients have not been able to protect themselves from the expressions of this hostility: they have no sanction available to enforce their claim on the public, and neither they nor anyone else has effectively organized on their behalf. Although the social work profession purports to speak for both itself and its clients, it has not done so effectively because of the irrelevance of the profession's interests to the reality of public assistance.

The federal public assistance program is based on the taxing power. This means that Congressional supervision of the substance of public assistance is in the hands of the Ways and Means and Finance Committees. The broad scope of concern of these committees, with public assistance of much less importance to committee members than other issues, has meant that neither committee members nor staff pay it much attention. Nor does it receive much attention in the appropriations process: the open-ended authorization and the obligation felt by Congress to appropriate funds to match state expenditures in accordance with past Congressional promises add up to a situation in which public assistance policy and expenditures are considered by the Appropriations Committees to be beyond their control. Congress thus devotes little effort to public assistance policy, and the program continues on its way.

Moreover, the effect of Congressional action on the actual contours of the program is limited because public assistance is not simply a federal program, but a federal-state program in which the federal government pays most of the bill, but the states make many of the important policy determinations. This has been true from the very

first, when Congress defeated proposals for restrictions on state programs in 1935. With time, the federal bureaucracy began to assert more control over the program, but the states have the upper hand in most cases.

This is a description of the situation three years ago, when Steiner's book was published. The situation since then has changed somewhat, although Steiner cannot be faulted for not discussion what had not yet happened when his book was written. Indeed, his description and analysis help one to understand what has happened more recently. The recent rise of the welfare rights movement and the increasing demands for participation by the public in governmental decision-making, mean that the recipients are no longer unorganized, and the inadequacies of the program no longer go unnoticed. The black rebellions, increasing amounts being spent for public assistance, and the resulting financial pressures on the states, have forced Congress, the President, and the intellectual community to pay greater attention to public assistance. By the time this review is published, it is likely that the President will have proposed innovations in the program.

Steiner's book is clearly written. His presentation is topical, rather than chronological, with some resulting confusion for the reader who is not already familiar with the history of public assistance in this country. In his discussion of Congressional action, he sometimes equates increases in the extent of federal sharing of expenditures with increases in the benefits received by public assistance clients. Although there may be a close correlation between the two, it is easier to understand Congressional generosity towards a program whose recipients Congressmen regard with hostility if one realizes that the Congressional action is aimed at helping the states meet their financial burdens, rather than at increasing benefits for recipients.

What we have here, then, is a helpful introduction to the politics of public assistance, one that discusses the basic determinants of public assistance policy and most of the major issues in the program—EUGENE FEINGOLD, *University of Michigan.*

Majority Party Leadership in Congress. By RANDALL B. RIPLEY. (Boston: Little, Brown & Co., 1969. Pp. 187. $5.00.)

This is the second volume in what promises to be a valuable series of monographs on Congress, sponsored by the American Political Science Association and under the direction of Ralph K. Huitt and Robert L. Peabody. It has as its modest goal a discussion of majority party leadership patterns within Congress in this century.

Randall B. Ripley divides the 33 Congresses that met between 1901 and 1966 into four categories. The most easily defined category is that of the "truncated majority," when the House, the Senate and the Executive were not controlledy by the same party. This occurred seven times. The remaining Congresses are divided into those with a "presidential-partisan majority" (8), those with a "presidential-bipartisan majority" (11), and those with a "congressional majority" (7). The outside boundaries of these groupings are evidently established by the size of legislative majorities. Though the author doesn't make a point of this, the three Congresses in which the dominant party held two-thirds of the seats in both houses (the 74th and 75th under Franklin Roosevelt and the 89th under Johnson) clearly produced presidential-partisan majorities. The three in which the dominant party held less than 55 percent of the seats in both houses (the 68th under Harding, the 70th under Coolidge and the 83rd under Eisenhower) clearly produced congressional majorities. Beyond this, the categorization is concerned with whether Congress or the President tends to dominate, and in the latter case, whether the President works primarily through his own or legislative parties.

The author then singles out ten representative Congresses for study in greater depth. For each he discusses the nature of presidential and congressional party leadership, analyzes their relations with each other and with the members of the legislature, describes the handling of a particular measure by way of illustration, and assesses the legislative accomplishments of the two-year period.

All this he does in a workmanlike fashion. Clearly this monograph is an addition to the literature on Congress. My problem is that I want more. His handling of each Congress would have been more valuable to me if it had been less sketchy and repetitious. The conclusions concerning each type of party majority do not seem in all cases to follow inevitably from the text. The book would have been stronger if the author had written more on the development of legislative party institutions, though he has filled part of this gap elsewhere in his *Party Leaders in the House of Representatives.* I would have liked more on the growth of large dissident blocs within the dominant parties: the middle-western progressive Republicans during much of the first half of the period and the southern conservative Democrats during the second half. It would seem almost inevitable that a strong president should operate under a presidential-bipartisan model when the dominant party is so divided internally.

I also wish that he had explored further his finding that the most successful Congresses are those when a party has just come into power.

The examples he chose for expanded discussion were the Democratic takeovers under Wilson and Franklin Roosevelt. There were, however, two other periods when Congresses had presidential-partisan majorities when there was new presidential leadership but no change in the dominant party or in the legislative party leaders (under Theodore Roosevelt and Lyndon Johnson). Perhaps he should have made more simply of the importance of new leadership. His data does support a "honeymoon" thesis. No President has succeeded in maintaining a dominant position for his entire term of office. Franklin Roosevelt's record was the most successful, for he dominated three Congresses under conditions of national economic emergency. Theodore Roosevelt and Woodrow Wilson each dominated two and Lyndon Johnson only one. Could the 22nd Amendment actually help promote strong executive leadership? Would there be any advantage to encouraging more frequent change of legislative leaders by placing some limits on their tenure?

I can see advantages to the organizational pattern which the author follows: discussing Congresses according to their type of majority rather than chronologically. Yet this makes it more difficult to draw generalizations concerning the development of institutions and traditions in the national government, and to contrast majority party leadership in the earlier Republican-dominated portion of the century (1901–1932) with the later Democratic-dominated era.

Perhaps this could have been handled by a longer last chapter with more historical perspective. The data show that during the Republican period the most frequent pattern was that of the congressional majority. Theodore Roosevelt was the only Republican President in the period (or in the succeeding era) to work within a presidential-partisan majority. The Democratic era saw a marked trend away from the congressional majority and an increase in the presidential-bipartisan majority model. As Ripley notes, this reflects the development of the institutional Presidency and the wider expectation that the President would lead and that the legislative party leaders would act as his loyal lieutenants.

The author prefers the presidential-partisan majority model:

> The most successful majority party is likely to be a presidential majority with a large majority, newly come to power, with a President actively involved in legislative tactics, and with innovative leaders. The least successful is likely to be a congressional majority with a small majority, concluding a long tenure in the majority, with a hostile President, and with complacent and unimaginative leaders. (p. 187)

This study will be useful in the continuing debate over the possibility of moving in the direction of greater party responsibility. It would seem that the nation, by moving from a norm of congressional government to one of presidential government has taken a giant step toward party responsibility. The fact that the system continues to provide truncated majority government and only rarely provides a chance for presidential-partisan majorities shows how far we still are from its achievement.—GEORGE GOODWIN, JR., *University of Massachusetts, Boston.*

Bureaucratic Behavior in the Executive Branch. By LOUIS C. GAWTHROP. (New York: The Free Press, 1969. Pp. 276. $7.95.)

The study of public administration in the United States has been greatly enriched since World War II by the development of a unified theory of organizations which embraces both public and private institutions within its scope. Louis Gawthrop's *Bureaucratic Behavior in the Executive Branch* is a very useful addition to this literature. It attempts to illuminate the salient characteristics of government agencies by analyzing the way in which they differ from private organizations.

This does not always prove to be an easy task. For example, Gawthrop argues that hierarchical authority is much stronger in private than it is in public organizations. This is certainly true, at least as far as American bureaucracy is concerned, when attention is centered on the difficulties political executives have in controlling the activities of their career subordinates. The heads of executive departments in the United States have clearly not had as much authority over the units under their jurisdiction as the heads of major industrial firms.

But if attention is shifted to the structure of authority within the career ranks of public bureaucracy, then hierarchy often appears to be much weaker in a private than it is in a public setting. Nowhere in the private sector of society is it possible to find a more hierarchical set of arrangements than those which bind together the uniformed personnel in the armed forces. Military bureaucracy exemplifies hierarchy in its most extravagant form.

Illustrations of a significantly greater degree of hierarchy in public organizations can also be found in civilian administration. Private universities are commonly less strongly centralized in administrative structure than public institutions of higher education. Social workers often seek employment in private welfare organizations precisely because of the network of hierarchical controls which limit professional discretion in public welfare agencies. Herbert Kaufman's *Forest Ranger* has demonstrated that even a nominally decentralized government agency can weave a web of informal controls that leaves very

little actual decision in the hands of subordinate officials.

The most interesting section of the book is Gawthrop's discussion of organizational change. He distinguishes between consolidation, which is change induced by pressures from the internal or external environment, and innovation which he describes as change occurring at the initiative of administrators in an effort to avert impending crises. The executive branch he concludes, is much too prone to focus upon consolidative change and to ignore where possible the need for innovation.

From the perspective of Lindblom's now dominant incremental analysis of organizational change, Gawthrop's concern on this score may seem to be unwarranted. The absence of pressure for change can be viewed as the absence of need for change in the best of all possible administrative worlds that incrementalism often seems to portend. But events of recent years, particularly in foreign affairs and in the administration of urban programs in the United States, suggest that Gawthrop's misgivings are justified. The Bay of Pigs, Vietnam, and the crises in urban education and housing administration point up the mournful consequences that ensue when bureaucracies fail to plan ahead to anticipate troubles before they arrive. Gawthrop's emphasis upon the need for calculated innovation by administrative agencies thus seems amply supported by the recent history of policy-making in the United States.—FRANCIS E. ROURKE, *The Johns Hopkins University.*

Minorities and the Police: Confrontation in America. BY DAVID H. BAYLEY AND HAROLD MENDELSOHN. (New York: The Free Press, 1969. Pp. 209. $6.95.)

This book is a worthy addition to the growing literature on the police in American society. Through the use of several opinion surveys, it seeks to explicate the attitudes, experiences, and emotions of both the police and urban ethnic groups as a means of understanding the texture of their relationship. It is the authors' thesis that this complex relationship can best be understood by examining the constraints, or more accurately, the predispositions of each side. In pursuit of this goal they have chosen to focus on the relationship as perceived by the principals rather than as viewed objectively by a detached observer. This is a useful tack because, as is so often the case, the problem is more a function of the perceptions of the participants than of objective reality. In any event, the picture they paint is not an encouraging one.

The minorities' regard for the police is not high. Very few thought that the police enjoyed a high reputation or did an excellent job in their neighborhoods and almost half of the minority population believed that police treatment is often unfriendly or prejudiced toward them. They believed the widespread charges of police brutality and a substantial proportion said that they or someone in their family had been badly treated by the police.

These perceptions have important consequences for police-minority group relations. One can easily understand the fact—borne out by the data—that minority group members are less likely to contact the police than those in the rest of society despite their demonstrably greater need for services provided by the police. Thus, minorities approach the police with reluctance and low expectations. This difficulty is, in the authors' view, exacerbated by the policeman's status as a symbol of "the backhand treatment [minorities] receive from society as a whole." (P. 141)

The survey indicates that there is corroboration for minority views of the police. Twenty-seven percent of the police officers interviewed admitted witnessing an incident in which a fellow officer had been guilty of excessive use of force or unjustified harassment of a citizen. Moreover, one-third of the policemen admitted that they believed stricter law enforcement desirable in minority communities.

Despite the justification for some of the minorities' views, the position in which the police find themselves when working in ghetto areas is not an enviable one. Police perceive substantially greater danger of violence to themselves in such areas and the majority claim to have been victims of physical or verbal abuse. Importantly, their perceptions are probably justified by the facts, as an examination of the geographical distribution of instances of assaults on officers and cases of resisting arrest will demonstrate in nearly every major city. (The authors did not make such an analysis for Denver, the sample city.) Moreover, the consequences of a misstep in the ghettoes are potentially serious, for even beyond the possibility of personal violence lies the threat of civil disorder or formal complaints against the officer by aggressive minority group members.

The important conclusion to be drawn is that fear is the controlling force in police-minority group relations. Minority group members fear the police: they fear unfair treatment, they fear personal abuse, and they fear physical brutality. The police, on the other hand, fear physical attacks when working in ghetto areas and fear having their conduct subjected to official scrutiny.

In this atmosphere of mutual distrust, what can be done to relieve tensions? What is required, of course, is a solution to the problem of securing true equality for all our citizens. Aside from this

fundamental change, however, the authors have little to recommend. Short-term approaches must naturally, as they indicate, take account of the respective "worlds" of the police and the minorities. However, no hard policy alternatives are offered—and this is the weak point of the book For having sketched with some sensitivity the nature of this difficult relationship, the authors leave us without much analysis of the policy implications of their study.

There are a few additional problems with the book, though of rather less import. The surveys were done in Denver, a city wracked by a police scandal during the early sixties. This may bias some of the results concerning community regard for the police. In addition, the questionnaires are not appended and there are very few tables. Consequently, some data and most of the questions appear only incompletely and sketchily in the text and the precise results and interview schedules are often unavailable.—LEWIS A. KAPLAN, *Bureau of the Budget, City of New York.*

An Act of Congress: The Legislative Process and the Making of Education Policy. BY EUGENE EIDENBERG AND ROY D. MOREY. (New York: W. W. Norton and Company, Inc., 1969. Pp. 256. $6.50.)

One of the costs of our status striving as an academic discipline is a growing disposition to denigrate the case study, and to exalt the study with a wide data base. The timid, therefore, whose research direction follows a careful sensing of the profession's unwritten canons of the moment, often will forego a deeper understanding of political processes in favor of the extensive registry of surface characteristics for a large number of research objects. One should expect, though, that a mature scholarly enterprise would be sustained by the simultaneous production of intensively and extensively focused research. The two ought to be complementary as the author of a case study is mindful of his obligation to theory, and to the extent that the broad data scholar does not confuse breadth with depth, or description with understanding.

Eugene Eidenberg and Roy D. Morey have written a very good case study, introduced with a slight nod to theory, and concluded with a smattering of general observations that should have theoretical utility. It is the best thing that has been written on educational policy making. Indeed, it is difficult to name a study in any domestic policy category that is superior to this one with respect to its expert and detailed analysis of the substance, behavior and nuance which constitute the process of policy making.

The major purpose of the book (which is artfully realized) is to provide a "working description of the American policy process by focusing on the issue of federal aid to education." The education focus is auspicious. There was a long history of immobilism in this policy area, during which the simultaneous influence of a variety of issue conflicts divided potential supporters of federal aid into several blocs, preventing the formation within Congress of a sympathetic majority. Gradually the conflcts were negotiated (primarily because of changes in the political and social environments) so that by 1965 the one remaining barrier was the church-state issue. Even here a precedent—overlooked by the authors—had been worked out in 1963 with passage of the Higher Education Facilities Act. Congress was conditioned by 1965 to compromise on the church-state question; it remained for the principal interest groups to reach an accord.

The authors discuss with skill and insight the formulation and implementation of strategy to (1) surmount the religious controversy, (2) tie the bill to the then popular anti-poverty drive, and (3) secure passage in both houses without amendment. Major credit for building interest group consensus (essentially between the National Education Association and the United States Catholic Conference) is assigned to Francis Keppel, who was detailed by the White House to persuade the group spokesmen to reason together, as it were. Eidenberg and Morey contend that such agreement was necessary in order for a federal aid bill to pass in 1965, and suggest that the case for accommodation had to be put to each side with equal force.

I should have thought, however, that the NEA had no choice. Through its zeal and self-righteousness it had become discredited in Congress. If ever there was to be federal aid to education legislation, 1965 was the year. The overwhelming Democratic majorities in Congress, buttressed by authentic White House enthusiasm, made a favorable decision likely. Under those conditions the NEA had little veto power; those whom it could influence probably would have favored federal aid legislation even if NEA's support were withdrawn. Organized Catholicism, on the other hand, enjoyed a potential veto, and the course of action open to NEA, therefore, was to adhere to whatever arrangements were made in order to retain the Catholic spokesmen in the coalition.

There is very little in the narrative of this excellent book that could be improved. Any book, of course, can supply truffles for those who root after minor blunders. This one is no exception. Subcommittees are incorrectly named (pp. 57, 60, 81); reference is made to a non-existent "Higher Education Act of 1962" (p. 106); the first name of a well-known syndicated columnist

is misspelled (p. 152); and, in my judgment, the characterization of the past working relationship involving Representatives Edith Green and Albert Quie, and former Representative Charles Goodell, is wide of the mark (p. 108).

Conspiracy theorists will find the authors blinded by pluralist assumptions. Consider the definition of a policy system:

> The policy system that contributes to the making of education policy is primarily an elite of individuals and groups *who have a right to participate in the decisions affecting public education, based upon recognition of their particular interests in the issue.* (p. 4) (italics added)

Add to this the conclusion that

> One of the clearest impressions that results from close study of the education issue during 1965–1967 is the diffusion of power and control in the American political system. There is no single official or institution of government that was responsible for the passage of federal aid legislation. (p. 229)

And for democratic accountability? "Public opinion as a single force has no significance in the workaday world of the policy-maker." (p. 215).

Finally, as successful as the book is in depicting the policy process (as competition and conflict among elites), the authors fail, I believe, to develop as fully as they might have the theoretical potential of the data. We really need some good longitudinal analysis of the emergence, development, dissipation and decline of policy systems. We need an adequate taxonomy by which to compare policy systems in terms of their compositions, decision-making patterns, degrees of autonomy within the larger political system, and the relative permeability or impermeability of their boundaries. Eidenberg and Morey have the materials in their study for at least a modest beginning toward these, or other, objectives. With a firmer theoretical context, the book could have been a useful supplement in advanced undergraduate and graduate level courses. As it stands, it certainly is one of the best books available through which to provide beginning undergraduates a clear understanding of the legislative process and the larger policy making process.—LAWRENCE K. PETTIT, *American Council on Education and Montana State University.*

Candidates for Office: Beliefs and Strategies. BY JOHN W. KINGDON. (New York: Random House, 1968. Pp. 176, $1.95.)

Despite a plethora of empirical studies in recent years that bear in one way or another on the relationship between elected politicians and their constituencies, it has become all too painfully obvious that we have hardly begun to construct in our professional literature a reliable set of generalizations about this centrally important democratic phenomenon. Even if we ignore electoral politics in other political systems and look for such generalizations applicable only to American politics, the problem remains. Somehow, it seems, even in our own back yard, the linkages that operate between the electorate and the proximate policy-makers who produce what we have come to call the policy outputs of the political system have evaded successful description. This is not to say, of course, that we do not know a great deal about the American electorate. We do. Nor is this to say that we know little about the policies that elected officials produce. In fact, information abounds. What remain all to mysterious, however, are the linkages between electoral phenomena and the policy-making activities of elected officials. It is to this murky area that John Kingdon gives his attention in *Candidates for Office: Beliefs and Strategies.* While this little volume seems to me to raise a great many more important questions than the answers it provides, it *does* raise important questions. And if for no other reason than that it adds to the very little bit we know about the cognitive processes of electoral candidates and elected public officials, it deserves the attention of students of American political behavior. All too often as Professor Kingdon sees, we have somewhat simplemindedly conceptualized the elected official (especially the legislator) as a kind of inert black box between such factors as party, constituency and "group pressures" and the policy outputs of the political system. Surely, this is a woeful mistake. I see this book as a modest, but helpful addition to the still small literature that seeks to overcome this difficulty. To me, at least, it is a welcome addition that ought to help nourish future inquiry into the linkages that operate between the electorate and the policy-making activities of the elected.

What Kingdon has done here is really rather simple. He has interviewed a sample of candidates for the statewide elective offices, for the U. S. House of Representatives and for the state legislature in Wisconsin in 1964. To be precise, he interviewed all candidates for Congress and statewide offices and a sample of candidates for the state legislature—a total of 62 individuals. Through structured post-election interviews he sought to investigate their beliefs about the electorates they faced, their sources of information about them and their thoughts about campaign strategy. He has then speculated about the likely importance of his findings in relation to pre-election and post-election behavior. There is some evidence that the study was imperfectly designed to answer certain kinds of questions that turned out to be unanswerable when the data had been collected. For example, it seems that an original purpose—testing the proposition that candidates with an accurate cognitive picture of the electorate should be more successful at winning votes

—simply had to be jettisoned as it became clear that it could not be dealt with in a single, post-election interview study. There is simply no way of knowing whether the pictures of the electorate that the study revealed were there before the election or whether they had been significantly altered by the campaign and election experience. As it turned out, moreover, the more successful candidates seemed to have the more inaccurate cognitive picture of the electorate.

Kingdon argues that winners rather than losers tend to downgrade party and emphasize issues and personal appeal as the decisive factors in election outcomes. This, on the basis of past survey research studies, he reasons, is unlikely to correspond to the actual facts of the electoral situation. Faced with this finding, Kingdon *speculates* that there is a "congratulation-rationalization" effect at work. Winners, he supposes, are likely to congratulate themselves, thinking that the electorate is paying more attention to them as individuals and to the issues they raise than is actually the case. Conversely, he supposes, losers may tend to rationalize their defeat by playing down these factors, which after all reflect on their personal qualities and their campaign strategies, and explaining their defeat in terms of such factors as party which are largely beyond their control. The congratulation effect, Kingdon further speculates, may very important consequences for post-election behavior. It may, he suggests, help to expain why electoral politicians act as if the electorate is watching them very closely even though we know that in the vast majority of cases it is not. This is an interesting speculation. Unfortunately, however, there are difficulties.

As Kingdon suggests, the congratulation effect in his Wisconsin sample is largely the product of marginal winners. This being the case, it will hardly serve to explain why non-marginal candidates also act as if the electorate is watching them more closely than we know it is. Unfortuately, there is more difficulty. Kingdon has argued in an earlier presentation of these data (this REVIEW, LXI, 1967, p. 137) that candidates with a high estimate of voter interest and issue knowledge vote more in accord with their constituents' opinions than those with a lower estimate. This he bases on a secondary analysis of the Miller-Stokes Congress study of 1958. He goes on to argue in *Candidates for Office* that ". . . by virtue of his electoral victory, an officeholder (*especially a marginal winner*) [italics mine] believes that the eyes of the public are on him, that voters cast their ballots according to his actions and characteristics, and that they are comparatively well informed about the issues of an election. If an incumbent thinks the electorate is watching him, whether or not they are in fact, he will attempt to anticipate their reactions to his decisions." All of this seems to make good sense, but unfortunately the Miller-Stokes data suggest just the opposite for congressmen. In the issue areas of civil rights and welfare there is a strikingly lower degree of correspondence between congressmen's perceptions of what their constituents want and their roll call voting in the case of marginal rather than safe men.[1] Surely, this is paradoxical, but it is the case, and it directly contradicts Kingdon's point. I mention this only to show that the picture is by no means as clear as it is painted in *Candidates for Office*.

In the remainder of his study Kingdon investigates candidates' beliefs about group support, sources of information and campaign strategies. Again, there are interesting data and speculations on all of these matters, which ought to be heeded by students working in this area. Most important, I think, is the general insight that constituency is a far more subtle phenomenon than much of our literature suggests. In addition to the mass electorate all sorts of elite groups are important. And, finally, as Dexter and others have argued, it is the mind of the elected politician, ultimately, through which all "constituency factors" must be filtered. I agree completely with Kingdon that we have all too often totally neglected this insight, as obvious as it would seem to be.

In sum, this is a helpful little volume, whose speculations are often better than its hard data. It deserves to be read and pondered. I very much hope it will be taken as the point of departure for a number of other studies in both state and national contexts. Kingdon is to be thanked for a number of shrewd reflections on the problem of constituent-politician relations. Hopefully, he and others of us working in this area will soon design and execute the kinds of studies that will be necessary if we are to understand what would seem to be the most central question of democracy—how and why various elected officials do the things they do.—W. WAYNE SHANNON, *The University of Connecticut.*

Discretionary Justice; A Preliminary Inquiry. By KENNETH CULP DAVIS. (Baton Rouge: Louisiana State University Press, 1969. Pp. 233. $8.50.)

In this book, built upon the Edward Douglass White lectures given at Louisiana State University in 1966, Kenneth Culp Davis endeavors to develop a partial answer to a very basic question: "In our entire legal and governmental system, how can we improve the quality of justice for

[1] See Malcolm E. Jewell and Samuel C. Patterson, *The Legislative Process in The United States* (New York: Random House, 1966, pp. 441-444.

individual parties; how can we reduce injustice?" (p. 215) He argues that one of the most fruitful lines of attack is against administrative discretion, or those decisional situations "where rules and principles provide little or no guidance, where emotions of deciding officers may affect what they do, where political or other favoritism may influence decisions, and where the imperfections of human nature are often reflected in the choices made." (p. v) Though the book is admittedly "a preliminary inquiry" and suffers from some repetition and a conversational rather than an analytical emphasis in many places reflecting its origin as lectures, the central argument is clear and provocative: one of the major causes of injustice to individuals and of "official illegality" in the society is unnecessarily broad administrative descretion, and something can and ought to be done about it.

Support is mobilized from diverse areas, including welfare, housing, lending and loan guarantee, regulatory and law enforcement programs. Two broad and especially fertile sources of injustice receive particular emphasis. The first is the selective enforcement of laws by police, regulatory agencies, and especially prosecutors, where important choices are constantly made, unfettered by traditional safeguards against arbitrary and unjust action. There is, for example, the ordinary policeman who possesses "unguided discretionary power to weigh social values . . . and make a final decision as to governmental policy . . . , despite a statute to the contrary, without review by any other authority, without recording the facts he finds, without stating reasons, and without relating one case to another." (p. 88) Davis shows that equal treatment of individuals in the enforcement of laws, despite contemporary concerns centering on minority groups, has never been and is not now solidly implanted in our notion of justice. He is fully cognizant of the varied rationales for selective enforcement, such as overreaching statutes, scarce resources, and the leniency that often results. However, he questions whether selectivity is the right approach and suggests that non-discrimination in law enforcement is an appropriate goal to seek. If this redefinition were made, if actions were taken to remove the reasons making selective enforcement necessary and practical in many instances, and if proper safeguards were adopted, he estimates that a high percentage of injustice to individuals caused by unduly expansive administrative discretion would be eradicated.

Actions bestowing gratuities or privileges are a second main source of injustice. It seems to be generally accepted that decisions about subsidies, for example, need not be surrounded by protections of the sort required when legal rights are affected. Davis feels this to be an improper distinction and one of rapidly increasing significance in the production of injustice; the value of the outcome to the individual is the important element. An injustice may occur when an application for welfare is decided, just as when a zoning matter is determined. But because of the traditional distinction, the system is designed to minimize the chances of an injustice in one type of situation but not in the other.

It should be emphasized at this point that Davis is not a foe of administrative discretion *per se*, as readers familiar with his other works will know. He appreciates the necessity of a considerable amount of discretion owing to our natural incapacity to forsee all possible circumstances and write rules accordingly. Further, he appreciates the desirability of discretion in many circumstances for stimulating flexibility, creativity and innovation. On the basis of this book, Davis might be described as an impassioned realist on the question of administrative discretion. He is passionately angry about injustice resulting from unnecessary broad administrative discretion and is extremely effective in pointing up problems which have received too little attention from scholars in recent years. He is realistic about the terrible difficulties involved in doing something about extraordinarily complex problems which have deep institutional roots and about the requirements for change and the instruments to be used.

Change is very much related to each of Davis' three major purposes. The first is to demonstrate that discretion is not just an illusive will-of-the-wisp and that it can be studied profitably. This he does. The second is to suggest further lines of inquiry, and the third is to devise a framework for additional study. Important contributions are made along these lines, but as Davis notes, many problems remain and much filling in must be undertaken.

One of the best examples concerns the first step in the change process: determining the optimum degree of discretion in any particular situation. In the Federal Trade Commission's pre-merger clearance program, so secret that even the reasons for secrecy are secret, there is obviously too much discretion. But how much is enough? What are the relevant criteria? What are the competing values, and how are they to be balanced? Davis identifies problems such as these, but he cannot be said to provide much in the way of specific direction for future analysis or the development of operational answers.

Limitations of the type just indicated also apply to the discussion of the next step in the change sequence, which is to reach the desired quantum of discretion by use of the appropriate

combination of structuring and checking instruments. Some imaginative new departures are suggested, such as that government lawyers cease relying on ripeness and standing doctrines and other technical defenses under certain circumstances, but by and large the means discussed are traditional and formal in character. Structuring, or limiting the range of choice within agencies, can be achieved by employing a mixture of plans, policy statements, rules (which Davis considers especially valuable), statements of findings and reasons in particular cases, and openness and fairness in informal procedures. The means for checking discretion to insure that it is exercised properly run the gamut from normal hierarchical controls to various forms of external review.

A number of interesting points are implied by these portions of the book. One is that contemporary enthusiasms such as decentralization, community control and citizen participation in the administration of programs provide no ultimate answers to the problem of injustice, though they may be alleviative, so long as discretion remains unstructured and unchecked. That is, increased citizen control of police will not necessarily in and of itself eliminate injustice resulting from selective enforcement of law, but may only alter the pattern of selective inforcement and shift the incidence of injustice.

Overall, however, description of particular means and particular situations is accentuated, rather than the development of a framework for determining how the proper combination for any situation might be development, much less suggesting criteria for telling what that combination might be.

With some justification, Davis gently chides students of public administration and political science for their contemporary lack of concern about administrative injustice, suggesting that a major reason is a preoccupation with "science." One of his subordinate purposes is to encourage scholars from other disciplines to bring their expertise to bear on the problems he discusses. There is a modest irony here. It seems that the book would have been much stronger in its assessment of problems, in dealing with the intricacies of change, and in its prescriptive aspects if the rather extensive relevant literature, both theoretical and empirical, produced by those whom he chides, had received more attention. To cite only one of the many possible examples, Davis judges normal hierarchical controls to be the most significant check on the exercise of discretion. However, his discussion on this point is not only truncated, but it evidences no familiarity with the knowledge and insights that have been developed in recent years not only by students of public administration, but also by others such as sociologists, as to how people behave in organizations. Certainly there is rich material here for use in analysis.

Nevertheless, *Discretionary Justice* is a powerful and stimulating argument for more systematic attention to administrative discretion. It deserves to be read and pondered by all of those concerned with administrative justice—which today, it should be added, is a most "relevant" matter.—DAVID M. WELBORN, *The University of Kansas.*

The Politics of the Bench and the Bar: Judicial Selection Under the Missouri Nonpartisan Court Plan. BY RICHARD A. WATSON AND RONDAL G. DOWNING. (New York: John Wiley and Sons, Inc., 1969, Pp. 393. $10.00.)

This book comprises a comprehensive study of the mode of judicial selection commonly referred to as the "Missouri Nonpartisan Court Plan." The Plan was examined in its "mother" state. The study of judicial recruitment within the context of comparative state political systems has been advanced very substantially by this intensive six year investigation of the Missouri judicial system. The authors not only trace in competent fashion the historic antecedents of the modern Missouri Plan; they also investigate thoroughly the links between lawyers, the legal profession, law-oriented interest groups, governors, gubernatorially-oriented lay nominating commissions and judges.

One particularly significant portion of their study embodied a comprehensive analysis of the characteristics of lawyers and bar organizations in the two major metropolitan regions of Missouri, Kansas City and St. Louis. What kinds of issues generated cleavages in the bar in these large cities? What are the attributes which are related to the development of a "two-party" system within the bar of these cities? To what extent is "style of practice" related to lawyer advocacy or participation in either political party or bar association politics? What about the process of political and/or legal socialization of lawyers? Variations in the nature and situs of legal education? Income? Watson and Downing addressed themselves systematically to the task of providing empirically verifiable answers to these questions.

More than a century ago, Alexis De Tocqueville contended that "the seat of the American aristocracy is with the judges on the bench and lawyers at the bar . . . the lawyers are the most powerful class in the community, and the only real counterpoise to the influence of the democracy." Whereupon a contemporary American reviewer opened a long lasting argument about the validity of that assertion. Commenting in *The United States Magazine and Democratic Review* of July, 1838, De Tocqueville's critic retorted that "The members of the legal profession in this

country are not the aristocracy but the agents . . . of the aristocracy . . . [which] is constituted by the owners of *accumulated* wealth, and chiefly by the moneyed men of the great commercial cities." Unfortunately, much of the debate over the past 120 years merited the same sort of comment as that provided by Herbert Jacob when he said "the debate over judicial selection continues in a 'factual vacuum.'" Drawing upon the modern contributions of sociologists as well as upon the data gathered by means of mail and personal interview questionnaires, Watson and Downing provide a refreshing departure by gathering empirical evidence derived from the lawyer populations of St. Louis and Kansas City. Their evidence indicate that,

> Attorneys want certain kinds of judges on the bench who will interpret rules in a particular way because such matters affect their individual law practices . . . in judicial selection, as in the handling of legal affairs generally, attorneys act as spokesmen of the social and economic interests they represent.

Interestingly enough, the major competing intraprofessional groups, plaintiffs' and defendants' lawyers, have, during the period investigated, maintained "a fairly even distribution" on the judicial nominating commissions utilized in the Missouri system.

With respect to the social and political role of the legal professional, this work is of especial significance. For in addition to providing factual answers to broad public policy questions such as that raised by De Tocqueville, the authors probed deeply many other aspects of lawyer behavior. Of salient interest were their inquiries concerning (a) "the motivations behind lawyers' seeking a judicial position, (b) the barriers, social, economic, or professional, which block judicial candidacies, (c) the objectives or goals which motivate lawyer invelvement in judicial contests, whether as candidate or lawyer activist, (d) the "social cleavages" mentioned above, and (e) the roles played by lay nominating commissioners and particularly by sitting judges in the judicial process. The researchers also sought to describe the consequences associated with the change from the old elective method of selection which was avowedly "partisan" and the new mode of judicial selection which was dubbed by its advocates as "nonpartisan." Utilizing longitudinal data, the judges chosen before and after adoption of the Missouri Plan were evaluated by the bar interviewees. Similarly, a comparison of the vistages of the older system retained in "outstate" Missouri and in urban St. Louis with the operation of the new system was made.

Because the main thrust of the inquiry was focused upon the legal profession, the rather clearly identified consequences for the political parties were not etched as clearly as were the impressions these changes made members of the legal profession. Of the five measures employed, four involved lawyers or judges, only one, the general retention vote, the public at large. Thus lawyers' opinions were determinative of evaluations of consequences or "pay-offs," while assessment of the new plan by party regulars of the system was omitted. Recognizing that the authors have indeed made a substantial contribution, and recognizing also that a broadening of the dimensions of this study would inevitably have entailed a more substantial investment of time, the following recommendation warrants consideration principally for future exploration: that the same sort of intellectual cross-fertilization from the literature and empirical discoveries of leading students of political parties which characterized the authors' analysis of the realities of judicial selection would contribute saliently to further investigation of the consequences of the basic change in mode of selection.

Just as Schattschneider posited his scope of conflict approach with respect to political parties, investigators of judicial recruitment may profitably explore perspectives which are broader than those of the legal profession and the judges chosen from the political subculture comprising the legal profession within a particular state. Such inquiry could contribute importantly to the developing explorations by Burnham and others of the institutionalization of political parties in America as well as broaden the research horizens of students of comparative judicial recruitment. Similarly, the problem of the explanation of judicial consensus—e.g., "the overwhelming amount of agreement among members of [the Missouri Supreme Court]"—may be brought closer to solution by fuller understanding based upon empirical evidence of the property oriented frame of reference of the legal profession. For example, Leon Mayhew and Albert J. Reiss, Jr., investigated a sample of nearly 800 interviewees in metropolitan Detroit to explore the social organization of legal contracts. They concluded that,

> The implication of our findings is that untreated problems exist for all segments of the community. Organized to serve property and a few other problems, notably divorces and accidents, the legal profession provides relatively little professional representation and advice in relation to a broad panply of problems surrounding such daily matters as the citizen's relation to merchants or public authority . . .
>
> It is an interesting commentary on the legal frame of reference to note that one legal scholar has argued that such interests as rights to welfare benefits, job and retirement rights, and civil rights will only be adequately protected when lawyers come to see them as property rights.

These admonitions for further inquiries are made not as severe criticisms of the current re-

search report of the investigators, but in recognition of the solid research foundation they have provided for future expanison of the dimensions of comparative state judicial recruitment. Watson and Downing have made a major contribution.—JOHN SCHMIDHAUSER, *University of Iowa.*

Public Opinion, The President, and Foreign Policy. BY DORIS A. GRABER (New York: Holt, Rinehart and Winston, Inc., 1968. Pp. 374. $6.95.)

Public participation in the making of foreign policy is a democratic myth. Foreign-policymaking is the prerogative of the President, and in this area there is "an absence of specific concern with public opinion." At most, the citizenry has a "veto-support" function in determining America's course in world affairs. That is the way it has been since the formative years of the nation, that is the way it is now, and that is the best way to protect the "public interest." Such are the major conclusions of this book dealing with the "opinion-policy relationship as seen from the presidency's perch."

Despite the Vietnam experience, these conclusions are probably correct. But unfortunately they cannot be regarded as valid or definitive on the basis of Professor Graber's work. For while she has approached the subject systematically and has asked all the right questions, her conclusions are based on such limited evidence that they can only be viewed as hypotheses requiring further study. Her book is a serious attempt at "empirical investigation" of the foreign-policy-making process, but she builds too large a theoretical edifice on too narrow an empirical foundation.

The "data" here consist of four case studies in presidential decisionmaking: Adams and the effort to maintain peace with France in 1793, Jefferson and the purchase of Louisiana in 1803, Madison and the War of 1812 with Britain, Monroe and the hemispheric doctrine of 1823. Each is a descriptive-analytic gem—pared to the historical bone, properly focused, interestingly recounted. But these materials are not sufficient to support a general theory of contemporary public-presidential relations in the field of foreign affairs. The author states that "The political world has changed greatly since the eighteenth century. But the relationship of public opinion to foreign policy decision making in the White House has remained essentially the same." Perhaps. But certainly we are entitled to have some evidence which is less than 150 years old. As Hans Morgenthau indicates gently in his *Foreword* some reference might have been made to Woodrow Wilson and the League fight or to Franklin Roosevelt and American involvement in the Second World War, and though the book was completed before Lyndon Johnson's Vietnam experience could be fully analyzed, his opinion-policy problems—manifest as early as 1965—might have been given some attention.

Even aside from the failure to present modern "data," however, the value of this book is diminished by its cursory treatment of several crucial matters. There is virtually no mention of secret policymaking by Presidents, the "representative" role of Congress in the formulation of foreign policy is hardly discussed, and there is no serious analysis of the impact of radio and television on the public-presidential and opinion-policy relationships. If the behavior of Presidents in making foreign policy has not changed "essentially" since Washington's time, certainly there have been significant changes in the way public opinion is communicated to Presidents and in the way Presidents attempt to influence, mold, or control public opinion. Moreover, Professor Graber fails to discuss why it is (if it is) that democratization, industrialization, and urbanization—not to mention technological, institutional, and educational development—have had no significant effect on the "policy-opinion relationship." Again, it may be true that despite the vast changes in America and the world during the past 150 years that relationship has not changed in any important way. But so sweeping an argument challenging the validity of so fundamental a tenet of American democracy deserves the fullest discussion and analysis.

This book is valuable not because it answers questions, but because it raises them. A democratic society must constantly examine the issue of public participation in the making of foreign policy. And such an examination is particularly desirable now. For, given the nation's leadership role in world affairs and the sensitive nature of foreign relations in the nuclear age, that issue is more critical today than it has ever been before. Professor Graber has not answered the questions raised in her book, but she has done well to remind us of the continuing dichotomy between democratic theory and governmental practice in the formulation of American foreign policy.—ROBERT S. HIRSCHFIELD, *Hunter College, City University of New York.*

The Politics of Religious Conflict: Church and State in America. BY RICHARD E. MORGAN. (New York: Pegasus, 1968, Pp. 156, paper.)

When many of us write or think about religion and politics, we put on blinders. Many excellent scholars have produced works in political philosophy, in constitutional law, in impact, and in voting analysis considering this question, but few

utilize a format of analysis adequate to see beyond the specifics. Besides, some studies are more dedicated to proving one contesting viewpoint or another "right" than in accurately describing what the views are.

This brief study is organized to present a more comprehensive picture of the complexities of religion and politics than is generally available. To summarize: Morgan briefly indicates the theological differences which serve as a partial basis of church-state conflict, spells out the resulting personal attitudes and religious institutions which serve to structure the conflicts, and suggests that "governmentalization of welfare services" and the "welfare revolution" are "trigger issues" to that conflict. He then provides original materials for an excellent description of the organizations involved in current church-state conflicts, classifying them as "Separationist," and "Accommodationist." This is followed by a systematic description of the activities of these groups in national and state, and in judicial, legislative and administrative arenas. The most interesting chapter is a case study of New York's 1967 attempt at constitutional revision which would have abolished the particularly strict "Blaine Amendment" restrictions upon church-state interactions. These are written in an intimate style; we can all become inside-dopesters.

The book describes ideas, actors and actions, but does not always show why these are important. For example, group theory is briefly explained early in the book, and methodological notes are sprinkled throughout, but the conclusion does not follow this up. There is ample opportunity to comment on the utility of that theory because it is only somewhat useful here. Two examples may suggest that the political groups weren't really political groups after all. Contestants in the central case study were classified as either "Separationists" or "Accomodationists." While the description of the leadership's behavior indicates that it fits well in such a classificatory scheme, political followers fit less well. Catholics voting on the proposed New York State Constitution acted as though those groups were not their political referents; Professor Morgan indicates that they voted like middle class suburbanites. It would have been useful for him to have speculated as to why other classifications (upper class, middle class; suburban, urban) would not have presented clearer explanations.

More important, implicit within the basic organization of the book is the hypothesis that theology affects the kinds of organizations individuals join and the behavior they follow. Thus, if the Catholic, Protestant and Jewish beliefs reflect fundamental theological differences, then one might expect Catholic, Protestant and Jewish groups to advocate polar policies and for individuals in those groups to vote differently from each other. Here, they don't; the groups aren't groups. The significance of these facts to what I assume to be a major theme of the book is not explained, nor is the opportunity taken to propose other hypotheses of belief-action, even though the excellent bibliographic notes include sources which suggest divisions within these three faiths, rather than among the faiths.

Comment upon the utility of group analysis of belief oriented groups would have seemed to me to be a logical conclusion to the book. Such a conclusion could have reiterated the importance of the method and, through it, of the subject matter. Instead, this basic organizational theme is dropped and the concluding chapter merely hopes that religious conflict will subside.

The descriptions of the groups involved in religious politics and the case study of New York's attempt to replace its constitution are particularly well done. The break from more limited and more stodgy analyses of church and state allows us to remove our blinders for a short, fresh view of religion and politics. I have used the book in an undergraduate course and found it valuable to my students. In the particular subject matter of religion and politics, such a brief, direct book can be very useful to professional readers.—J. P. JONES, State University of New York at Buffalo

Double Jeopardy: The Development of a Legal and Social Policy. By JAY A. SIGLER. (Ithaca: Cornell University Press, 1969. Pp. 264. $9.75.)

The Supreme Court's recent decision in *Benton* v. *Maryland*, which incorporated the double jeopardy provision in the Fifth Amendment into the due process clause of the Fourteenth Amendment, emphasizes anew the need for a clear and explicit understanding of the meaning of double jeopardy in American law. By analytically delineating the major issues surrounding the double jeopardy problem, Sigler does much to clarify the options presently open to policy makers who favor reform in this area of the criminal law.

This book will very likely appeal to both lawyers and political scientists. The double jeopardy concept is initially described in terms of its common law development and is then explored with regard to both federal and state policy. In the two chapters on federal and state policy, the various tests which have been used by the courts in applying the double jeopardy concept are defined and their various weaknesses are revealed. Lawyers may find these largely descriptive chapters to be of special interest for they canvass much of the available case law. On the other hand,

political scientists may find themselves somewhat more interested in the chapter in which there is a generalized comparative treatment of double jeopardy as it has appeared in the world's legal systems. Similarly, in a following chapter there is an analysis of the policy issue raised by double jeopardy. The policy issue is, as Sigler perceives it, one which "resolves around the decision whether the community is well served by the restriction upon the prosecutor's powers to initiate and pursue the prosecution of criminal suspects" (p. 155).

A comparative examination of double jeopardy reveals that the concept, although present in most legal systems, has been variously understood. In addition to a frequent lack of agreement as to the precise meaning of double jeopardy, many of the newer nations have not accepted the concept nor has double jeopardy been accepted in the civil law countries. An essential purpose of a comparative analysis of double jeopardy is, Sigler suggests, to facilitate reformation of the concept. Accordingly, one of his several conclusions which may be of interest to students of countries with federal systems is that "no definite solution to the problem of overlapping federal and state jurisdictions seems [elsewhere] to have developed" (p. 153).

The policy issue presented by double jeopardy involves fundamentally the nature and extent of the discretion which ought to be entrusted with the public prosecutor. Double jeopardy is a limitation upon the prosecutor's power, since it serves to restrict the initiation of charges which can be brought against a given defendant. However, the prosecutor still has considerable control over the number of crimes a defendant may be accused of having committed. This discretion is especially significant because of the steady tendency in the United States to proliferate by statute the total number of possible criminal acts. Although his discussion of the policy issue involved is restrained and judicious, Sigler does favor a revitalization of double jeopardy in part because of the growing strength of the prosecutor in matters of criminal procedure.

Not everyone is persuaded that double jeopardy is "a constitutional principle under stress" (p. 189). There are those who take the view that double jeopardy is a lingering anachronism from an earlier time when unmitigated severity in the punishment of criminal offenses was alarmingly common. Consequently, since the harsh punitive spirit which once pervaded the administration of criminal justice, and which gave rise to the double jeopardy doctrine, no longer prevails, the doctrine is said by some critics to fall for want of a viable rationale. Sigler does not find this view persuasive—and, I think, properly so—favoring instead reforms which would halt the decline of the doctrine, although he emphasizes that major changes in double jeopardy policy should be initiated by legislators rather than by judges.

There is a decided need for books of this kind in public law. Case analysis, when supplemented with a broader social science perspective, continues to be an indispensably valuable tool in public law research, especially where, as here, questions of social policy are at issue. The selection of a research design thus depends largely on the nature of the questions with which the researcher is concerned. Rigorous case analysis is in this book combined with a broadly gauged interdisciplinary approach which draws heavily on law, criminology, and political science. The result is an able and admirably balanced treatment of a highly significant constitutional doctrine.—DAVID N. ATKINSON, *University of Missouri, Kansas City.*

Participation of the Poor: Comparative Community Case Studies in the War on Poverty BY RALPH M. KRAMER (Englewood Cliffs: Prentice-Hall, 1969, Pp. 273. $3.95 Paper.)

Participation of the Poor is a study of "the inception of the Community Action Programs in the San Francisco Bay Area to see how various communities would implement the legislative mandate to secure the maximum feasible participation of the groups and areas to be served." (p. v.) The volume, therefore, is not a description and analysis of all phases of the CAP programs in the five communities chosen for study (the cities of San Francisco, Oakland and Berkeley, and Santa Clara and Contra Costa Counties). Rather, the focus is on the implementation of the controversial "maximum feasible participation" clause of the Economic Opportunity Act of 1964. In the author's words, "it is an analysis of some of the consequences of an innovative but ambiguous social policy and, on another level, of an attempt by various groups to translate social and political ideologies into action." (p .1) In format the work is similar to Edward Banfield's study of Chicago, *Political Influence.* As in that volume, *Participation of the Poor* begins with a theoretical introduction, which is followed by detailed case studies and concludes with comparative analysis of the case materials presented.

The author, a professor in the School of Social Welfare at Berkeley, obtained his data by means of direct observation of hundreds of meetings as well as by interviewing participants in community action projects. These data were supplemented by newspaper clippings, official government documents (including a wide range of CAP materials), and a large number of relevant scholarly and quasi-scholarly writings. The description and

analysis of such new, complex, and fluid organizations as the CAP's and their neighborhood arms (target area organizations) is fraught with many difficulties. Not the least of these is the problem of assessing the reliability and validity of the findings. Given these difficulties, one cannot help but be impressed by the magnitude of Kramer's research effort and the appropriateness of the data gathering techniques, given the stated goals of the study.

The author argues that four modes of resident participation evolved in response to the "maximum feasible participation" clause of the legislation creating the War on Poverty. Participation took the form of involvement in CAP policy making at the community level, assistance in program development in the target areas, social action (organizing the poor to affect power redistribution), and/or employment in the program. Underlying these various modes of participation were two opposing conceptions of the causes and possible cures of poverty: (1) a notion of individual deficiencies with a resultant emphasis on social service programs, and (2) a notion of structural weaknesses in the society with a resultant emphasis on organizing the poor for a restructuring of power. The case studies make clear that OEO sanctioned all of these modes of participation at one time or another.

The community responses to the prescription to involve the poor varied tremendously. Differences were due to such variables as the political culture of the community, the level of civil rights activity, the type of governmental structure, the scope of previous anti-poverty efforts, the pattern of community decision making, among others. The author characterizes the process of conflict resolution in the five communities as either a debate, a game, or a fight (a la Anatole Rappoport), thus focusing the reader's attention on general questions of conflict resolution and demonstrating the relevance of these questions for the understanding of policy making processes. Contrary to some expectations, "the feared political insurgency of the poor did not really develop in any substantial way" (p. 235) as a result of the mandate to involve the poor. In part, this was due to the fact that the CAP's did not have the independence from the political structure to develop a power base that many believed them to have. For this and other reasons, the author expects CAP's to spend more of their time and money in the future on promoting services rather than on organizing the poor.

Kramer concludes the work by arguing "that there is great need to organize the poor around community development and social action objectives," (p. 268) although he does not see the CAP as the vehicle for attaining these objectives. Instead, he favors the autonomous neighborhood corporation, while at the same time arguing that low-income indigenous organizations (social, religious, civic) that already exist should be strengthened. His prescription seems valid if one accepts the idea that a major problem of the poor (and especially poor minorities) is personal alienation. This alienation is the result of basic dissatisfactions with one's everyday life situation (housing, job, education, family situation, etc.), and may lead to a withdrawal from the political sphere. It may lead to withdrawal of support for political institutions and thus may threaten the legitimacy of these institutions, if the sense of alienation is widespread. Participation in politics and in other organizational facets of life, as suggested by Kramer, may offer the best hope for eradicating feelings of personal alienation among the poor. It is to be hoped that his excellent volume will stimulate more research on organizational efforts among the poor and the functions of this participation for the citizenry and the system alike.—LYMAN A. KELLSTEDT, *University of Illinois-Chicago Circle.*

Maximum Feasible Misunderstanding: Community Action in the War on Poverty. By DANIEL P. MOYNIHAN. (New York: The Free Press, 1969. Pp. 218. $5.95.)

Poverty, Planning and Politics in the New Boston: The Origins of the ABCD. By STEPHAN THERNSTROM. (New York: Basic Books Inc., 1969. Pp. 199. $5.95.)

These two books add further to the growing literature on the problems and politics of social reform in America in this particularly turbulent period of our national life. The principle issues of that reform with which we are concerned revolve around two basic and highly interrelated questions. Can the political system, in the larger sense, produce an operational equality which our political ideology posits? Can we achieve it within a reasonable time span and with means that will not in themselves be destructive of that which we are trying to save? Much of this literature which deals with our recent experiments and experiences in social and political reform offers a recounting of or an evaluation of the "experiments" or demonstration programs launched by the Ford Foundation through its "gray areas" programs, the Kennedy Administration's Committee on Juvenile Delinquency and Youth Crime and, of course, with the poverty programs and efforts spawned originally under the authority of the Economic Opportunity Act of 1964. To this literature these two books make a valuable, interesting and engaging further contribution.

The Thernstrom volume is devoted to a history of the formation and early years of Action for Boston Community Development (ABCD). At

its genesis it was envisaged by local leadership (principally Edward J. Logue, then the new head of Boston's urban renewal enterprises) as the social planning counterpart of, and as a support vehicle for, the physical renewal activities of the Boston Redevelopment Authority. In the mind's eye of its local initiator it was to be an action agency from the word go, and it was to be tied closely to the political leadership and objectives of the administration in City Hall. The interaction of private and public politics and the external forces which shaped the life of ABCD resulted in none of this happening quite as originally planned or conceived. What did happen and why it happened is carefully and thoughtfully detailed by the author in an excellent historical reconstruction accompanied by an insightful and carefully constructed commentary and analysis of why what happened came to pass. In so doing he adds considerably to our knowledge of the politics of community planning.

Thernstrom gives his attention to a period preceding the formal advent of the war on poverty as a Great Society offensive while Moynihan concentrates on how the policies and programs represented by the Economic Opportunities Act came into being, how it (or particularly the "community action" part of it) came to be something that was perhaps unintended and not consciously proposed by Congress and how and why it failed or is failing. (That is, one must infer, until the reforms of the new administration with the author's influence will correct its bearings and bring it back on course.) The two studies intersect in that they both examine the phenomenon and practice of community action, although from different perspectives, and they both explore activities and ideas that were clearly antecedents to and influences upon the anti-poverty program as mounted under the Economic Opportunity Act of 1964.

Thernstrom's study has as a principal concern the tracing out of the influences on current urban policies of the experiments of the early sixties. These were the "gray areas" projects of the Ford Foundation and the "demonstrations" of the President's Committee on Juvenile Delinquency and Youth Crime. Both of these efforts sought to find and fund new types of community action vehicles through which to get the cities of the United States to give sustained and dynamic attention to their urban problems, thus making real and meaningful change possible. While this had already been looked at in a comprehensive macro-way by Marris and Rein (*Dilemmas of Social Reform: Poverty and Community Action in the U.S.*), the early history of ABCD presents a view from the bottom up and, ". . . will yield a different and perhaps no less valuable perspective." One thrust of this study, then, is "to indicate the gulf between local realities and external conceptions of the local scene." (p. x.) The second major theme is to demonstrate, at least via the early story of the ABCD, (the study covers the period from 1960 to 1964 before ABCD became the OEO-CAP vehicle for the Boston poverty program) the limitations of scientific social planning by experts. The author discerns from the ABCD saga, "that the root problems of the modern metropolis are not technical but political, and that expertise is largely irrelevant to their solution. ABCD was a novel and ambitious effort to create a new kind of institution that would both devise and demonstrate the efficacy of new "scientific" solutions to the pressing problems of the city, an effort launched under highly auspicious circumstances. It had political support, civic respectability, professional competence, and a good deal of money. Not only the top political leadership of Boston but the civic elite and key social-service professionals were eager to see this experiment succeed. Both the Ford Foundation and the federal government, in search of a new point of leverage from which to press for the reform of central city institutions, were happy to foot the bills. This promising venture, however, failed to accomplish its central objectives." (p. x.)

The first part of the Thernstrom exposition (from the Fall of 1960 to the Spring of 1962) charts the quest for Ford Foundation funding and the neat adjustments that were made in local thinking and action in order to produce a proposal and an institutional arrangement that would satisfy the funders even if violative of the original intentions of the local initiators. In the process of recounting this story much is revealed about the internal workings of the political system, the theoretical and practical foundations of the Ford program, the interaction of local and national actors and tie-ins of private sector programs with the emerging program at the public level represented by the program of the President's Committee. (The Moynihan volume deals with similar materials.)

The quest for Ford funds overlapped with and was complemented by the struggle for a federal delinquency planning grant. Again the interplay between the local and national actors and the impact upon the local institutional arrangements and ultimately the self-defeating forces that were set in motion by the external forces are fully explicated. In the detailing of this process Thernstrom clearly demonstrates the interconnections between the President's Committee and the Ford Foundation and how this was used to further influence (coerce) the local community to move in a given program and organization direction.

Thernstrom concludes that the kind of institutional change that was the goal of the Ford pro-

gram was not possible because the political power in the vehicle for planning that change and coordinating its implementation internalized contradictory forces that could not be mitigated and lacked the political power to effectuate the needed decisions. He argues, furthermore, that a model which required a lengthy and a careful period of planning with built-in research and evaluation techniques propounded by both Ford and the President's committee was unrealistic in respect to the needs and the forces at work—such rational processes and social experimentations are not suitable to the facts of life (as we are finding out again in the Model Cities program).

How could programs have been formulated and initiated that would have more successfully recognized the requirements of a politics of social change and produced a coalition that could have been "dedicated to changing the institutional structure of Boston in ways that would improve the lot of disadvantaged social groups?" Thernstrom observes (even with the hindsight of ABCD's later involvement with citizen participation community action) that while ABCD could probably not have created a new constituency for itself, it "could have listened more carefully to the voices that were being raised in Boston as early as 1963." He certainly parts company with the inferences one draws from Moynihan when he says, "The real choice may not be order or disorder, but rather creative disorder or destructive disorder. . . " (p. 179) For Moynihan our misunderstanding of "maximum feasible participation" has already brought too much disorder to the local and national political scenes.

Moynihan's main line of argument has appeared in various places previously. Since the release of the book coincided with his entrance into the Nixon Administration it has received especially wide publicity. That great effort at social change exemplified by the war on poverty, he maintains, had by its very nature internal contradictions; and these were aggravated by misinterpretation and/or deliberate misconstruction by OEO administrators. The contradictions and the misconstructions, he further propounds, were not recognized early enough or given sufficient attention by political leaders from the President on down (except for the big city Mayors who did know the score right from the outset). This was mainly so because the whole program was not truly a response to a need demanded by an active and powerfully political constituency. Such being the case it follows that politicians in crucial places did not really participate in the making of the policy. It all occurred because of the professionalization of reform and because social scientists somehow became policy makers and foisted untested, unproved and fuzzy theories or social science doctrine on an unsuspecting or unsophisticated public and national leadership in Congress and the executive branch.

The author also maintains that the misemphasis on maximum feasible participation and the kind of community action it produced—mostly organizing the poor and the dissident against city hall with public funds—dissipated energies, caused loss of support and took attention away from developing the programs that were needed. What was needed were workable and massive employment programs and an income maintenance effort of some kind (mainly to put money in the hands of the poor so they wouldn't be poor and so they would then "participate" as normal actors in the political process). The income strategy as opposed to the service strategy has been propounded vigorously by others too. The question, however, is why were there not such programs? Certainly the proponents of "maximum feasible participation" and a politicized community action program were not opposed to such, and perhaps if adequate money had been put into the poverty war and consequently greater emphasis on jobs and income programs the "excesses" of community action might not have occurred. Indeed, the minimal amounts of community action dollars unearmarked for such programs as Headstart, Legal Aid, etc. might very well have been only a palliative in substitute for the larger amounts that were needed and upon which the poverty program had been predicated. In this respect, then, perhaps failures should be attributed to the Viet Nam war and the priority and attention which the President gave it, rather than to the misinterpretation and maladministration of the "maximum feasible participation" part of the community action programs.

Perhaps this is too hasty a summary and somewhat of a caricature of the Moynihan thesis. Basically the points he makes are well-taken and well-made. The record *would* seem to indicate that the "maximum feasible participation" requirements of the Act were principally intended to provide the entrance wedge needed to insure that Southern Negro needs would not be ignored. That the community action components of the Economic Opportunities Act embraced many possible approaches, that they were perhaps hastily drawn together and that they were primarliy intended as a device to insure coordination of programs and efforts at the federal level through the coordinated comprehensive demands that would be thrust upon the federal establishment from the bottom, are also sound observations. That it became something else, that it took in reality "the form of denying the legitimacy of those institutions of electoral representation that had developed over years the—indeed, the centuries, and which nom-

inally did provide community control" (p. 182) (if such were the case) cannot all be attributed at least by this reviewer, to social scientists stepping out of their proper roles as measurers of the results of policy and acting instead as policy formulators, or to poor or sloppy administration and an inadequate organizational setting for the poverty programs. Perhaps it took the form it did as a reaction or a response to the fact that people were not being represented. A struggle for a sense of community in the midst of complexity (a point which the author himself makes) was really occurring and institutional change at local levels of a radical kind and at an accelerated pace was needed. Presumably the Model City program, with which Mr. Moynihan is now wrestling, had carefully modulated citizen participation features with program responsibility and operation clearly resting with the traditional local institutions of government rather than with paragovernmental agencies (a phrase Moynihan has recently used). Somehow, however, even including the effects of some pretty obtuse federal guidelines, there are still strong pressures for changing existing local government arrangements and adversary proceedings to effect shifts in power and a greater control by constituencies over how and by whom services will be rendered. To say that all of this is just a perversion of what ought to be the way of democratic self-government, does not fully deal with the question of what is happening with local self-government, what the turmoil is all about and what we should do about it. Nor does it face up fully to the basic questions of democratic theory in respect to current practice which, many argue, needs reexamination.—SEYMOUR Z. MANN, *Hunter College, City University of New York.*

The Korean War and American Politics: The Republican Party As a Case Study. By RONALD J. CARIDI. (Philadelphia: University of Pennsylvania Press. Pp. 319. $10.

This is a disappointing book. Written by an historian, it tells us little new about either the Congressional Republican Party's criticism of the Democrats' Far Eastern policy after Chiang Kai-shek's demise or its impact upon the conduct of the Korean War. And its central thesis—when finally stated after 15 pages—is a dubious one. Finally, while the publisher claims the "author's provocative conclusions are particularly applicable today, for it was the experience of Korea that led to the United States' growing involvement in Asia and to her greater reliance on the concept of wars of limited objectives," no tentative comparisons are offered on Vietnam. Indeed, Caridi shows no familiarity with recent political science research on "linkage politics".

His story is by now familiar: the collapse of Nationalist China, its coincidence with Senator Vandenberg's death, the new prominence of conservative Congressional Republicans who were anti-European, unilateralist, and pro-Asian in their orientation, and believed that Democratic domestic and foreign policies ought to be attacked in order to augment their Congressional representation and recapture the White House in 1952. Caridi, like others before him, shows how their charges of "appeasement" and "softness in Communism" made it impossible for the Truman Administration to dispose of Formosa or recognize the new Chinese regime, propelled us across the 38th Parallel and exercised great pressure on the Administration to adopt MacArthur's strategy after Peking's intervention. Finally, having failed to obtain Taft's nomination as their Presidential candidate, they only reluctantly accepted Eisenhower's Korean peace terms.

The thesis is however, novel. Caridi agrees with the Committee on Political Parties of the American Political Science Association which in 1950 advocated a greater centralization of both parties that would clearly express fundamental policy differences and be more publicly accountable. Recognizing that American parties are decentralized and that normally it is difficult to say who are the out-party's spokesmen, he nevertheless asserts that between Vandenberg and Eisenhower the Congressional conservatives presented the Republican foreign policy alternatives. He then concludes that the "party was neither consistent nor sincere(!) in its development of an alternative to the policies formulated by Truman and the Democratic party." First supporting Truman and the intervention, it later fluctuated between suggesting a complete withdrawal from Korea and that we should never have intervened, and supporting MacArthur's philosophy of "no substitute for victory." Finally, the party nominated a man who rejected this viewpoint and supported the limited war effort. "Hence the conclusion that when the nature of a party's dissent indicates that its members are motivated more by political expediency than by a desire to present a consistent and viable alternative to Administration policies, censure is in order."

What can one say about such a value judgment? Even if one deplores the Republican conservatives' criticisms, does the author intend to suggest that in British disciplined party system the Republicans would have acted more on "principle" and presented responsible alternatives? Even before Waltz's devasting attack on Britain's political institutions and post-war foreign policy record, for instance on defense and European unity, this conclusion would have been doubtful. Or is his reference to party expediency meant to imply that an admonition to place the "national

interest" above partisan advantage is the solution? But can a party in a democracy avoid being concerned with the next election, and will it not "sincerely" equate its position with the "national interest"? It is one thing to argue that the outparty's criticism should be "constructive" rather than "destructive", but whether it is one or the other is surely a matter of individual judgment. Caridi does however make it clear, although he seems unaware of it, that the bipartisanship he favors can function best when Administration policies are successful; it cannot survive major setbacks. The resulting critique is bound to seem "destructive" to an Administration. In any event, the author's acceptance of James McGregor Burns' four party thesis defeats his own argument. If both Presidential parties are to the left of their Congressional wings and it is the former who ensure the continuity of American domestic and foreign policies, then one may question whether too much emphasis has not been given to the latter; in the three preceding Presidential elections, the Republican eastern wing selected the nominee and paid little attention to its Congressional party's record.

Lastly, one can only reiterate the disappointment that a book purporting to be a case study whose conclusion(s) presumably apply to other similar cases, and appearing after domestic forces had such a significant impact upon the Vietnamese War, and raised fundamental issues about our foreign policy and allocation of resources, makes no reference to that conflict. The post-Korean limited war advocates of the 1950's had a case; but their strategic prescriptions were always divorced from any analysis of the interactions of American domestic politics and foreign policy in a less than total conflict. This proved to be a fundamental weakness. Unfortunately, it remains so.—JOHN SPANIER, *University of Florida.*

COMPARATIVE GOVERNMENT AND POLITICS

Beyond Eagle and Swastika: German Nationalism Since 1945. BY KURT P. TAUBER. 2 vols., (Middletown: Wesleyan University Press, 1967, Pp. 1598. $35.00.)

Professor Tauber began his research for this work with the intention of analyzing "the factors that make for the survival of nationalism in postwar Germany" and of explaining "why Right-extremism has so far failed to achieve a decisive breakthrough." He came to realize, however, that available sources could not supply plentiful and accurate illustrative materials for this analytical purpose, that "even basic facts were frequently entirely unknown," and that much of the existing data was inaccurate. He therefore decided to produce a comprehensive work of reference as well as an analysis.

The result is an enormous book with a mine of information on right-wing nationalism in postwar West Germany. The entire second volume, some one-third of the whole work, consists of notes and references, appendixes, and bibliography. The bulk of the book consists of a detailed chronicle of the activities of right-wing groups in West Germany after 1945, many of them by now faded from the view of even the most diligent observer of the German political scene. There are also substantial sections about "the antiliberal tradition" in Germany and about the fate of nationalist conservatives under Hitler. The text reflects the third purpose of the author: to show that there are many forms of antidemocratic and antiparliamentary nationalism which are not identical with Nazism, and that "most current generalizations about 'neo-Nazism' " are misleading.

Armed with *Beyond Eagle and Swastika,* the reader could confidently expect to confront a possible resurgent German nationalist dragon with an armor of intricate information at his disposal. This is true despite the fact that the manuscript was essentially completed in 1964, considerably before the electoral successes of the NPD (*Nationaldemokratische Partei Deutschlands*) in state elections in 1966. The name of Adolf von Thadden, chairman of this new party founded in 1964, recurs frequently in Tauber's book in association with earlier postwar right-wing parties or groups, and the author notes in a brief allusion to the founding of the new party that "the NPD stands symbolically as the crown of von Thadden's years of unrelenting effort to pry open the gate to the lush fields of German bourgeois nationalism."

The book is a vast and careful compilation of information, its materials are presented with admirable thoroughness, and its author is very well attuned to the characteristics of conservative nationalist ideologies and political practices. *Beyond Eagle and Swastika* is a substantial accomplishment, and one may predict that it will remain a work of importance for some time to come. Yet both the subject matter and mode of treatment should be viewed against the perspective of other knowledge about postwar German politics and other modes of approaching the significant features of the West German polity. There is in the book a certain tendency to assume that German nationalist movements will come to occupy a far more central role in West Germany in the future than they have in the earlier postwar years.

While this may become true, the materials in the book do not permit one to make such a prediction, nor was it the author's intention to do so. However, the very fact that he has written what is probably the longest work on West Germany to appear in the English language conveys the impression that nationalist movements are somehow central to understanding the potentialities of West German politics.

Had it appeared during the Weimar Republic, a book such as Tauber's would have been extremely informative and useful in comprehending events and their ultimate direction. Whether Tauber's book will be equally useful as a *Baedeker* to future German politics is uncertain. In fact, *Beyond Eagle and Swastika* does little to shed light on possible answers to this question. The impression one gains of the potential central importance of nationalist movements arises less from demonstrated evidence than from the sheer bulk of the materials presented and from the author's use of analogies drawn from earlier German political and intellectual history. The author never says that such movements will come to occupy center stage; indeed, he stresses that he is not dealing with the question "How alive is democracy?" but with the inquiry "How dead is antidemocratic nationalism (and why)?" It was not his intention to "present a full-length portrait of German political life" or to deal "with those massive democratic, social, political, and cultural forces which can be counted on to resist (and are resisting) any recurrence of authoritarianism and integral nationalism." Nevertheless, the author's conceptual framework rests on certain assumptions which are not themselves critically examined, reinforcing the impression of the reviewer that there is some discrepancy between the careful presentation of evidence on the activities of nationalist movements and certain *a priori* views about the significance of these movements for the future of German politics.

Tauber is eminently clear on the variety of views which characterize the nationalist groups. An entire chapter, for example, is devoted to exploration of ideas of nationalist neutralism or "East orientations" among some of these groups. There are long and thorough chapters on the ideas and literature of nationalism, on right-wing youth groups, and on the relation between nationalist ideas and activities and the foreign and domestic policy choices of the Bonn Republic up to 1964. Nevertheless, one is left somewhat in confusion concerning the relative importance or lack of importance of the various views and activities.

The book also contains a number of assertions and assumptions which sound plausible, but whose relationship to the phenomena Tauber describes are speculative, rather than demonstrated on the basis of reference to other aspects of the West German political system and political culture. The author writes confidently of "the extinction in the West of the fire of ideology, after decades of politico-religious fanaticism and paroxysms of homicidal frenzy." Yet he also notes: "We have traced the political failure of the extreme Right as an independent, organized contributor to political decision making in postwar Germany, but we have also recorded the vitality and spread of nationalist thought, ideals, and ideology." His general conclusion is that the potential for realignment of West German politics exists, because of the continued strength of nationalist ideas, because of the growing mobility (or open-endedness) of West German politics, both internal and external, and because an acceptance of liberal democratic values, or a "liberating catharsis never took place among broad layers" of elite or public.

As indicated, these remarks have the ring of plausibility, but one is not sure whether this may only be because they (and particularly the last) are rather fashionable. *Beyond Eagle and Swastika* does not help one to judge whether they are more than that, because its material is not integrated with the dynamics of West German politics. Nevertheless, it provides an indispensable adjunct to the study of stability and change in West Germany.—GEORGE K. ROMOSER, *University of New Hampshire.*

The Nadars of Tamilnad: The Political Culture of a Community in Change. By ROBERT L. HARDGRAVE, JR. (Berkeley: University of California Press, 1969. Pp. 314. $8.75.)

Caste is one of the bed-rock institutions of Indian society, and students of Indian affairs have for some time tried to indicate the impact of caste on politics and *vice versa.* A number of studies, not only by political scientists, but by anthropologists, sociologists, journalists, etc., are already available; and quite reasonably they approach this general problem with varying degrees of emphasis on historical development, on different levels of analysis, on the broader environment within which castes function, and so on. To the very meager list of studies which examine caste groups over a rather wide area (vs. village- or district-based studies), over a long time span, and with a concern for the changing environment over time and space within which castes function, Prof. Hardgrave now makes a very significant contribution, indeed, through his study of the Nadars of Tamilnad, one of the major caste groups of South India (already treated in more limited fashion by L. I. and S. H. Rudolph). In his effort to utilize anthropological, sociological, and political perspectives in tracing the evol-

ution of this wide-spread community over a span of roughly 150 years, Hardgrave has obviously been unable to deal with each facet of his problem in lavish detail; and doubtless specialists concerned with each of the narrower disciplinary perspectives will enter pertinent *caveats* (some of which I shall suggest below). In the nature of the case, this is inevitable, and the shortcomings of this study should not obscure its very substantial merits.

Hardgrave's concern is with the Nadars, a rather far-flung, numerous, and, at the time his inquiry begins, a very low status community in South India. Following a brief but pertinent introduction on the general role of caste in Indian public life, the main body of this study begins with a statement of the position of the Nadars *circa* 1800, with due attention to internal differentiation (characteristic of most, if not all castes), "before substantial change had occurred." Hardgrave then examines "the process of change itself" as the community was (differentially) affected by a variety of forces, including Christianity (to which a number of Nadars converted), education, improved communications and transportation, urbanization and expanding economic opportunities, the rise of national as well as local political movements, and so on. These gave rise to altered community consciousness and self-definition, to new organizations to press Nadar interests, to new occupations, to new and varied perceptions of and involvement in the political environment.

Hardgrave traces the process of change through three stages: (1) the localized, parochial orientation of the "traditional" Nadar community (which still no doubt characterizes a proportion of contemporary Nadars), through (2) an "integrated" phase in which there was considerable social mobilization (which linked together Nadars of different regions, religions, economic conditions, especially through certain caste associations, in the interests of general community advance, first through emulation of higher caste Hindu norms, then through a more secular orientation, which in some cases involved an explicit rejection of these norms), and, finally, (3) to the stage of "differentiation" in which the community became more diverse occupationally, politically, and otherwise, and in which the capacity of the caste associations to speak for all Nadars over a broad range of public issues was severely restricted. The various transformations are well set forth, both in general and through pertinent case studies; and we are confronted with an account of the general, but not universal improvement of the lot of the Nadars, symbolized politically by the position of Kamaraj Nadar as a luminary in the Congress Party. (It should be noted that in making the ascent, the Nadars were involved in considerable violence, which is often overlooked by students of change in India but which here receives great stress.) The concluding chapter of the main body of the work (there are appendices), "The Nadars: An Overview", provides an excellent summary and may profitably be read first by those who want an idea of where Hardgrave's argument will lead them.

There are some difficulties, however. While Hardgrave notes the internal differentiation among the Nadars before substantial change occurred, and while he points at different junctures to increasing differentiation, there is something of a "success story" quality to his account which in my view pays insufficient attention to those Nadars who were left very far behind their "progressive" brethren. (Some brief biographical sketches, illustrative of different Nadar subgroups, help a bit here, but the picture still remains rather vague.) Further, Hardgrave indicates the nature of the conflict (often violent) which accompanied the rise of (a segment of) the Nadars, and in defining the Nadars' own view of the world, he emphasizes their increasingly secular, anti-caste orientation. Yet he does not dwell extensively on the actual behavior of the Nadars towards those non-Nadars who remained below them on the Hindu social hierarchy; and I find it difficult to believe that the Nadars were as generous towards other depressed communities as Hardgrave suggests. Further still (but on a different subject), Hardgrave will not satisfy those who expect to find Almond-Verba type analysis of the political culture of the Nadars, even for the contemporary period. In these and other areas, Hardgrave would have had to expand his analysis in many directions (e.g., to include the sort of intensive, more conventional anthropological studies which would illuminate the first two problems noted above); but this would have been an enormous undertaking, indeed, given the range of Hardgrave's concerns.

One could extend the list of points which would have benefitted from further attention, depending on one's principal concerns and orientations. *Chacun a son gout.* But as it stands, this is an important book for students of Indian affairs (although I would be less enthusiastic than the author of a tribute on the dust-jacket, where we are told that it is "one of the most important and exciting studies in the Indian field in recent years.") Moreover, this work's significance extends beyond the India field, as it delves into the transformation of "natural" or "traditional" associations which are confronted with forces of "modernization". Even if one were to assert the uniqueness of caste as an institution, Hardgrave's study should certainly be of interest to those who are concerned with, for example, the role of tribes in recent African history and even with the evolu-

tion of various ethnic minorities in the United States. Thus, broadly speaking, this volume has comparative significance as well; and it may be read with great profit by specialists whose domain is not India.—HOWARD L. ERDMAN, *Dartmouth College.*

Bureaucrats Under Stress: Administrators and Administration in an Indian State. By RICHARD P. TAUB (Berkeley: University of California Press, 1969. Pp. 235. $6.75.)

From Zamindar to Ballot Box: Community Change in a North Indian Market Town. By RICHARD G. FOX. (Ithaca: Cornell University Press, 1969. Pp. 302. $12.00.)

The two studies considered here engender considerable uneasiness about the capacity of the Indian political system to stimulate rapid economic and social modernization. The pathos of this condition lies in the distinct possibility that the Indian style of politics and administration, while democratic both in form and content, may not be conducive to the promotion of societal change.

Thus, one of the prime instrumentalities of the Government of India in attempting to modernize that society has been the elite cadre of administrators recruited into the Indian Administrative Service (IAS) mainly by national competitive examinations. The fewer than 2,000 men enlisted for this directive corps of the national and state bureaucracies are relatively westernized in their orientations and trained to assume an activist role. But the IAS today operates in a cultural and political environment where many of the bureaucratic values incorporated in its recruitment, training and service patterns contribute little—if they do not actually impede—the successful pursuit of economic and social change. This situation constitutes an important theme of Professor Taub's valuable study of the IAS cadre assigned in 1962–63 to duties in the State Secretariat of the relatively backward state of Orissa.

Taub's book, as a whole, provides important insights into the attitudes and patterns of behavior of the Indian bureaucratic elite—a subject which has received surprisingly little systematic study heretofore. Extracts from his indepth interviews with the 28 IAS officers "attached" to the Orissa Secretariat and his observation of the activities of the state bureaucracy are the most striking features of the book. These materials are supplemented by elite interviews with forty other persons: members of the Orissa (State) Administrative Service; leading state politicians; businessmen; academics; and government engineers.

While the salaries and perquisites of the IAS officers are large and they remark favorably upon the security provided by their positions, few derive personal satisfaction from the *content* of their work. Their decision-making authority is increasingly circumscribed by state and regional politicians; frequent directives from the national government calling for programmatic innovations must necessarily meet with inadequate administrative responses. These responses grow not only out of the political pressures exerted upon the top administrators but out of their own career patterns and the bureaucratic structures within which they function. Their generalist training and brief tours of duty at specific tasks prepare them poorly for dealing with the technical demands of development administration; the careful planning and staged execution of large projects is almost impossible for men assigned to their posts for three years or less. Since the system rewards even those who risk little (promotion is assured and discipline for poor performance is rare), few incentives exist for those who might attempt to cut through the maze of red tape and confusions of responsibility which have resulted from a rapidly expanding, but poorly coordinated and partially politicized, state bureaucracy.

Basic to the difficulties encountered by some of the IAS officers is the fact that those from outside of Orissa are treated as "foreigners" and, in turn, insulate themselves from the people of the state and from those members of the service drawn from Orissa. The latter, in contrast, tend to be implicated in inter-group conflicts characteristic of the region. Social differences within the IAS exacerbate an administrative situation in which mistrust is already rife and subordinates tend to respond to this climate by resorting to administrative inaction or by taking refuge behind rules.

Under the circumstances, Taub is justifiably pessimistic that the IAS can act as an organizational vehicle for major societal change. Recognizing that neither dissolution of the IAS (to end local resentments against "foreign" administrators) nor greater centralization of bureaucratic control is likely to be a real solution, he concludes on a somber note—a note, fortunately, which does not follow entirely from the limited scope of his data:

> Unless the bureaucratic organization can contract, emphasizing those tasks that it can do well, while creating independent sources of initiative to manage those it cannot, the administrative apparatus of the state will one day collapse, prey to the rage of its hungry and frustrated people, who will turn to demagogues to accomplish what their present national leaders have so far failed to do. (p. 203)

Whether such a harsh forecast is warranted depends upon one's understanding of the character of Indian society in general. Professor Fox's study does little to lighten the burden of Taub's conclusions, although his concerns lie less in the future than in the past.

Tezibazar is a market town of 7,200 people in the state of Uttar Pradesh. During 1963–64,

Richard Fox studied its social structure, its economy and the nature of local political conflicts. He employed two surveys of businessmen to gather some of his data about the economic behavior of the town, but most of his materials are derived from direct observation and from informal and unstructured interviews.

According to Fox's informants, Tezibazar was created four centuries ago as a commercial enclave serving the interests of an agricultural region dominated by local overlords (*zamindars*). Fox argues that the town has no history of an integrated social or moral order. What integration existed prior to Independence was provided by the zamindars who controlled local positions of power and by the caste organizations which regulated the social interactions of the segmented constituent elements of the town. During the late nineteenth century, the influence of the traditional zamindars declined; they were partially displaced by descendants of the merchants they had originally induced to migrate to the town to conduct its commerce. Today, Tezibazar also exhibits a social atomism very different from the relatively coherent caste, ritual and economic structures anthropologists have found in some Indian villages.

Here lies whatever criticism might be leveled at Fox's approach. Rich in detail on such matters as caste relations, the specifics of political conflict, and economic behavior, this is a complex account of the multiple dimensions of a population aggregate stranded between village and urban forms of organization. As an anthropologist interested in social structure, however, the author tends to view the town from the perspective of its inadequacies as a village. Fox despairs over the low levels of economic cooperation and commercial morality in Tezibazar; such behaviors are matched in politics by a constantly shifting series of alliances based both on traditional and expedient intra-caste and inter-caste cleavages, conflicts between Hindus and Muslims and interpersonal feuds. The existence of such conflicts and the absence of an integrative social structure have opened the town to substantial participation in the party politics and economic life of the state and region. Yet, in a style reminiscent of Vidich and Bensman's *Small Town in Mass Society*, this is not a prospect which Fox views with any equanimity.

There may be some reluctance on a reader's part to accept Fox's standards of evaluation, but the study is in itself a major contribution to the the literature available on Indian urbanism and is likely to be of value to anyone interested in the economic organization in a transitional society. Fox's sometimes acid portrait of town life as typified by the unscrupulous behaviors of its merchant castes is hardly likely to provide comfort to those wishing to view the prospects for economic modernization in India with optimism. Caught between a subsistence economic mentality which discourages enterpreneurial activity and a social ethic which encourages a highly expedient approach to interpersonal relations and to public life, the capacity of a democratic political system to generate fundamental changes in an economy and society like India's appears at least rather limited.—DONALD B. ROSENTHAL, *State University of New York at Buffalo.*

Egypt Since the Revolution. ED. BY P. J. VATIKIOTIS. (New York: Frederick A. Praeger, 1968. Pp. 195. $6.95.)

There has been so much unfortunate misunderstanding, as well as controversy, about Egypt under Nasser that all serious studies on the subject, such as this small volume, are to be welcomed. It includes papers prepared by a number of scholars from the United States, Britain, France and Egypt who participated in a small working conference held in September, 1966, at the Centre of Middle Eastern Studies, London University, to discuss economic, political and cultural problems and developments in Egypt since the overthrow of the monarchy in 1952.

The book is divided into four major parts. In Part I, which deals with Egypt's economy, Professor Bent Hanson of the University of Californnia, Berkeley, discusses and evaluates the first "comprehensive" five-year plan covering the period from 1960 to 1965; Professor Galal Amin of Ain Shams University, Cairo, traces and analyzes the changes which took place in Egypt's economy after 1952; Professor Roger Owen of St. Antony's College, Oxford, reviews agricultural developments in Egypt from 1890 to 1939; and M. Riad el Ghonemy, on the staff of the Food and Agricultural Organization in Rome, examines the major changes which have taken place in Egyptian agriculture since 1952. This section contains much useful information, many valuable tables and discerning analyses and conclusions. While considerable stress is given to such major topics as economic planning, rates of economic growth growth and especially agricultural developments since 1890, one could have wished for more details and discussion on other important aspects of Egypt's economy, such as the industrial sector, the causes for foreign exchange difficulties and the impact of foreign aid, foreign relations and defense needs on the economy.

In Part II, covering the evolution of Egyptian politics, Professor Maximé Rodinson of the Sorbonne presents an astute and lively study of political developments in Egypt since 1952. He notes that the principal motivations of the Free

Officers in overthrowing the monarchy were, first, to secure complete national independence and, secondly, to modernize economic, social and political life in Egypt. He shows how politically naive the Free Officers were at the beginning and how greatly they had underestimated the obstacles to the achievement of the advanced political, economic and social reforms they had in mind. He explains how and why these officers then tightened their political grip on the country and how Nasser emerged as the undisputed leader. Professor Rodinson correctly points out that developments outside Egypt helped to shape internal developments. Nevertheless, more stress could have been given to the serious repercussions which a number of external developments—such as the British-French-Israeli attack on Egypt in 1956, the various Arab-Israeli disputes and the Yemen—had on Egypt's domestic policies.

Also in Part II, Professor Malcolm Kerr of the University of California, Los Angeles, discusses and analyzes, with perception and objectivity, the most sensitive and controversial subject in the book: Egyptian foreign policy under President Nasser. On the one hand, Professor Kerr criticizes Egyptian policy for its "equivocation" and "shifting" nature and for its occasional "ambitiousness" and "activism" beyond the nation's needs and capabilities. On the other hand, he holds that Nasser's foreign policy has been more pragmatic and flexible and less influenced by "abstract preconceptions, emotions, or megalomania" than many "hostile quarters' have contended and that his major foreign policies had their "origin not in activism or ambition, but in long-standing Egyptian national interests and facts of life that no Egyptian leader of any political colour could have ignored from 1952 onward" (p. 118). He indicates that Egyptians generally have much more in common—especially culturally and ideologically—with the West than with the Communist world and that Egyptian policy is "potentially in direct competition with long-range Soviet objectives as the Egyptian government estimates these" (p. 132). Professor Kerr not only provides a great deal of essential information and valuable insights, but he also disposes of many long-standing myths widely believed in the West about Nasser's foreign policy.

In the final contribution to Part II, Kaled Mohieddin, former member of the Free Officers Executive, questions some of Professor Kerr's analyses, at times somewhat overcritically, and he presents his own personal views. He insists that Egyptian foreign policy after 1952 was not merely a continuation of former policies but was, in fact, revolutionary in character. He denies that it was either ambiguous or contradictory. He contends that to avoid being isolated and endangered, Egypt has had to support at any cost those countries in the Arab and Afro-Asian worlds committed to the same political and social objectives as she, and that Egypt would always follow a policy of non-alignment towards both the Communist and Western blocs.

In Part III, Louis Awad, cultural counsellor to the Cairo daily ALAHRAM, provides an interesting paper on cultural and intellectual developments in Egypt. He states that, while the Egyptian revolution produced little "speculative, analytical and critical theory in the fields of political science, economics, sociology and moral philosophy," the revolution has, nevertheless, "given tremendous impetus and wider scope to the creative urge in writers and artists," and the government has patronized "all forms of informal education and non-academic culture" (p. 157). He indicates that the greatest advances have been made in the fields of drama, poetry and plastic arts. Professor David Cowan of the School of Oriental and African Studies, London, discusses literary development in Egypt since 1952.

Professor Derek Hopwood of St. Antony's College gives, in Part IV, a survey of some Western books on Nasser and Egypt after 1952. The survey is reasonably comprehensive, fair and useful.

Since these papers were prepared in 1966, they naturally did not take into consideration those major changes—especially political and economic—which were brought about by the June 1967 war. Nevertheless, they provide much valuable information about and insight into an important country that has played and will continue to play a key role in Middle Eastern affairs.—FRED J. KHOURI, *Villanova University.*

Business Associations and the Financing of Political Parties. By ARNOLD J. HEIDENHEIMER AND FRANK C. LANGDON. (The Hague: Martinus Nijhoff, 1968. Pp. 247. Guilders 32.50.)

The reciprocal relations of state and society have been a central and baffling problem of politics. Does the power of the state control the economy or do the managers of the economy (forces of production) determine the nature of the state? Since Marx's oversimplification a century ago this theme has been recurrent in traditional political theory. Nowhere do the respective contentions come into clearer focus than in the issues of political finance. The democratic multiparty state is essentially a propaganda polity with various group interests competing for the consent of the governed through the means of the increasingly technical processes of persuasion. The contemporary sovereignty of the camera makes access to the media both imperative and expensive. With neither profits nor taxes to support them how do

parties and political leaders acquire funds to conduct campaigns of propaganda to win possession of the machinery of the state.

Into this controversial area of politics and economics Professors Heidenheimer and Langdon have undertaken a specialized and rather involved study of *Business Associations and the Financing of Political Parties.* Their inquiry represents an effort at a transnational investigation of a common problem in postwar Germany, Norway and Japan. The authors restrict themselves to the organized activities of business men with only passing reference to countervailing organizations such as trade unions. In fact the establishment of associations for collecting funds from business men (Capitalists) for the purpose of funneling them through the "conveyer" agency to political parties was begun by Libertas in Norway in 1947 within two years of the end of the war, while this kind of enterprise developed in Germany and Japan in the 1950's. The three undertakings lasted for approximately a decade in each country. In Norway where the reviewer studied the problem at the height of the controversy, Libertas, originally begun in secret, was established partly to offset alleged restrictions upon printing materials exercised by the existing Labor government under rationing continued in the postwar period. An investigation by the Storting instigated by the Labor Party, amounted to little. Libertas transformed itself into a pressure group more important as a propaganda agency conciously making snob appeals to Norwegian secretaries to identify themselves with management rather than with labor. However, in 1965, during parliamentary elections which unseated Labor for the first time in thirty years, the Labor Party secretary felt sure Libertas was a primary element behind the scenes in the overturn though party splintering and weariness with old hat Labor leadership was an important factor. In Germany, public subsidy helped destroy the conveyer system. In Japan, not subsidy but decline of interest helped defeat conveyerism. Even the Socialists had received funds from the centralized collections. The failure of the conveyer scheme may be attributed to many elements but not the least of these is love of independence among politicians who do not like to be considered the "kept women" of an elite of managers. In fact the resentment of conservative politicians for business men sometimes resembles the dislike of trade unionists by Labor politicians. Frequently their reaction resembles the feeling of intellectuals towards their benefactors, the Foundations. Reicipients become weary of being perpetual mendicants.

Though the authors have done their research in the best traditions of political science, even accompanied by some of the patois of those scholarly imperialists, the political sociologists, they freely confess to the limitations of their findings. Certainly their conclusions do not verify the oversimplifications of the Marxists for they discover no uniform laws governing the conveyer process of political finance. Nor are there clear insights for those who still have to cope with the problem of party finance. There is a hint at public subsidy. The authors might well have mentioned the different treatment of political finance operating in economies without a Sherman anti-trust act. The Japanese were compelled to adopt American campaign finance publicity features at the instance of the great American democratic Mikado, Macarthur. Professor Harold F. Gosnell could illuminate this portion of history with what happened when the Japanese asked the practical question how or who will pay for the political parties provided for in the Japanese constitution of 1946.

All students of political finance will welcome this careful study to be added to the growing body of literature upon a primary problem in the propaganda state. In the name of more rigorous discipline a better proof reader would have avoided many misspellings which are widely distribtued throughout the volume.—J. B. SHANNON, *The University of Nebraska.*

Federalism and Fiscal Adjustment. By R. J. MAY. (New York: Cambridge University Press, 1969. Pp. 192. $7.75.)

Probably no area of government has received as much attention in recent years as federalism. W. Brooke Graves is in the process of getting a bibliography on federalism ready for publication, and it is likely that it will contain over 600 items. When yet another book comes along to be added to the list, it inevitably runs the risk of being ignored or overlooked—or of being read with an especially critical eye by a bored or jaded reviewer. This little book should not meet that fate. A revised thesis for the D. Phil. at Oxford, for which the examiners were themselves leading students of federalism—Sir Kenneth Wheare and Professor A. H. Birch—it provides both an analysis and some insights which set it aside from the run of the mill studies of federalism on both sides of the Atlantic. Intrigued by the universality of differences in size and wealth among units of federal systems and by recurring problems of stability in many federal states, May sets for himself the task of answering two main questions:

1) how do inequalities in size and wealth between units affect the fiscal relations between governments in federations
2) to what extent can revenue transfers be-

tween governments be used to promote political stability in federal systems.

To answer his questions, May first examines the decision-making process in federal states, which analysis is a very perceptive one. He finds that process to be largely one of bargaining. He then turns to fiscal adjustment, which he sees as constituting one of the major factors determining the relative bargaining position of governments within federal states. Finally, he examines with a good deal of insight intergovernmental financial relations in 12 nations with particular reference to the distribution of revenue between richer and poorer units and to the role of finance as a stabilyzing or destabilizing element in the political life of the particular country. The final chapter is devoted to conclusions.

Although he finds (not surprisingly), "that every federation is to some extent unique" and thus that nothing "but fairly tentative generalizations" (p. 161) can be drawn from his analysis, he does offer new light on a couple of long-accepted dogmas of federalist lore. The evidence he has marshalled, for instance, convinces him that encroachment by the central government through the fiscal system on the powers and responsibilities of the units is either a gross simplification or entirely misleading. Much closer to the truth is a split between the poorer units, who tend to favor a strong central government with power to redistribute revenue in their direction, and the richer units, who favor fiscal and other decentralization and something close to a balance of powers between the central government and those of the units. Recent developments in Canada would seem to offer proof of his assertion. With regard to the role of fiscal adjustment (by which he means chiefly revenue transfers) in helping to secure political stability, he finds the evidence not nearly so conclusive. Nor is it clear that one type of revenue transfer—grant-in-aid, bloc grants, grants based on a fixed formula—is better than another in terms of its effect on the decision-making process in the receiving unit. The effect, indeed, of none of the ordinary types of grants on political stability is at all clear. May's final conclusion is that

> "Just as there is not in any federation a lasting concensus about what federalism implies in terms of the rights and obligations of its members, there is no universal principle according to which financial arrangements can be fixed to the permanent satisfaction of all the governments." (p. 168)

May's conclusions are thus not startling and require no readjustment in thinking about federalism. However, both his preliminary analysis of federal decision-making and his careful surveys of leading federal states—four "classic" federal models, Australia, Canada, Switzerland, and the United States, and eight newer models, West Germany, India, Malaya and Malaysia, Rhodesia and Nyasaland, Nigeria, French West Africa, the West Indies, and Yugoslavia—although in some cases necessarily superficial are very helpful to the student of federalism. This is not an easy book to read in either style or substance, and it will suffer from early obsolescence, so rapidly are changes taking place even in the so-called "classic" models of federalism.

With specific regard to the United States, but perhaps generally, one wonders why no attention is paid to local units of government—cities and towns—and to their role in the federal system. It is all well and good to paint a legal picture in only two colors, but as anyone familiar with the United States at least, especially since World War II, knows, cities have become claimants and on the very grounds of financial inequality, or indeed poverty, to which May addresses himself. On p. 99, in noting that "the governments of the poorer states have been relatively weak pressure groups at the federal level," he goes on to say that this has been the case because "more important pressures on the national government have been those of regional or national functional interest groups . . . ." Although he does suggest that "urban interests" have exerted some pressure, he does not define the term nor go on to demonstrate the extent to which cities are now employing lobbyists to win a greater share of federal funds for themselves or how the war on poverty or model cities programs were responses to that pressure and to the effect of such programs on balance within the American federal system. Indeed, in American federalism the struggle for fiscal adjustment has pretty much shifted to the nation's urban areas, and it is likely to remain there into the indefinite future. Thus any study which confines itself to federal-state bargaining alone misses much of the story. I suspect the same remarks could be made about a number of other federal states, probably at least of Australia, Canada, and West Germany.

But as I had occasion to note in another connection not long ago, "Federalism is a vast and complicated subject . . . To master it completely . . . [is] a project for a lifetime, if even so long be enough . . ." We are still far from understanding the phenomenon in all its parts, and May's book cannot be faulted for not by itself completely filling the void. It does bring us a little closer to understanding and opens the door of investigation to a fruitful field of inquiry. So it should be applauded and recommended, and what is more, read.—RICHARD H. LEACH, *Duke University.*

The Politics of the Barrios of Venezuela. By TALTON F. RAY. (Berkeley: University of California Press, 1969. Pp. 211. $7.00.)

In this valuable study of the barrio phenomenon in Venezuela, Talton Ray attempts to link patterns of barrio development, the urban environment, and the roles of municipal and state governments and political parties, to explain barrio politics. Ray's effort, the first book-length study focusing directly on this important subject, achieves commendable success.

The problems and the potentials of the urban poor, particularly as these affect underdeveloped countries, have been the subject of numerous analyses. Myriad interpretations result, ranging from the optimistic to the abysmally pessimistic. But much of the ground that Ray ploughs has been explored inadequately in previous research. Many of the interpretations and conclusions of other studies suffer from ethnocentric bias of middle-class observers or from the "standard myths" about which William Mangin and others have written.

Talton Ray bases his study on two and one-half years of work in community development in the Venezuelan environment. That experience, although perhaps lacking the formal structuring of a planned research design, at the same time lends a useful additional dimension and scope to the study. Ray also incorporates the limited available data (such as the CENDES-CIS study of the Universidad Central de Venezuela and MIT) on attitudes and behavior of Venezuelan barrio dwellers. In addition, he draws upon the project reports of workers in ACCION en Venezuela, the private community development organization, and upon censuses taken by barrio leaders. The author was thus able to observe barrio politics in a large number of different locations. Altogether 130 barrios in sixteen major cities and towns provide the setting for his analysis: however, much of the emphasis in this study is directed toward four major urban centers of Venezuela, Caracas, Maracaibo, Ciudad Guyana, and Valencia.

Ray divides his study into three parts. Part I treats the migratory patterns of barrio development in Venezuela and general social, economic, and physical conditions of the barrios. Part II analyzes the effects of the urban environment and modernization on political behavior, attitudes, and leadership patterns, particularly as the government and political parties are involved. Part III is an assessment of the role of barrios in national politics, with especial attention to the political integration of the barrios into the national political system.

Ray's analysis takes account of the vital importance of urban land distribution as a political issue in Latin American countries, a factor often overlooked in the prevalent focus on rural agrarian reform. Recognizing the permanence of the barrios (barriadas, favelas, callampas, villas miserias, etc., in other Latin American settings), he analyzes the differing approaches of political parties towards them and offers an explanation of the varying patterns of leadership characterizing different barrios. The crucial variable operating in connection with choice of leadership is seen to be the economic, social and, political character of the city where the barrio is located. Leadership appears to change as the cities are transformed by the modernization process.

Despite the power potential of the barrios in Venezuelan politics (between one-third and three-fourths of the population of major Venezuelan cities live in barrios), most cooperative action—including direct political action—is initiated by outsiders. Those outside mobilizers have been principally government agencies, labor unions, and political parties. Confirming this, Ray found that elected barrio juntas are more likely to represent the interests of the government than the barrios themselves. Widely practiced partisan discrimination has a strong effect on the political attitudes of the barrio residents.

One of the difficulties faced by Ray in his study is the problem of defining and identifying barrio influence. If specific political decisions could have been isolated, and if structured analysis of attitudes and behavior could have been focused on those decisions, Ray's study would be even more valuable than it is. Even so, he shows convincingly how the political parties have become aware of the enormous political power potentially available in the barrios, and how such power probably has restrained the military from direct intervention in the civilian governments of Venezuela since 1959. But the existence of this potential has not produced notable changes in barrio political behavior, at least not to the point where barrios become violent and aggressive in demanding payoffs for their political services. Ray speculates that this form of behavior stems in part from the fact that many aspects of barrio life are not only tolerable for first generation residents but—contrary to the mythology—are actually appealing. The future danger may reside in the frustrated ambitions of second generation residents who cannot personally appreciate the relative advantages of urban over rural life, as could their migrant parents.

In sum, Ray's book stands as an important study which represents a major step toward filling the many gaps in the area of urban politics of Latin America.—JACK W. HOPKINS, *Emory University.*

Political Parties in Action: The Battle of Barons Court. BY ROBERT T. HOLT AND JOHN E. TURNER' (New York: The Free Press, 1968. Pp. 311. $7.95.)

It is not likely that *Political Parties in Action*

will become a political science classic, but it has intrinsic value which should be called to the attention of students of both British and United States political parties. The book reports the observations and analyses of the co-authors from within opposing constituency-level Labour and Conservative organizations as they vied for a marginal parliamentary seat in 1964. The authors aimed to study the impact of local party organization on election outcome, to "determine what holds the constituency organization together and explore how the systems operate." (pp. 9–10) The fly leaf tells us the book is an empirical study, and although it has its share of detail, this comes in the seventy-five percent of the text in which the case is described. The work has deficiencies as a scholarly study, but the research technique, and the sections of the final chapter dealing with internal party organization made it a worthwhile experience for the reviewer. The fact that this chapter is based not on the 1964 research but on interviews conducted in 1966 does not detract from the chapter's value.

The book has three important deficiencies and a number of minor unanswered questions. The first major deficiency stems from the research technique. It is a case study of a single election with all the idiosyncracies of historical setting. As with all case studies, this one is limited in its generalities. The authors refer to their conclusions as generalizations and hypotheses, but they are not generalizations in the true sense. They are explanations of the case at hand. An illustration of this type of generalization is the Holt and Turner conclusion "in a general election a significant portion of the most involved Tory activists are interested in becoming parliamentary candidates." (p. 264) This hypothesis follows from the fact that of the Conservative workers most active "in both the 1964 and 1966 campaigns, no fewer than nine had hopes of someday winning a seat in parliament." (p. 254) No evidence is presented to indicate that the "generalization" refers to other constituency-level Tory organizations.

This example serves to illustrate a second criticism: (aside from the misuse of a statistical term): the use and absence of data. It would be valuable to know the size of the population of which the nine Tories were a sample. Indeed, through the discussion of the dynamics of party organization, there is a conspicuous lack of detailed supportive data. We are told that the Labour organization was more effective and efficient than the Conservatives but we do not know whether this is due to the relative size of volunteer workers (not reported) or to more efficient workers (not measured). On the other hand, the background and description of the election occasionally lapses into painful and unnecessary detail.

Some of the interpretation of data is not convincing. The authors take issue with Richard Rose's dismissal of British party canvassing as unreliable, and claim they found an "amazingly high" agreement between the party's canvasses. However, the error level in the canvass results exceeds 12%. One could argue that such a high error level supports Rose. A crucial campaign decision based on such unreliable results could easily alter an election outcome in an unpredictable way.

The final important flaw in the book is the failure of the authors to link their work to the wealth of existing theoretical literature on organizations and political parties. The congruence of the parties' internal structure to Downs' model is obvious. Each party seeks only to win the election; ideology is not an end but a means to get votes; party workers are self interested only incidentally promoting the party goal, etc. Simon's and Olson's treatments of incentive systems and Olson's typology of groups would also help to put many of the study's conclusions in context. Holt and Turner offer the insights and the information for a systematic look at parties as organizations, but it is regrettable that they did not seek to couch their conclusions in a theoretical framework.

Other criticisms are minor, being questions of style or organization. Particularly frustrating is the frequent use of the term "knock up", and one does not discover until page 201 that to "knock up" a voter is to urge him to vote on election day.

The evidence is not presented, but the battle of Baron's Court probably is fairly typical of a campaign in a marginal parliamentary constituency. And campaigns in marginal constituencies determine who shall rule in Britain. As the authors point out, fewer than fifty seats were won in 1959, 1964, and 1966 by more than 1000 votes and the outcome in these determined the majority in Parliament. The book, in covering one event clearly and comprehensively, offers a model of all these campaigns.

For the student of United States political parties, Holt and Turner offer a contrast. Where in the U. S. do our parties get 200–500 volunteer workers in a constituency of 30,000 voters? Only very rarely in the United States is a 100% canvass used either for information gathering or for soliciting support, yet it is apparently common in British marginal constituencies. Other comparisons are possible, each leads to a better understanding of the respective party systems. Thus the value of the study is as a heuristic device. As such it probably will achieve some success.—JOHN C. BLYDENBURGH, *Rutgers University.*

Northwest Tanzania under German and British Rule: Colonial and Tribal Politics, 1889–1939.

By Ralph A. Austen. (New Haven: Yale University Press, 1968. Pp. 307. $8.75.)

Prof. Austen is representative of a new generation of historians of Africa whose work seeks not only to fill the enormous gaps in our knowledge of Africa, but even more important, to examine the colonial period from the perspectives of the Africans as well as the Europeans concerned. This is, it must be pointed out, no mean feat, since it involves painstaking recreation from highly limited written, blurred traditional, and indistinct eyewitness accounts and sources. It has been said that history is the record of the winners: that truism, cliche though it is, rings even truer for Africa: the winners not only could write, but recorded their heroic deeds, feelings, rationalizations, and interpretations *ad nauseam*. The bulk of sources for the colonial period in Africa is, then, the written record of the colonialist, the colonizer, and the imperial power. It is to Mr. Austen's credit that he seeks to present his picture of the contact and interaction of Europeans and Africans in an area of northwest Tanzania by focussing on the interaction as it affected and was seen *by each group*. And it is a fascinating story that unfolds.

The focus of Prof. Austen's study are two major ethnic groups living along the southern and southwestern shores of Lake Victoria, the Haya and the Sukuma. In a short but well constructed chapter, the author examines the traditional politics of the area before colonialism; he treats of the first contact between Europeans and Africans along the Lake beginning in 1961, and then devotes three chapters to German rule and its consequences. The Germans, it appears, were no better or worse in their colonial management than any other colonial power of the time; and their record was as spotty: military subjugation, commercial exploitation, brute coercion at some times, genuine concern, "enlightened" paternalism, willing accommodation, at others. And throughout this period both the Sukuma and Haya, though subject to an alien power, retained their ethnic integrity by timely adaptations and resistance to the demands and pressures put on them. Both groups, however, found German rule easier to bear after 1904, when the Dernburg reforms changed the policies and performance of German colonial administration. The Germans were pushed out of what is now Tanzania in 1916 after a military campaign marked by the extraordinary guerrilla resistance of Von Letow-Vorbeck, and the British took over. Among their baggage was the policy of "indirect rule"—which had been practiced by the Germans without the designation—by which the British sought to use existing traditional hierarchies for the performance of modern—i.e., their—political tasks. In any case, the Haya-Sukuma areas were to be model applications of Lord Lugard's celebrated scheme, and it is the tug and pull of the British and the Africans in relation to that set of policies that takes up much of the rest of the book.

Again, to make the point, Prof. Austen's history is as fairhanded as colonial history can be. I was impressed by the fact that he not only combed all the conventional and unconventional written and published sources, but sought out some twenty-five oral accounts by Africans, some of them going back sixty years and remarkable for their detail and accuracy. Prof. Austen's recreations have, in this sense, the stamp of a versimilitude not present in the more conventional histories. Moreover, he tells a good story, a matter which, to the non-historian-acting-as-reviewer-of-a-work-by-an-historian, still makes a deal of difference.

In all, then, this is interesting, well-done history. It gives the political scientist some novel insights into the on-the-spot workings of colonial policies and provides a welcome re-assessment of the impact of those policies upon the Africans, hitherto mute in the pages of colonial historians. —Victor T. Le Vine, *Washington University, St. Louis.*

Chinese Government in Ming Times: Seven Studies, Edited by Charles O. Hucker. (New York: Columbia University Press, 1969. Pp. 285. $12.00.)

In spite of its derivation from a 1965 "Research Conference on Ming Government," this anthology is no symposium. The essays are on widely disparate subjects, uneven in quality, and they do not speak to one another. One essay was written for another occasion and published elsewhere before the conference. All this plus the nature of the subject suggests that an introductory essay was essential for the integrity and value of the volume. The editor was of course eminently suited to sew these essays together, to make them enter a dialogue, or simply to give the reader an indication of how the essays respond respectively to whatever common themes they share (for example, the issues of centralized vs. local political power, or monarchy vs. bureaucracy).

The first essay, by L. S. Yang, does not serve this integrating or introductory function for the volume. This is so primarily because it addresses itself to problems to which no one else here speaks directly; and again, its treatment of attitudes toward centralization and localization of power, back and forth across a millenium of Chinese history, is so meandering and choppy that it is difficult and uncomfortable to read—even for one

familiar with the technicalities. This is exacerbated by the lack of a summary or concluding statement for the essay. It just ends. And that's no way to begin a volume like this which, in the editor's words, is supposed to give the nonspecialist reader " . . . gratifying glimpses into the detailed workings of a strange governmental system grappling with universally understandable problems."

To the readers of this journal, Jung-pang Lo's essay will perhaps have broader appeal than the others, for it attempts to speak in high enough generalizations with concepts generated in the ambiance of Western political studies. His subject is decision making and the issue is whether or not decision making (and implementing) power was the monopoly of the monarchy. He focuses on "issues respecting peace and war" and argues the feasibility of using R. C. Synder's *modus operandi* for this period and with the available data. Though he leads us towards a knowledge of the plurality of decision making bodies and situations during the Ming, his case is weakened precisely because he does not apply to his fourteen pages of case histories the "behavioral approach" which he trumpets at the outset, and which was to focus on the personalities, the social economic status, and states of minds of the decision makers. As a result, we see the decisions on peace and war were made outside of the court, but we don't know why, according to those abovementioned factors, nor do we know if there are attendant patterns.

Ray Huang's essay on "Fiscal Administration During the Ming" is a refreshing, ambitious *tour de force* which is the only essay in the lot that combines command of sources and detail with a sense of scope and dynamics. He knows how to analyze fiscal policy so that someone other than a Ming specialist can understand him and he is capable of using specialized data to adumbrate some of the most central of the perennial problems of Ming administration. His discussion of the role of the ministry of revenue in the gathering and disbursement of taxes is a lucid example of the relationship of the central government to local administration. Though his conclusion to the essay outruns his presented data (a fact he acknowledges), like the essay, it is full of fascinating hypotheses and questions to which future thought about the Ming polity could profitably respond. It is curious that Mr. Huang only raised the issue of the growth of a money economy during the Ming in his conclusion.

In "Academies and Politics in the Ming," John Meskill somehow seems to avoid any questions or conceptualizations that might lift his narrative above mere description. He does not, for example, as did C. O. Hucker (the editor) a decade ago, ask whether these prominent academies had some affinities along class, regional, or other extra-intellectual lines. Periodically, the central government systematically suppressed them. But why? Just what interests did the academies represent? Did they have anything to do with the burgeoning Ming economy and the shift to a money economy? Could we perhaps understand them better if we analyzed them as political parties, or pressure groups, or lobbies? How powerful were they, and in what way? These are questions begged by the essay that must be treated if the charts and tables and dates are to have any meaning for someone who wants to know something about political processes and thought in traditional China.

In "The Ming Dynasty Bureaucracy," James Parsons uses descriptive statistics to define the contours of bureaucratic structure and power. Though he has some evident troubles moving from statistical data to concepts, his description of the regional and clan backgrounds of the bureaucracy, for example, is quite instructive in a simple, direct fashion, and makes some headway toward discussions on the relationship between central and local political forces and on other questions begged in the Meskill essay.

Sinological narrowness, for the most part, will prevent the potential insights of the studies in this volume from being communicated to an audience much broader than "Ming specialists." Lax editing has permitted, among other things, the inclusion of pieces which are fragments, or marginalia and not proper essays. This volume does not reveal the considerable talents of its contributors in the most flattering light, and thereby it fails to do justice, even as a "sampler," to the young (but not necessarily callow) field of Ming studies.—LAURENCE A. SCHNEIDER, *State University of New York at Buffalo.*

Toward Strategies for Public Administration Development in Latin America. By JOHN C. HONEY, with commentaries by PETER D. BELL, RICHARD A. FEHNEL, JAMES R. HIMES, AND GEORGE SUTIJA. (Syracuse: Syracuse University Press, 1968. Pp. 175. $5.75.)

In his Foreword to Honey's nonbook, Prof. Fred Riggs writes that the volume represents an " . . . important study on the problems Latin American countries face in their efforts to improve administrative capabilities in relation to developmental needs." (p. v) Indeed, from his long association with public administration in Latin America, Professor Honey could perhaps produce the important study Riggs heralds. The present piece is not it. Rather, the reader finds an essay by Honey and four brief country "commentaries" by Ford Foundation staffers.

Honey's essay was prepared for the Ford Foundation as a statement of priorities and targets to which the Foundation might address its resources. His objective is to suggest strategies which will strengthen public administration in Latin America. And in the nearly universal fashion of such essays, Honey argues that strengthening public administration is a prerequisite to development.

The essay itself contains three major elements: a brief review of existing public administration development activities; an exposition of selected variables which affect the operation of administrative systems (civic culture, political socialization, pressures for rapid development, institutional competence, etc.); and a set of "action proposals" intended to remedy the major shortcomings found in contemporary Latin American administrative systems. Honey suggests that individual countries establish national councils on public service to develop comprehensive reform plans; collect data concerning institutional as well as attitudinal processes; improve pre- and post-entry education and training for administrators; obtain expanded technical advisory services in such operations as finance, budgeting, and planning from USAID and related donor agencies; and developed indigenous political leadership through seminars, out-of-country training programs, and scholarships to foreign universities. He also proposes the establishment of a Latin American Center of Public Affairs to coordinate public administration research, training, and practices; and the creation of a Latin American Social Science Research Council to serve not only public administration but broader academic interests.

The strength of Honey's contribution lies in his emphasis on improvements in local Latin American university and professional education as a major (perhaps *the* major) factor in any overall strategy of administrative development. He correctly points out that in Latin America there has been little or no research in the areas of political socialization, civic values, current political affairs and governmental functions, urbanization, and other dimensions of social science. And "as for the schools and institutes of public administration, they are, generally speaking, very modest in terms of financial resources, physical facilities, depth and quality of staff, breadth and variety of curricula, and output of research." (p. 45) Thus not only is there a general absence of empirical studies of Latin societies, economies, and polities, but there are educational and training defects that prevent bureaucrats from effectively overcoming these deficiencies. It is with the remedying educational shortcomings that Honey is principally concerned. To this end he urges greater involvement by United States universities in training North American scholars and conducting research in neglected areas. He argues that USAID should shift from its assumption that the Alliance for Progress is basically a governmental responsibility to the recognition and support of a basic role for US universities.

The essay's weakness, and it is an important one, rests in the failure to grant proper recognition to the importance of domestic politics as a factor in any strategy of administrative development. While Honey recognizes political leadership as a factor, he treats it as something that can be manipulated or shaped to conform to his strategies: "It is possible to suggest some of the steps that can be taken to aid the development of a better understanding of competent administrative performance on the part of a variety of contemporary political leaders. It is also necessary to think about future political leadership and the ways in which it can be prepared to assume responsibility." (p. 69) Having conducted research on the influence of Latin American political leaders on administrative behavior, I cannot share Honey's faith in the likelihood of shaping the attitudes of indigenous leaders to fit a gringo's notions of proper administrative practices.

But of greater significance, Honey's strategies ignore the Latin American bureaucracies as part of a political milieu in which they are both actor and acted upon. In Latin America the national bureaucracies act as primary regime support mechanisms: any reform or modification necessarily will have important consequences for national politics. Yet the politics of administrative development is not considered by strategist Honey. This failure to conceptualize bureacracy as an integral dimension of politics reflects the general failure of students of development administration to relate the role of public administration in a theory of politics. In turn, the absence of comprehensive theory has facilitated an ad hoc approach to administrative development which allows scholars and practitioners to concentrate on isolated facets of administration, generally to the exclusion of a comprehensive analysis of the interrelation between and among other variables. (e.g., the political power of modernizers and the initiation of administrative reform.)

The four commentaries which make up half the volume are sketchy and present little useful information about administrative procedures or problems in Brazil, Chile, Peru, or Venezuela. Each discusses the status of empirical social science and the efforts of administration-reform agencies in the individual country. Too selective for the general reader, these commentaries offer only occasional bits of information for the patient specialist. In sum, the volume's overall contribution is commensurate with its modest price and modest size.—JERRY L. WEAVER, *California State College, Long Beach.*

INTERNATIONAL POLITICS, LAW AND ORGANIZATION

Arms Control in International Politics. By David V. Edwards. (New York: Holt, Rinehart and Winston, Inc., 1969. Pp. 200. $7.50 cloth, $4.50 paper.)

As policy scientists, few achievements will matter more than significant progress toward a systematic and feasible design to achieve arms control. As social scientists, few achievements will be more impressive than significant progress toward a theory dealing with determinants of the acquisition and use of coercive capabilities by political actors. Regretfully, this volume does not seem to me to move us appreciably closer to either of the above goals. Why not? Clearly the author was not lacking in emotional or intellectual commitment to his subject or satisfied with previous work. The reasons for the shortfalls, I suggest, lie in characteristics endemic to the customary practice of political science research and education. Until these traits are modified, it seems likely that the output from our work will be much less than the energy invested would warrant.

Professor Edwards sets himself challenging objectives: (1) to improve a situation in which the "most promising opportunities for arms control have often gone unnoticed" (p. v); (2) to "suggest much about the prospects for arms control"; and (3) to "increase our knowledge of our world, particularly the nature of and trends in international politics and their relevance to important policy problems" (p. 1). To achieve these objectives, he discusses the nature of arms control, conditions conducive to it, procedures to achieve and maintain arms control and, finally, arms control futures for the next decade. He devotes almost as much attention to the procedures section (i.e., tactics) as to the three other topics combined. In conclusion, he states that he has given us a "comprehensive demonstration of a method of analysis" (p. 161).

The approach used is that of subjective taxonomy and pre-operational hypothesis statement. The analyst makes a number of analytic distinctions and puts forth statements about relationships and trends. The analytic distinctions are intended to pick up all major possibilities and the relationship and trend statements are neither based in data nor directly comparable to empirical patterns. The reader is left with a series of lists and of assertions which are neither derived from the lists nor bound to a systematic set of observations. The assertions frequently will seem valid, moot, or invalid to one or another reader, e.g., that neither superpower has been able to gain from the Vietnam conflict (p. 146). The ways in which elements in different lists relate to each other in the framework of distinct processes (e.g., escalation, arms reduction) are open to equllay varying judgments.

It seems to me that a fruitful attempt at either systems design (in the sense of engineering progress with arms control) or theory construction will have to proceed very differently.

A powerful engineering attack on arms control as a systems problem will require: first, defining target end states (arms control outcomes); second, stating initial input conditions (current national and international attributes germane to arms control and directions and rates of change in these attributes); third, working out the sets and sequences of actions required to get from the input conditions to the end states. Because the design will deal with a nonderterministic system many of whose parts are beyond the control of the designer, uncertainty must be explicitly recognized and key points for evaluation and modification must be made salient. At whatever time the requirements no longer seem feasible or worth a variety of costs, the target end state formulations will be modified (e.g., from disarmament to arms control). At various points in the analysis, critical alternatives will receive intensive scrutiny (e.g., choice of parties between whom arms control will be sought). Critical interaction effects will be measured and modeled (e.g., between military procurements by country A and country B). The last activity will attempt to resolve the discrepancies between alternative process models and to determine the thresholds for them to work as postulated. (A useful example of a process model is Charles Osgood's GRIT formulation.) Available point and time-series observations will be used to help reduce uncertainty. (A number of important fragments are already available in the conflict research literature and in social science data banks). When needed measurements are not available of the type and quality desired, the designer will seek to have them made. If this is not realistic, he will resort to alternative aids for handling imperfect information (e.g., the family of Bayesian techniques of DELPHI procedures). The strategy outlined to this point is a terribly complex and resource hungry approach. There is little reason to expect tortuous social problems to succumb on the cheap. Arms control is such a problem and the scale and type of effort to design ameliorative strategies should be commensurate with the difficulty and importance of the problem.

A fruitful theory construction strategy will differ markedly from that outlined above; however, I suggest that it will differ to an equal extent from that pursued by Edwards. First, the

theory construction excerise will not be based on the phenomena to be found in a slice of current history. Instead, the investigator will be attracted to an aspect of behavior which has manifest importance across time and across actors. Second, the investigator will not intend direct application. He will seek to develop a predictive group of axioms from which inferences applicable in principle to "real world" situations can be drawn. Before doing so, he will draw and test some parsimonious inferences in highly controlled environments. Given our current and growing ability to conduct full computer simulations, these controlled environments need no longer be those of the classic psychological laboratory. Third, the theorist will engage in rigorous analysis to determine scope conditions, i.e., that subset of possible positions on key variables to which the theory can logically apply. For example, if one wanted to determine the scope conditions of a theory of renunciation or abandonment of coercive capabilities, one may well ask the following question. Does the theory deal only with conditions in which both actors can eliminate the other? Note that conclusions about scope conditions set boundaries on the possible applications of the inferences one can draw from the theory. At this point, *my* reader may groan at the vision of such a long road to theory and from theory to practice. Long it is, but there seems little reason to expect the journey to be shorter for the social sciences than for the natural sciences. The itinerary outlined above grossly resembles that of the natural sciences.

There clearly is at least one other way for us to write about arms control which has merit. I have in mind the tour de force essay marked by compelling insight, historical and disciplinary range, and provocative statement. The volume under review occasionally seems of this third mode even though that does not seem to be to the author's primary intent.

Because three different approaches can be productive in different ways does not imply that a blend of all three in a particular work has cumulative value. Indeed, partial efforts at approaches as different as systems design, theory construction, and humanistic appraisal are likely to cancel or at least heavily dilute each other. If political scientists are to make major contributions to problems of the order of magnitude of arms control, we need to engage in choice and the actions which follow our choice. The choice required involves what "our bag", i.e.. intellectual strategy, is and the actions are the program of personal and student education required to pursue any one of the three approaches noted above with power and finesse.—DAVIS B. BOBROW, *Office of the Director of Defense, Research and Engineering.*

India and World Politics: Krishna Menon's View of the World. By MICHAEL BRECHER. (New York: Frederick A. Praeger, 1968, Pp. 390. $10.00.)

One of the exciting but as yet unchartered areas of study is the personality and perceptions of top level decision makers. Everyone pays lip service to the fact that the images or world perspectives of Presidents, Prime Ministers, Foreign Ministers, and like, are important, indeed perhaps even critical, in the understanding of the foreign policy making process. Yet there are almost no systematic studies primarily devoted to this problem. The book under review here is an attempt to move in this direction. Though not truly systematic and only in a limited sense quantitative, this study moves away from the usual political biography towards the construction of the image of one major decision maker: Krishna Menon. That the book is not entirely successful should not detract from the significant fact that Mr. Brecher has blazed new paths for further investigation.

The book has three main components. The bulk of the study is a transcript of extensive taped interviews that Mr. Brecher conducted with Krishna Menon in November and December of 1964 (later supplemented with a number of additional interviews in May 1965). Although the transcripts were edited by both the author and Krishna Menon, the result remains a highly readable and intriguing dialogue. Brecher poses a wide ranging series of questions to Menon, and Menon, in his characteristic and honest fashion, reflects upon these topics: the concept of nonalignment, Suez, China, Pakistan, Nehru's successor, to name but five of the twenty-one areas covered by Brecher's questions. While, as often occurs when dialogues of this sort are committed to paper, some of the meaning is lost through the loss of the nuances of expression, nevertheless the character of Menon emerges in sharp and distinctive outline. To see how a man of the stature of Menon views his environment and, of even greater consequence, how he arrived at and explains his position of various issues, is fascinating. Once begun, I found the book impossible to put down. This is clearly the principle contribution of the study: an indispensable insight into the mind of a major world leader.

But while intriguing, the dialogue could have been enhanced on two points. Brecher does not explain how he determined what areas he would cover with Menon or how he choose his questions. Why, for example, did he never ask Menon about the personalities of other top level decision makers. Undoubtably there were reasons for the choice of topics, and the meaning and significance of the dialogue would have been enhanced had these

reasons been explicitly put forth. In fact, the rationale for the choice of questions becomes critical in the second part of the book when Brecher presents his analysis of Menon's image, but we will turn to this in a moment.

The other difficulty with the interview is that a considerable amount of historical background is assumed on the part of the reader. While most of Brecher's readers will have a general background on such issues as Korea, the Geneva Conference, Suez, the Congo, etc., it is doubtful whether the typical reader will remember the specific details or the chronology of events of these historical episodes. Unfortunately these are instances in which Menon's reflections become significant only if one knows these details. A short discussion of the historical period being discussed along with a chronology would have been useful. Such brief historical descriptions would also have put Menon's comments into a broader context and made Brecher's subsequent analysis more meaningful.

The second part of the book contains Professor Brecher's analysis of Menon's image. Following a presentation of a model of the foreign policy making process which shows the relevance and significance of a decision maker's image, Brecher attempts to pull together what he takes to be the main themes of Menon's world perspective. Unfortunately, this is not simply a summary of Menon's perceptions and positions. Brecher wishes to go beyond this to show "irrationality" in Menon's positions, "inconsistencies" in his arguments, and "rigidity" in his ideology. Had these concepts been carefully defined the reader might have been willing to at least entertain this rather clinical appraisal. But no definitions are given and the concepts are frequently used in their negative, value-laden sense. Thus the reader begins to feel that Brecher is condemning rather than analyzing. Consider, for example, Brecher's claim that Menon is "irrational" in his views on nuclear weapons. This conclusion is based on Menon's position that the production and/or possession of nuclear weapons by India should not be debated in the Indian Parliament. But, argues Brecher, since so many other states, most notably China, have nuclear weapons, this position does not make sense: "Irrationality is evident . . . in his categorical refusal to debate the issue." But from this reader's point of view Menon explained it quite well:

> "The debating of this issue means that we are showing the world that we are in two minds about it, that if the majority decides or if something happens we are prepared to use the bomb. Moreover, we are proceeding on a fundamental fallacy; just six months ago we signed an agreement, only to say now that we don't adhere to it. Only three or four months ago we made a declaration that under no circumstances would we use it. Who is going to trust us afterwards? We shall have shaken everybody's confidence."

The last section of the book presents a content analysis of the Menon interview, as done by Janice Gross Stein. The purpose of the analysis is to " . . . derive Menon's perception of the environment . . . " To accomplish this end, the interview data are subjected to three stages of analysis: First, the frequency of occurrence of each of seventy-one content categories (e.g. references to the various states in the systems, different symbols, values) is observed and conclusions are drawn from these frequencies about the relative salience of the topics. The second stage involves another content analysis using the concepts of hostility, friendship, change of the status quo, and satisfaction with each occurrence of one of these themes being further scaled for its intensity. Comparisons are then made among the four themes and the intensity levels. Here it is found that "hostility was by far the predominant attitude of Krishna Menon." The third stage of the study involves a factor analysis of the original seventy-one content categories to determine "patterns of association along several basic indicators."

Thus this analysis is essentially a quantitative description of the Menon interview. Where Brecher intuitively felt that Menon made many references to the United States, Stein can quantitatively support this contention. Whether this amounts to a derivation of Menon's perceptions of the environment is perhaps questionable, but the analysis is nevertheless an interesting adjunct to the overall study. There are, however, a number of difficulties in the execution of the analysis. Like so many content analytic studies, this one neglects to report the details of the content analytic procedure. There is no discussion of the operationalization of the "content categories" or the themes of hostility, friendship, etc. Furthermore, the reader is not told whether the analysis was done by hand or computer, and if by hand the reliability of the coding process. These problems are added to by the fact that the author, in a footnote, indicates that a weighting procedure (unexplained) was used which makes it impossible for the reader to check any of the tallies in the tables. Using the intensities of the four affective categories, various indices are derived which the author claims will predict foreign policy behavior especially in terms of the individuals propensity for violence. But the rationale supporting this contention is omitted. Finally, the meaning and purpose of the factor analysis is not clear. Apparently, the correlations, upon which the factor analysis was based, were computed using the frequencies of reference to each of the seventy-one content categories as they were distributed over sixteen interviews which related to foreign policy (this is never explained). The factors then show, *for this specific set of interviews*, the extent to

which the frequency of reference to any one content category co-occurs in the interviews with the frequency of reference to any other content category. Professor Stein would like to conclude that this shows some meaningful relationship between the content categories that load highly on a factor. But the precise meaning of this relationship is not clear. What can one conclude from the fact that in those interviews in which atomic weapons are heavily referred to there are also many references to such values as respect, affection, etc.? And to what extent are these co-occurrences an artifact of the type of issue being discussed rather than, as Professor Stein suggests, a demonstration of how Menon linked these concepts in his own mind?

In short, like most first studies the results presented here are suggestive rather than conclusive.—DINA ZINNES, *Indiana University.*

The Politics of Force. By ORAN R. YOUNG. (Princeton: Princeton University Press, 1968. Pp. 437. $12.50.)

A number of recent studies of international crises have used case materials in order to assess the options and constraints of the superpowers in periods of extreme tension. Oran Young's ambitious new volume builds on the previous works of Thomas Schelling, Fred Ikle and others in an effort to illuminate the role of force in international politics by examining aspects of "bargaining under conditions of international crisis." While the book is appropriately read in the context of Young's earlier study of the behavior of third-parties in international crises, it also offers fruitful speculations for those unfamiliar with *The Intermediaries* (1967).

Since the case materials are organized around "hypotheses" rather than presented sequentially, a conventional portrayal of the four crises chosen is avoided. Young carefully delineates the limited "universe of cases" to which his hypotheses are presumed to apply: crises involving the major powers in systems characterized by "autonomy among the component units", "the presence of weapons of mass destruction", and "at least some superiority of offensive over defensive military technology." Yet, for this reviewer, the book's central value lies in the possible utility of Young's conceptions for other types of crises and other time periods.

Young explains his selection of four crises for detailed analysis by asserting that a "contextual unity" marked international politics in the years 1948–1962, and that therefore the Berlin crisis of 1948–1949. the Taiwan Strait crisis of 1958, the Berlin crisis of 1961, and the Cuban missile crisis of 1962, may be judged as parts of a complex whole. He correctly cites the shifts in the policies of the US and USSR *after* the Cuban crisis, as "common and overlapping interests" began to emerge and as "mutual co-ordination" began to supplant or at least co-exist with tendencies toward "global competition." Although Young is suitably cautious in suggesting the temporal and structural confines within which his hypotheses should be used, it would be interesting to "test" some of them with reference to cases drawn from the later phases of the 1960's, perhaps the period of the Six-Day War in 1967 and the ambiguous Soviet-Chinese border confrontation of 1969. Such additional investigations would be necessary if we are to offer convincing evidence that: "fundamental challenges to the stability of the system still arise only from two sources: direct clashes between the superpowers or escalation of lower levels of conflict into superpower confrontations."

The hypotheses themselves, as might be expected, vary considerably in novelty and scope. Some appear obvious, and not specifically relevant to "crises" as distinguished from other political interactions, for example: "conditions of crisis raise incentives, at any given moment, to utilize actions and diplomatic formulas designed to divide the issues at stake in ways facilitating the retention of freedom of choice in subsequent phases", or "conditions of crisis generate strong incentives for decision-makers to manipulate the perceptions of various parties concerning the critical aspects of the clash." Others, by contrast, are so complex that a small range of cases contributes at best a partial interpretation: "as alternatives to actions that might precipiate violence during a crisis, efforts to structure the fundamental dimensions and specific issues of the clash in order to utilize or create advantageous asymmetries are apt to become highly influential coercive procedures."

Many of Young's findings appear to be unexceptional, for example: "positions generally become harder to relinquish as they become invested with emotional commitments and as they are subjected to greater publicity over time" (p. 263); or "great powers handle crises in such a way as to preserve the basic arena of conflict in an area even while terminating the physical confrontation that has arisen in a specific clash." (p. 204) Nevertheless, the general reaction of scholars of international politics to the book is likely to accord with the author's stated preference: the production of new concepts, questions and hypotheses about forms of coercion short of war. Indeed, for those who would examine crises involving middle-rank or smaller states, as well as the superpowers, Young's study is a well-argued point of departure.—LINDA B. MILLER, *Wellesley College and Harvard University.*

Of Powers and Their Politics: A Critique of Theoretical Approaches. BY ARTHUR LEE BURNS. (Englewood Cliffs: Prentice-Hall, Inc., 1968. Pp. 299. $7.50.)

In this book, which is built upon and incorporates half a dozen of his published essays which have appeared during the past decade, Professor Burns undertakes to critique some of the basic presuppositions of behavioral and quantitative research and theory in international politics. Because the informed criticism necessary for scientific progress in a field has been slow in coming to the scientific study of international politics, and because the author raises issues of profound importance for the activity of creating a science of international politics, the critique should be welcomed. It is to be hoped that the arrival of this book will help raise philosophical controversy in the field from the level of questions of technique, and from the sterile controversies on the relative merits of "method" and "judgment" in political research which have passed for philosophical analysis in the field.

The book deals with many aspects of international political theory, but three themes stand out: the nature of political action and its proper explanation, the relationship between scientific and historical explanation, and the place of evaluation in theorizing about politics. On each of these themes, Burns argues positions which political scientists with positivist and behavioralist beliefs are likely to find misconceived.

In treating the first of these themes, the nature of political action, the author takes the position that "politics is action, not behavior" (p. 171). Unfortunately this statement neither follows from nor introduces any precise analysis of the concepts and arguments that would be involved in defending such a position. It is, rather, used as an axiom upon which to build an argument about the proper form of explanation of political action. The argument is that political behavior, which is passive and which follows from political action, is explicable only if action is explicable. But "action. . . is not numerable." Therefore, "anything worth calling a science of politics must be qualitative." Furthermore, because action is explained by reference to the ends it seeks to achieve, explanations of politics require mention of the particular ends that actors pursue in given historical situations. The explanation of political action, therefore, must not only be qualitative but historical.

This leads to the theme of historical explanation of political phenomena, and its relationship to scientific or general theoretical explanation. Burns argures that general theory is appropriate in the explanation of phenomena occurring in "system-dominant" systems (those characterized by numerous actors none of whom can alone much influence the behavior of the system as a whole), but that explanations pertaining to "subsystem-dominant" systems (in which the number of actors is small and their actions individually important) must refer to the actions of particular actors identified by name. International politics, insofar as it is the politics of the "Powers" is, in Burns' view, subsystem dominant. Hence explanations of power politics are necessarily particularistic—that is, historical.

On the third theme, the importance of evaluation in theorizing about power politics, Burns characterizes as both misconceived and impractical the belief that theory should be value free. At least in subsystem-dominant systems, it is possible to assign responsibility for the consequences of actions. Where responsibility can be assigned, the "critic" of political practice can evaluate the skill with which it is executed, once he has identified the ends that the practice seeks to achieve. This evaluation, the author argues, is simultaneously "objective" and "evaluative" (p. 265). Burns further finds the student of power politics committed to the value of humanizing power politics.

On each of these themes, the author's positions seem reasonable, although perhaps overstated: but the arguments which support these positions are often inadequate. There is surely something to be gained in the explanation of power political and other strategic activity by using the language of action—that is, by referring to the actors' intentions, to the purpose of their activity defined by its rules, and so forth. But it is mistaken to argue, as Burns does, that the explanation of action is necessarily qualitative: game theory provides a familiar example of a quantitative theory of action. Finally, whether one chooses the language of "action" or of "behavior" would seem to depend upon what aspects of a situation one wished to explain.

Burns is at his best in arguing the case for historical explanation and in discussing the limits of general theory, and it is to be hoped that what he says about these matters will contribute to the realization that much of international politics can scarcely be understood outside of its historical context. However, Burns' arguments depend largely on the assumption that there is only one world-political system: it then makes sense to argue that one should name actors and processes rather than create almost empty classes, and to stress the abstractness and irrelevance of theoretical explanations which ignore the historical and particular. But there is much to be explained in power politics where individuals, or international subsystems, or classes of events such as crises or international conferences, rather than the world-

political system, are the unit of analysis: and here, it would seem, there are fewer obstacles to generalization and to scientific explanation.

On the theme of evaluation in political theory, the author quite rightly stresses the inescapability of judgments of value in building empirical theory, but fails to give a clear and sound argument for this position; the arguments which are given seem muddled. Here the proper direction would be to demonstrate the need to analyze how important concepts of empirical political theory, such as "modernization", "pluralist", or "international stability" are used to commend, to condemn, or to ascribe responsibility, as well as to describe, and are thus in varying degrees evaluative.

Burns offers a much-needed critique of scientific theorizing in international politics. Unfortunately, the force of the critique is undermined by the weakness of Burns' arguments and by his style. This is a badly written book: too often vague, disorganized, hard to read, and difficult to understand. Those who would engage in the scientific study of international politics must seriously consider the unfashionable positions that Burns argues, but they are not likely to do so until a more adequate treatment of these positions appears.—TERRY NARDIN, *State University of New York at Buffalo.*

Contending Approaches to International Politics. EDITED BY KLAUS KNORR AND JAMES N. ROSENAU. (Princeton: Princeton University Press, 1969. Pp. 297. $7.50.)

Reviewers of collections of essays on the state of a discipline seem often to find the experience depressing. The depression I felt upon reading this collection is heightened considerably by my inability to avoid discussing this same topic in my review rather than devote all my public attention to the agreement and disagreement, the strengths and weaknesses, of the 12 essays which constitute this volume.

The volume itself arose as a somewhat staggered—and staggering—reaction to Hedley Bull's much noted article on "International Theory: The Case for a Classical Approach" in *World Politics*, April 1966. It includes responses by Morton Kaplan and David Vital which appeared subsequently in that journal; pieces by Michael Haas, Robert Jervis, and Robert North which appeared in approximate form in the *International Studies Quarterly* for December 1967; a review of Raymond Aron's *Peace and War* by the prolific Oran Young, also from *World Politics*; an essay by David Singer written specially for this volume but, as the author grants, essentially a restatement of an argument he has made various other places, addressed in this instance to Bull's arguments; a general essay on social science by Johan Galtung; and specially prepared essays by Marion Levy and Richard Brody which merit further attention.

If this table of contents engenders a feeling of deja vu, well it might and well it should, for the fights and debates of the essays are both old and tired, and as most of the authors agree, it is time we got on with it.

The trouble is, there is not yet agreement about just what getting on with it would involve. Many of the authors, like their alter egos and subalterns in our academic world, are generally concerned to show, or at least to claim, that there are significant differences between most self-proclaimed traditionalists and most self-proclaimed modernists (or behavioralists, as they are often but somewhat misleadingly termed)—and well there are. The image of the emperor (consider his clothing) has recently become popular; and while the term implies a grandeur and a significance generally lacking (most of us are more like tribunes), the analogy is a propos. The trouble is, perhaps, not just that too many of us have become industrious tailors (to borrow Oran Young's appellation for Bruce Russett), but even more that not enough of us have seen and reconciled ourselves to (if not appreciated) the Living Theatre. My point is that nudity, if not becoming, may well be functional, precisely because it reminds us of the gap between precept and practice *and* often demonstrates its insignificance to all but our conventional-attitude-ridden minds and images.

If we really knew what we were doing, or wished to do, we would not need to spend so much attention on method and rationale and their justification and purity, for these questions would be resolved by our success and failure in doing it. But only, we should realize, if we do have accepted and justifiable standards of verification or confirmation. It is lamentable that scholars in international relations have not addressed themselves to this problem—and indicative of the extremely primitive state of our deliberations on methodology that most have not realized the crucial importance of this. It seems clear to many of the contributors to this guttenbergian symposium that traditionalists and modernists need not work at cross purposes, if even they cannot work at shared purposes. The trouble is that the possibility of progress even within one of the approaches, let alone that of mutual contributions, depends upon each side knowing just what it is desirable to attempt to learn and knowing how and when to conclude that something has been learned, if even just until further research proves

it too simple or misguided or incomplete.

All the old allegations—that available method determines inquiry and tends to force trivial investigations; that complexity of subject matter forces either simplemindedness or development of conceptual frameworks rather than actual efforts to venture beyond frameworks to explicit, testable and disconfirmable propositions; etc.—all these criticisms are of course substantially true of virtually all our contemporary work. And if they are not so true of the work of the traditionalists this seems to us to be because traditionalists are so unaware of these problems that they conduct themselves as Bull recommends, in a way that denies either the possibility or the practicability of intersubjective knowledge.

The quickest confirmation of this contention lies in the challenge offered often by sociologist Levy in his essay. Following an impressive statement of "the rules of the scientific game" and a catalog of fallacious employments of them (arising originally out of his chastisement of one of our foremost scientists, Morton Kaplan), Levy challenges anyone to cite a work which is "playing this game"—which meets the minimal canons of scientific enterprise. He wishes he could, and so do I. He and others recognize that this may be unlikely at this stage of our endeavor. But the ominous implication of this collection of essays, and of the works by these and other modernist scholars with their obvious limitations, is that we do not seem to be progressing satisfactorily toward such a capability.

The point is not that we need eternal truth. No one believes that possible any longer. But most scholars still seem tied to the desire to produce something of semi-eternality. Thus we rarely if ever get efforts to explain why wars occur in the nuclear age and why these wars vary in their form, but rather standardized lists of states which have engaged in or might engage in wars. Etc. What work we get on war tends to be still of the traditionalist sort typified by Aron and subject to all the difficulties indicated by Young in his review.

It was remarkable that Quincy Wright was able to produce his monumental *Study of War* in 1942. But it is even more remarkable that it could be reprinted unchanged 25 years later—and not for the same reasons that we reprint Harvey's treatise on the circulation of the blood. We know that most of the work appearing in our major journals these days will not be reprinted in the future; but unfortunately this will not be because it has been transcended so often as it will be because it has not addressed substantial parts of the crucial questions of our discipline, whether those questions be put in terms of the causes of events or of the motivations of actors. And perhaps worst of all, in this age of drastic change, we have successfully ignored the most crucial question of all—the nature and causes of system transformation. Whatever ways our system survives or changes will not be attributable to, and may not be understandable through, the work of our scholars. Nor will the survival of our discipline be due to the contribution of books like this or even of the "contending approaches" they attempt to memorialize or even institutionalize.

This is not actually much of a criticism of the authors of the volume, for I have clearly been unable myself to escape the trap. If it is an entropic trap, as I believe it is, so much the worse for us all. We can benefit from the discussion in the volume between North and Jervis on the Stanford studies, as we can from the analysis of motives of scientific scholars by Richard Brody. And we may even benefit from, and in any event will be amused and often delighted by, the polemics of others. But how much our discipline benefits is another question. And one that should probably be answered by being honored in the breach through commission of studies written to be transcended and thereby contribute to the cumulation of significant intersubjective knowledge and suspicion.—DAVID V. EDWARDS, *University of Texas.*

The Global Partnership: International Agencies and Economic Development. ED. BY RICHARD N. GARDNER AND MAX F. MILLIKAN. (New York: Frederick A. Praeger, 1968. Pp. 498. $9.50.)

The Winter 1968 issue of *International Organization* contains twenty essays on international agencies and economic development edited by Richard N. Gardner and Max F. Millikan. The issue has been published in book form with no apparent changes. The purpose of the volume is "not to celebrate past accomplishments"—although much space is devoted to doing this—it is rather "to examine, in a critical and realistic way, the potentialities and the limitations of international agencies in the struggle for economic development." Included are essays on individual agencies or groups of agencies, such as ECOSOC, IMF, GATT, the World Bank Group, UNCTAD, OECD, and regional development banks, as well as problem oriented essays on private foreign investment, education, population control, food, and technological development.

How is one to evaluate in one book review a book consisting of twenty articles by nineteen authors? The articles do share the assumptions that economic development is desirable and that international agencies can and should help fur-

ther this goal, but beyond this there is scant evidence of a common intellectual approach. This is hardly surprising, since coordinating the efforts of nineteen authors is a nearly impossible task for any editor.

Uneven quality is to be expected in such a compendium as this. Only a few of the pieces would be likely to meet the editorial standards of *International Organization* if submitted by the authors for anonymous evaluation. The article by Robert W. Cox on "Education for Development" and that by Sherman E. Katz on "The Application of Science and Technology to Development" are especially good examples of thorough and creative scholarship. Several other essays are interesting, provocative, sometimes brilliant, treatments of often neglected topics. Especially appealing to this reviewer were the pieces by Richard N. Gardner on UNCTAD, Stanley D. Metzger on private foreign investment, Frank M. Coffin on the prospects for multilateral assistance, Goran Ohlin on the OECD, and Robert E. Asher on an overview of the role of international agencies in economic development. Many of the articles, however, are rather unexciting combinations of conventional history and conventional wisdom. Editors of scholarly journals should be cautious about inviting articles. The anonymous screening process helps maintain scholarly standards. Let us be wary of endangering these standards in order to enjoy the alleged—but rarely demonstrated—benefits of bringing together a number of articles on the same topic.

In the preface the editors note that they wanted their authors to focus "not only on the economic issues but also on the equally important political and administrative questions involved." In view of this explicit emphasis on the importance of political scientist is among the contributors. Almost all of the authors are economists and/or former government officials. One reads an essay on ECOSOC by one who has been associated with that body since 1947, an essay on UNCTAD by the vice-chairman of the U. S. delegation to the first UNCTAD, an essay on the IMF by the former research director of that organization, and an article on the "Nine Wise Men" of the Alliance for Progress by the former chairman of the group. In a similar context, John H. Kautsky has observed that "it is one thing to be an entomologist and another to be a beetle." Likewise, it is one thing to be an objective observer of international organization and another to be a participant.

It is hardly an exaggeration to describe the political dimension of this book as nonexistent. With the notable exception of Frank M. Coffin's essay, domestic politics is virtually ignored. Neither intra-agency nor inter-agency politics receives much attention. And the bargaining aspects of international aid transactions are often obscured. International agencies tend to be treated as technical problem solvers rather than as political entities. For example, Paul N. Rosenstein-Rodan suggests the creation of a "nonpolitical" international committee "of recognized prestige and expertise—like an International Court of Economic Justice"—which would apply "purely functional criteria" in evaluating the adequacy of national development "efforts" and in recommending aid allocations. (pp. 225–26). Such a committee "would remove any suspicion of political strings and make aid truly a mutual partnership." It is a little difficult to imagine Raul Prebisch, Harry Johnson, Gunnar Myrdal, Milton Friedman, and Paul Baran all agreeing on how to apply "purely functional criteria" in evaluating a nation's aid needs. It is even more difficult to imagine the "nonpolitical aid" administered by "nonpolitical organizations" presented in this book.

All this is not to say that this is not a worthwhile book. A book of little value to one audience may be quite useful to another. Although the specialist will find little to interest him in this volume, the layman, the undergraduate, and the generalist policy maker can all benefit from it. It is not a book to be read from cover to cover, however. It is one to be read selectively when one needs a brief general introduction to a particular aspect of the relationship between economic development and international organizations.

The title, *The Global Partnership*, correctly implies an emphasis on cooperative rather than conflictive aspects. This emphasis obscures many problems likely to interest the political scientist. Perhaps the editors of *International Organization* should publish another symposium on international agencies and economic development entitled "The Global Squabble."—DAVID A. BALDWIN, *Dartmouth College.*

Stalin and his Generals: Soviet Military Memoirs of World War II. EDITED BY SEWERYN BIALER. (New York: Pegasus, 1969. Pp. 644, $10.00.)

In the last year or so A.J.P. Taylor and C. P. Snow, following the footsteps of E. H. Carr and Isaac Deutscher, have been loud in their pronouncements on the greatness of Stalin, who, after all, did win the greatest clash of arms in history. Careful study of the campaign on the Eastern front, however, makes it abundantly clear that the victory was won rather by the superior Soviet economic and population resources and the tremendous sacrifices of the Russian people. In the final analysis, the victory was won despite, rather than because of, Stalin's military leadership which came close to losing the war.

Professor Bialer's book makes an attempt to shed some light on Stalin as a war-time leader through translations of memoirs of Soviet military officers. Most of the memoirs were written during the Khrushchev years, and some show refreshing candor. The Brezhnev-Kosygin regime has discouraged such tendencies toward objectivity, and the Soviet military historiography, mainly through omission of unpleasant facts, has become once again highly unsatisfactory.

The reader's first impressions of the memoirs are of the total subservience of the Red Army to a dictator who had reshaped it by massive and bloody purges and then replaced the professional leadership with incompetent hacks whose deficiencies were known even to him. Combined with this was a disastrous foreign policy whose cornerstone was the trust of Adolph Hitler. It suffices to say about Stalin as a military leader that it took the Soviets, despite superior forces, nearly three years to push back the Germans to the prewar Russo-German border from the Leningrad-Rostov line, which the invaders had reached in less than six months.

Stalin had cowed the Red Army to an extent that he never had to worry about a military conspiracy and in the process destroyed the command initiative so essential to the conduct of military operations. He rewarded military excellence, but dealt harshly with those who had failed or could serve as convenient scapegoats for his own mistakes. In defeat Stalin tended to interfere overmuch in military affairs and even to show signs of panic, whereas in victory he gave his commanders freer hands. He was frequently torn between moods of resolution and vacillation; he had a total disregard for human losses and put political factors ahead of military considerations; he relied on intuition and was often wrong. The best example of this was the complete failure of Stalin and Soviet Intelligence to assess the German intentions during the summer of 1942. It is quite popular today in West Germany to claim that Stalin through Soviet espionage was aware of German military decisions; yet as Marshal Vasilevskii's memoirs show, the Soviets were totally ignorant of German plans in the summer of 1942, and Stalin's wrong guess turned out to be disastrous.

The book, although of value to historians and students of strategy and foreign policy, suffers from some serious defects. Some of its selections have already been translated by the Russians themselves or have appeared in the JPRS "Soviet Military Translation" Series. The title is rather misleading because many of the chapters have little to do with Stalin; the book may even more appropriately be called "Stalin's Generals." The selections are rather arbitrary; Professor Bialer, for instance, omits Marshal Zhukov's memoirs of the Battle of Kursk (*Voenno Istoricheskii Zhurnal.* 6-9-1967) with its commentary on Stalin, both defending and criticizing him. The author's introduction and commentaries avoid too much generalization and even avoid basing the Soviet post-war military politics on alleged war-time rivalries between different generals—a popular myth of several years ago. The commentaries, however, contain numerous factual errors, indicating that the author is not quite at home in this field. One example is the comments on G. I. Kulik in which non-existent units (8th Assault Army, p. 633) and battles (the Kharkov disaster of 1943, p. 575) are mentioned. Professor Bialer to the contrary, Rokossovskii was not the only man to voice opposition to Stalin's military ideas: two less significant officers, Generals Lopatin and Vol'skii, did the same during the Battle of Stalingrad. Also, the Soviet military advisors to Spain had a fairly high survival rate during the purges, and A. I. Rodimtsev is certainly not the same man as P. A. Rotmistrov. (p. 644). An index, a must for a book of this nature, is also lacking.—MICHAEL PARRISH, *Indiana University.*

The Broken Triangle: Peking, Djakarta, and the PKI. By SHELDON W. SIMON. (Baltimore: The Johns Hopkins Press, 1969. Pp. x, 210, $6.95.)

The intent of "The Broken Triangle" is of interest: for the period of the 1960's, to cast light on Indonesian and Chinese "national perceptions" of one another, and on the influence of each party's policies, statements and actions on the other's. The author's desire for illumination is but dimly fulfilled.

Three fundamental defects marr this study: a lack of clarity as to the meaning of "national perception;" a severely limited information base; and personal prejudices.

The meaning of "national perception" remains unclarified. Does it mean the perception of the average citizen, of the government, or of the major political forces within a state? And how does one ascertain the *real* perceptions of any individual political group or government in the circumstances of a dictatorial or totalitarian regime? How can one separate *real* from politically calculated, publicly stated perceptions? Did Sukarno *really* believe his republic to be encircled and threatened by neo-colonialism-emperialism?

The nature of Professor Sheldon's sources prevents him from approaching with authority either of the two basic subjects embraced by his study. His sources are very largely translations from the Indonesian and Chinese press and radio. In and of themselves they are inadequate for an analysis of national perception, however defined. More seriously, by themselves they provide little, and often misleading, insight into the domestic

and broader international situations from which political actions and perceptions emanate. And Professor Simon exhibits little knowledge of his subject other than that provided by translation clippings. Thus he falls into innumerable errors of supposed causal relationships. Examples are legion.

The decision of the Indonesian government against a full-scale assault on West Irian "may well" have been caused by China's refusal to undertake a diversionary attack on Taiwan, an attack that Sukarno "may have encouraged" during his June 1961 visit to Peking (p. 36). Exclamation! A hypothesis founded on two unsubstantiated "mays" and ignoring completely the complex constellation of domestic forces and the other international factors which in fact produced that decision. A second example, supported by no references: the Japanese Communist Party and the Korean Workers' Party "decided to pursue a more independent course of action when they perceived that the PKI received no aid from Peking during its time of need" after the October 1, 1965 coup attempt (p. 169).

Or take Chapters Four and Five on NEFO and the PKI. The author's sources as usual prove incapable of providing any enlightenment on the basic questions that arise from that period of official Sino-Indonesian amity. What *was* Chinese influence, if any, on Sukarno: did it persuade or dissuade with regard to heightened confrontation, increased anti-Western vituperation, a pro-PKI domestic stance, or a policy of radical societal change? What *was* Chinese influence, if any, on the PKI, on the PKI's domestic and foreign policies? What *was* Chinese influence on the politically diverse Indonesian officer corps, on the Moslems, the PNI and the other political forces? And the obverse: in what way, if any, did the actions and statements of the Indonesian government, the PKI and other Indonesians affect the words, actions and perceptions of the Chinese government or elements within the CCP?

From his choice of language, Professor Simon manifests his dislike of Sukarno, the PKI and the power holders in Peking. Such a dislike may, of course, be based on an informed value judgment. It emerges as prejudice, however, when he moves beyond his limited knowledge in order to incriminate, villify or scron the enemy. Some examples from the discussion of the period around the October 1, 1965 coup attempt: when the PKI demanded arms in December 1964, "It is likely that Peking was consulted..." (p. 104); "much circumstantial evidence... pointed to a Chinese role in the coup attempt" (p. 152); the PKI "had become intimately associated with both the CPR and the Overseas Chinese" (p. 124); shortly after October 1, "Suharto and Nasution were creating a new government... faced with violent insurrection..." (p. 112); "Waves of terror spread..., focussing particularly on the Overseas Chinese, who had become tied in the popular image with the assassination of the generals" (p. 124).

Professor Simon makes much of Peking's "effort" to "hide" the death of Aidit, the PKI chairman, although, as the author seems not to know, Aidit was secretly murdered by the army and reasonably certain confirmation of his death was months in emerging. A great deal is made, too, of the Chinese publication and discussion of the "supposed," "alleged" PKI Politbureau Self Criticism issued on August 17, 1966—a document described as "Peking-directed PKI self-flagellation" (p. 167). In fact, the Self Criticism was the work of Sudisman, a survivor of the pre-coup Politbureau, and was so acknowledged by him at his trial in July 1967.

"The Broken Triangle" at times attempts to display a theoretical bent. This is sometimes harmless. Where it is applied to Indonesia, the results can be appalling, most glaringly when a simple mathematical equation purports to provide "considerable insight" into why the Communists did not resist the massive butchery that followed October 1 (pp. 115–6). This is an unhappy combination of pretentiousness and ignorance.

It should be of no surprise that Professor Simon concludes from the present study that "China is still plagued by xenophobia and a national superiority complex, both of which distort its view of the intentions of others and help to promote grandiose plans." (p. 202). There is no bibliography.—DONALD HINDLEY, *Brandeis University.*

The Challenge of World Politics in South and Southeast Asia. By WERNER LEVI. (Englewood Cliffs: Prentice Hall, 1968. Pp. 186. $4.95.)

This essay informs Americans that most nations of South and Southeast Asia are not yet very successful in developing collective regional solidarity, or individual national solidarity. It suggests to the leaders of South and Southeast Asia that they should concentrate on developing national solidarity and individual relations with major outside powers (especially the United States), even if this lessens regional solidarity.

Many Americans, with their noses pointed at the Vietnam war, may not have noticed the intra-regional rivalries growing in South and Southeast Asia during recent years. They may never have been aware of the enormity of the rift between the elitist leadership which controls foreign policy and the peasant masses who are largely divorced from nationalism and fragmented by racial and religious distrusts. I recommend that

those who wish to survey these trends read Chapters Two, Three, and Seven. Do not skip the footnotes, which spice the generalizations in the text with statistics from behavioral research.

National leaders in South and Southeast Asia will find in Chapters Three-Five an insightful analysis of the reasons for the breakdown of the Third World solidarity which presumably characterized the first Bandung Conference.

Professor Levi discusses the rejection of colonialism, power politics, and economic subservience which made non-alignment attractive. Then—dotting his commentary with quotes from statesmen, conferences, and foreign policy documents of the region—he analyzes how the struggle to establish nationalism eroded both non-alignment and regionalism. "If there was a threat to statehood, it came from among the new states themselves, not from either of the two major 'camps' which had originally given non-alignment its meaning." In the mid-1950's the United States and Russia began to realize that non-alignment was not a danger to them, and dropped their resistance to it. China's increasing belligerency speeded the move to open ties with the Western powers. He approves the latter move. "The Asian reaction to the United States and China reflected the victory of realism (favoring the United States) over emotions (favoring China.)"

But the essay scoffs at the Communist's argument that the independence of the states in the region is not real, due to economic subservience. No mention is made of the West's exceptional power over industry, communications, mining, plantations, and finance in the region, or of how little good these resources have done the peasant, or of the refusal of Western nations to help redress these imbalances in any major way such as through the rejected S.U.N.F.E.D. proposal.

The author does indicate that these "weaker nations" enter the arena of power politics with no weapon other than their "votes, sympathy or antipathy, respect or disrespect, moral support or its denial" to use against the great powers. He says, cryptically, in the last sentence of the book that these nations should accept "the fact that the international fate of their states" is "largely determined by outside nations," and that they should "act accordingly."

These nations are presumably trading their sympathy and moral support in exchange for the "minimum rather than maximum" American containment of China, assistance in economic development, and security against their neighbors within the region and their political enemies at home. But there is no discussion of what the eventual costs of that containment might be to the independence of the region, or defense of the author's implied assumption that the U.S. presence in Vietnam has contributed to the independence of the region. Nor is there discussion of how these nations might insure that U.S. economic assistance will not increase the economic domination of the West over their economies. There is no discussion of why they might hope the United States will not side with their neighbors rather than themselves if intra-regional wars occur, or why a national leader should hope that American intervention or assistance against internal insurgents will strengthen his hand instead of the insurgent's. What stake has the peasant in the present political setups that would be increased by making him more nationally conscious?

One would expect such commentary in a book which urges new nations to embrace nationalism as a means to "effectively contribute to the shape of their own future."

The essay repeats itself, and is too long. The last two chapters do not bring the book to any focus. The principal thesis is not clear. Is it the notion that these national leaders should support the U.S. only when it is in their national interest to do so? Or is it the notion that it is always in their national interest to support the U.S.? Or that they have no choice but to support the U.S.? Or just that they should continue to support our effort in Vietnam?

One has the impression that the author has not had any critical thoughts about United States Asian policy. He simply ignores the possibility that these nations—and the United States—might achieve more independence and social progress if America were to cut back on supplying military aid to regimes in the region, or decrease her industrial investments there, or get out of Vietnam. These are alternatives which have frequently occurred to Asian leaders. Asian leaders may begin to wonder why American foreign policy experts so seldom discuss them.—ROBERT E. GAMER, *University of Missouri at Kansas City.*

The Psychological Dimension of Foreign Policy. BY JOSEPH H. DE RIVERA. (Columbus: Charles E. Merrill Publishing Co., 1968. Pp. 441. $7.95.)

It is now commonplace to assume that personality almost inevitably has some effect on an individual's behavior. And currently many political scientists are giving increased attention to the psychological dimension in order to better understand political behavior. Careful empirical study of the relationships between personality and politics is a rather recent development, however, dependent largely upon advances in the discipline of psychology. This book, by a professor of psychology, makes a useful contribution to this rapidly growing approach to political analysis.

Professor de Rivera has set himself three ambitious goals: First, and primarily, to apply the

findings of contemporary psychology to the study of the formulation of U.S. foreign policy in order to better understand the decision-making process. Second, to test the adequacy and completeness of psychological knowledge by attempting to apply it in a new setting. And third, to contribute, hopefully, to increasing the effectiveness of American foreign policy by helping decision makers achieve greater self-awareness of the various ways in which psychological factors may affect the process of making policy.

The basic assumption underlying the book is, of course, that the psychological dimension is essential to the understanding of all decision-making. Although the author briefly argues for the validity of this assumption, the book taken as a whole offers the reader a good opportunity to assess the validity of the premise. The specter of reductionism, however, does not lurk in these pages. Professor de Rivera states early that he does not view psychological analysis as a competing, all-encompassing approach to the study of politics, but rather as a necessary complement to other, more traditional approaches to foreign policy making.

The organization of the book is straightforward: the first four substantive chapters survey the empirical—usually experimental—psychological findings concerning first, the general processes by which individuals perceive their environment and make decisions and second, the impact of personality differences upon these and other activities by decision-makers. The next three chapters focus on interpersonal relations, including such small group processes as consensus formation and communication. A final chapter discusses interaction among diplomatic representatives of nation states, and includes a discussion of game theory and studies of the negotiation process. A brief conclusion summarizes the thrust of the psychological perspective on decision-making, notes some questions in need of further research, and suggests some specific procedures which might improve the process—and thus the policies—by which American foreign policy is made. Few of the suggested reforms appear to be novel, but it might be reassuring to those inclined toward more traditional approaches that the proposals involve institutional restructuring rather than psychotherapy.

Such a survey of social psychology may, of course, be gleaned from any of several introductory texts. A contribution of this book is to illustrate many of the basic findings with data on the making of actual foreign policy decisions. Many of the examples are taken from extensive sets of data gathered by other students of foreign policy decisions. The data include materials on how U.S. decision makers entered the Korean War, formulated the Marshall Plan, and developed both the military budget for 1950 and a subsequent alternative proposal.

Reliance upon such data gathered by other scholars helps the author deal with a possible charge of bias since he admittedly writes from a motive of dissatisfaction with current American foreign policy. Obviously, such an caveat is no guarantee of subsequent "objectivity," but the use of data and conclusions presented by others helps to work toward that end. Professor de Rivera exhibits adequate competence with the political data he uses and, in keeping with his professional eclecticism, he has wide familiarity with the pyschological materials. Nonetheless, it is virtually certain that every reader will find himself disagreeing with some statements of fact, theory, and interpretation—psychological or political. The main effort to explicate the making of foreign policy decisions stands, however, regardless of the inadequacy of specific examples or arguments.

This work may be viewed as an elaboration of an important aspect of the basic decision-making approach developed by Richard Snyder and his associates. The decision-making approach has been criticized for its general lack of rigor, including failure to specify how, under what conditions, and to what extent various factors relate to decisional outcomes. These general comments are applicable to this book as well. Although it indicates in considerable detail various ways in which psychological variables might affect decision-making, this is only a first, albeit necessary, step toward developing a theory of the psychology of decision-making. Professor de Rivera has not promised more than he has delivered, however; he notes at the beginning that he has not made highly cohesive presentations of either the foreign policy making process or of social psychology. The primary value of this book, then, lies in its attempt to outline systematically many of the relevant psychological perspectives and to show how they may be applied to actual situations. This work should help subsequent researchers by suggesting problems, variables, materials, and approaches. In short, the book contributes toward the development of a sophisticated understanding of how personality affects political behavior.

It is doubtful if this volume makes more than a passing contribution toward testing the adequacy of current psychological knowledge. When even tightly controlled laboratory experiments are almost always open to question and to alternative explanations, materials drawn from incomplete and difficult-to-research case studies hardly constitute adequate opportunity for testing hypotheses. But it is gratifying and somewhat con-

firmatory to find that generalizing from college undergraduates to key decision-makers is often plausible.

Surely few would object to attempts at improving the substance of American foreign policy. A book such as this might well help impress upon any reader the ubiquity and importance of psychological factors in decision-making. This is a useful general insight, but given a very incomplete science, it is most difficult to deduce situationally relevant statements from general principles.

In summary, this should be a stimulating book to anyone concerned about the process by which U.S. foreign policy is made and/or is interested in the impact of personality upon political behavior. Correspondingly, it should be useful in advanced courses focusing on U.S. foreign policy, and less so for courses dealing generally with personality and politics.—EVAN D. ROGERS, *Washington State University.*

The Soviet Union: A Half Century of Communism. EDITED BY KURT LONDON. (Baltimore: Johns Hopkins Press, 1968. Pp. 493. $12.00, $2.95, paper.)

Fifteen papers comprise the published product of the Sixth International Conference on World Politics which met in West Berlin in September, 1967. Topics range from a panoramic analysis of Communist revolutions, to more specific evaluations of developments in Soviet society, the Party, Marxist-Leninist ideology, military-political relationships, agriculture, industry, and foreign policy.

Posing the possiblity of constructing a typology of comparative revolutionary successes, Robert Tucker reasons persuasively that communist revolutions have not occurred on the model projected by classical Marxism: that is, industrialization was not the cause of revolution, but the result. Thus, with the notable exceptions of Czechoslovakia and the GDR, communist revolution has been a creature of underdevelopment. Insightfully argued distinctions among Soviet, East European, and Chinese experiences form the heart of his thesis that although two world wars were probably indispensable to communist victories, in no case did the thirteen subsequent ruling party states replicate the Bolshevik pattern. He concludes: "Any future communist revolutions will probably occur not in developed industrial countries with advanced social and political institutions but, as in the past, in underdeveloped countries where economic progress is slow or stagnant, where society is divided into a privileged minority and a disadvantaged peasant majority, and where authoritarian government prevails."

Dan Jacobs attempts to show that the Politburo has not been an immutable or exclusive focus of Soviet power throughout fifty years. Under Stalin, "it was only one means of disseminating power, and in many ways and at many times not even the most important means." The Politburo's evolution is traced "from aborted inception, to power, to gimcrack, and back to power, only to find its regained power in the process of erosion." Several tables of personnel statistics are assembled to explain actual and potential decision-making dilemmas.

Frederick C. Barghoorn's paper posits adaptive evolution rather than political decay or revolution as the most likely consequence of trends in Soviet political life—an outcome likely to foster policies conducive to peace and welfare. Employing the macro-analytical schemes of Pye, Almond, Powell, and others, Barghoorn quite imaginatively applies to Soviet development such concepts as crises of identity, legitimacy, participation, distribution, and penetration; system capabilities; conversion functions; and finally, processes of system-maintenance and adaptation based on political socialization and elite recruitment. Chalmers Johnson's approach to revolution is used to predict that, "the Soviet political elite will probably continue to display sufficient adaptiveness and cohesiveness to prevent either a revolutionary breakdown of the USSR or the stagnation and 'petrification' foreseen by some analysts." Evolutionary development is thus initiated and controlled by established political authorities and is compatible with established institutions and practices. The result will be progress toward a knowledgeable society; but there is also a danger that the vested interests of elite incumbents may increasingly serve as barriers to the completion of modernization.

Focusing upon communist politics as a "ceaseless flux of change beneath an appearance of changeless continuity in the forms, structure, and . . . theory of political power," H. Gordan Skilling offers an analysis of interest groups and a fourfold typology of opposition. The thesis is familiar and has received considerable deserved attention in recent years: policy-making in communist states takes place increasingly within a context of sharp group conflict; and oppositional tendencies have assumed more vigorous and varied forms. More than this, "the Party increasingly performs the role of an aggregator of conflicting interests, rather than of the exclusive articulator of its own conception of the national interests."

Boris Meissner probes the social composition of the CPSU and traces the evolution of bureaucracy, elites and the intelligentsia through the October Revolution and industrialization. He agrees somewhat reservedly that interest groups, frictions, and struggles contribute to gradual so-

cial transformation. However, "it would be a mistake . . . to assume that this development indicates that totalitarianism of the Soviet communist type is already coming to an end." (Competing with this, of course, are the revised views of Friedrich and Brzezinski, two chief proponents of the original totalitarian model so fashionable in the 50's.)

Continuing the theme on Soviet society, Robert A. Feldmesser examines the function of ideology in social—especially occupational—stratification. Probing the intriguing dilemma of distribution "according to needs" under communism, he cites Khrushchev's evasive clarification that this really means "the healthy needs of a culturally developed person." To Feldmesser, the goal is not unique to the Soviet Union, for if there is a dominant economic concern among societies, it is apt to lie in distribution rather than simply more production.

Roman Kolkowicz refers to the historical agreement among Marx, Engels, and Lenin that, "in a communist society no one will even think about a *standing army*. Why would one need it?" The reason is that communist parties which in principle condemn professional armies as an evil force of suppression cannot do without them after achieving political power.

The resultant elaborate military policy is the topic of Thomas Wolfe's paper. Resource allocation, the possibility of general war, the build-up of strategic forces, the ABM issue, preparations for conventional and limited wars, Vietnam, and the Warsaw Pact are reviewed. According to Wolfe, Khrushchev had to confront the paradox that, even as Soviet technology invested military power with unprecedented potential destructiveness, constraints upon its use have grown, tending to multiply the risks and narrow the opportunities for turning military power into political advantage. (Wolfe would, however, have to agree that the Soviet Union's punitive intervention in Czechoslovakia was remarkably successful precisely because the Soviets managed to translate overwhelming military force into decisive political power.)

Turning to foreign affairs, Kurt London relates the Soviet Union's political schizophrenia resulting from inevitable contradictions between the roles of nation-state and revolutionary power. Central to his unequivocally traditional view is the key statement: "There can be no question that his thesis [Lenin's *Imperialism*] first published in 1916, has directly or indirectly dominated or influenced the thinking of communist foreign-policy makers ever since. It is obvious that the belief in these principles forecloses a pursuit of international relations in a fashion conconsidered 'normal' among Western States." The result—persistent dualism, documentable at least from Brest-Litovsk to Khrushchev's elevation of the peaceful coexistence doctrine from a tactic to a strategy. Even if state security interest "may have been primary during certain periods," he continues, "they remained immersed in doctrinal party-oriented thinking from which no Soviet leader . . . apparently can separate himself." (True enough, perhaps. But far more important would be concrete cases in which doctrine has actually superceded Soviet state interests. A frequently cited—and weathered—example, of course, is Stalin's devastating attitude toward German Social democracy.)

Papers analyzing object areas of foreign policy include one by Andrew Gyordy, who surveys Stalin's "revolution from above"—the takeover of Eastern Europe by employing a variety of nondoctrinaire techniques—as one of several examples of nonconformity between communist theoretical perception and political reality. One controversial conclusion is that "the longer communism survives in Eastern Europe, the more sporadic and atomized will be popular resistance to it." (Would not unprecedented popular solidarity in Czechoslovakia constitute an exception?) John Armstrong illuminates Soviet dilemmas in the Middle East where the interests of local communist parties are opposed to Soviet cooperation with a bourgeois national regime. Understandably, such parties are regarded as expendable if major USSR interests are at stake. German-Soviet relations, historically affected by ten factors, are the subject of Klaus Mehnert's essay. And, finally, Harish Kapur summarizes the shifting preoccupation of Soviet diplomacy from Europe to Asia, where Soviet leaders have perforce softened Leninist formulas and accepted unorthodox varieties of socialist doctrine. More questionable is the unconditional statement about the Korean war: "When one examines the period, one is struck by Soviet complicity in the origination of the war."

Admittedly, this reviewer has a bias. No reconnaissance into a half century of communism can be complete without recognizing that the Soviet Union, notwithstanding all its revolutionary effusions about proletarian unity and internationalism, must finally own up: international communism is at best a vain dream imbedded in a collective historical memory. The confrontation with Mao and the dissidence of all too many others really mean that the context for the realization of international communism, and even the perspective from which it is viewed, are markedly narrower than Marx, Lenin, Stalin, and cohorts could ever envision. Perhaps the most significant thing about fifty years of policy is this: the Soviet inability to realize worldwide communism as con-

sistently projected has not prevented tenacious efforts toward defending the *potential* for international communism in one subsystem—i.e., proximate Eastern Europe—a goal which is at least plausible and maybe attainable, but by no means inevitable.

Although stamped by some unevenness in quality, this book yields amost sixty pages of rich biographical footnotes and a wide range of cultivated viewpoints, making it singularly challenging for college seminars. It is a favorite and intellectually invigorating pastime to do battle with eminent scholars *in absentia*. Certainly this composite volume offers ample targets for reassessing conventional wisdoms. And in doing so, it enhances the necessary dialogue beneficial to us all.—Nish Jamgotch, Jr., *University of North Carolina at Charlotte.*

ANNOUNCEMENTS

RESULTS OF THE MAIL BALLOT ELECTION
FOR OFFICERS OF THE AMERICAN POLITICAL SCIENCE ASSOCIATION

(as certified to the National Office by
the American Arbitration Association
November 10, 1969)

For President-Elect	*Votes*
*Robert E. Lane	5474
Christian Bay	2816
For Vice Presidents	
*Grant McConnell	5622
*William H. Riker	5546
*Robert E. Ward	5185
David Kettler	2548
H. Mark Roelofs	2526
Alan Wolfe	2516
For Secretary	
*Thomas R. Dye	5739
Paul Minkoff	2319
For Treasurer	
*Francis E. Rourke	5513
Robert H. Clarke	2444

For Council (One-Year Term)	*Votes*
*Tobe N. Johnson	6601
For Council (Two-Year Term)	
*Jewel L. Prestage	6179
*Samuel P. Huntington	5669
*Herbert McClosky	5339
*Robert H. Salisbury	5319
*Allan P. Sindler	5102
*John H. Kessel	5008
*Ellis Waldron	4479
*Henry S. Kariel	3866
Lewis Lipsitz	3812
James M. Elden	3039
Theodore L. Becker	2849
William E. Connolly	2687
Philip Greene	2609
Alex Gottfried	2543

*Elected

The sixty-sixth Annual Meeting of the Association will be held September 8–12, 1970, at the Biltmore Hotel, Los Angeles, California.

ARTICLES ACCEPTED FOR FUTURE PUBLICATION*

March, 1970

Barry Ames, Stanford University, "Bases of Support for Mexico's Dominant Party"

Aage R. Clausen and Richard B. Cheney, University of Wisconsin, "A Comparative Analysis of Senate and House Voting on Economic and Welfare Policy, 1953–1964"

Michael Haas, University of Hawaii, "International Subsystems: Stability and Polarity"

Howard D. Hamilton, Bowling Green State University, "Direct Legislation: Some Implications of Open Housing Referenda"

John E. Mueller, University of Rochester, "Presidential Popularity from Truman to Johnson"

Paul F. Power, University of Cincinnati, "On "On the Grounds for Civil Disobedience within American Democratic Thought"

Kenneth Prewitt, University of Chicago, "Political Ambition and Democratic Accountability"

William H. Riker, University of Rochester, "Rational Behavior in Politics"

Patrick Riley, Harvard University, "A Possible Explanation of Rousseau's General Will"

Stanley Rothman, Smith College, "Barrington Moore and the Dialectics of Revolution"

Barrington Moore, Jr., Harvard University, "Reply to Rothman"

Martin Shubik, Yale University, "Voting, or a

* Production exigencies may occasionally necessitate publication of articles in issues other than those given in this listing.

Price System in a Competitive Market Structure"

Duff Spafford, University of Saskatchewan, "The Electoral System of Canada"

June, 1970

Joel D. Aberbach and Jack L. Walker, University of Michigan, "The Meanings of Black Power: A Comparison of White and Black Interpretations of a Political Slogan"

Steven J. Brams, New York University, and Michael O'Leary, Syracuse University, "An Axiomatic Model of Voting Bodies"

Steven R. Brown and John D. Ellithorp, Kent State University, "Emotional Experiences in Political Groups: The Case of the McCarthy Phenomenon"

Walter Dean Burnham and John D. Sprague, Washington University, St. Louis, "Additive and Multiplicative Models of the Voting Universe: The Case of Pennsylvania, 1960–1968"

William R. Caspary, Washington University, St. Louis, "The 'Mood Theory': A Study of Public Opinion and Foreign Policy"

Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, Carnegie-Mellon University, "An Expository Development of a Mathematical Model of Policy Formation in a Democratic Society"

Ada W. Finifter, Michigan State University, "Dimensions of Political Alienation"

Bryan R. Fry and Richard Winters, Stanford University, "The Politics of Redistribution"

M. Kent Jennings, University of Michigan, and L. Harmon Zeigler, University of Oregon, "The Salience of American State Politics"

Paul E. Peterson, University of Chicago, "Forms of Representation: Participation of the Poor in the Community Action Program"

David E. Powell and Paul Shoup, University of Virginia, "The Emergence of Political Science in Communist Countries"

John Duncan Powell, Harvard University, "Peasant Society and Clientelist Politics"

September, 1970

Michael Aiken and Robert R. Alford, University of Wisconsin, "Community Structure and Innovation: The Case of Public Housing"

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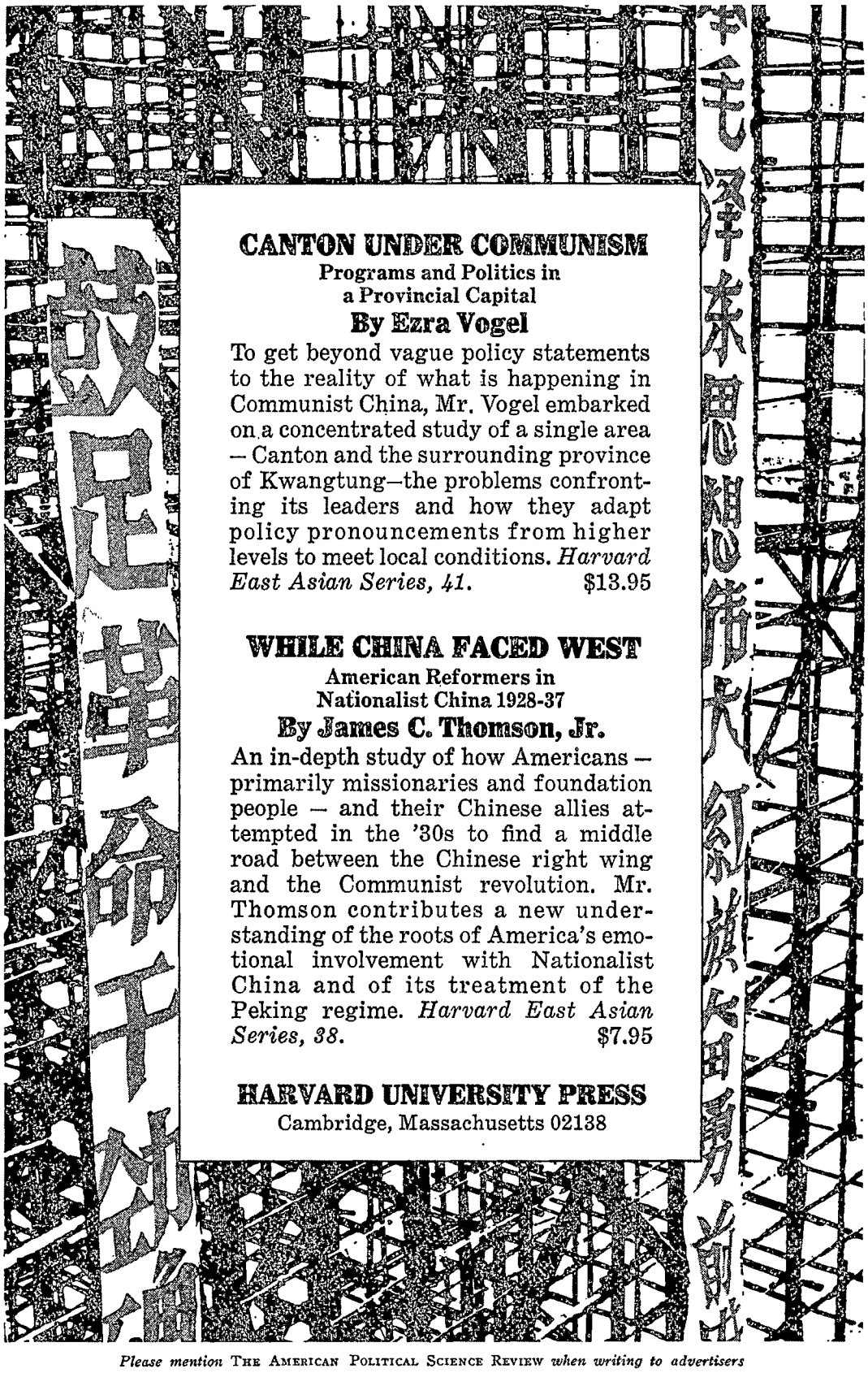
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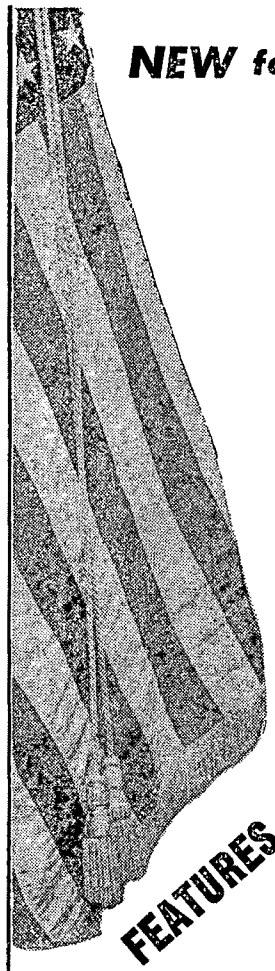
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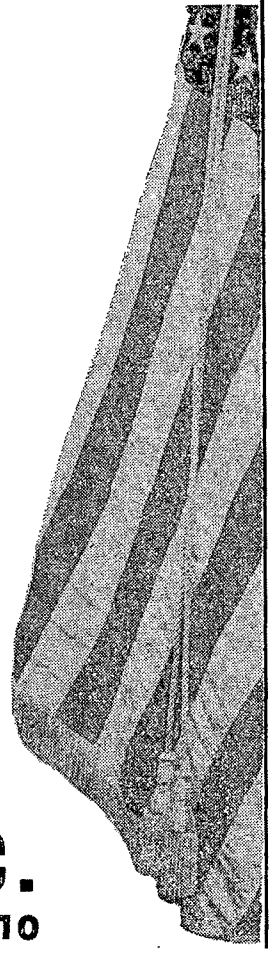
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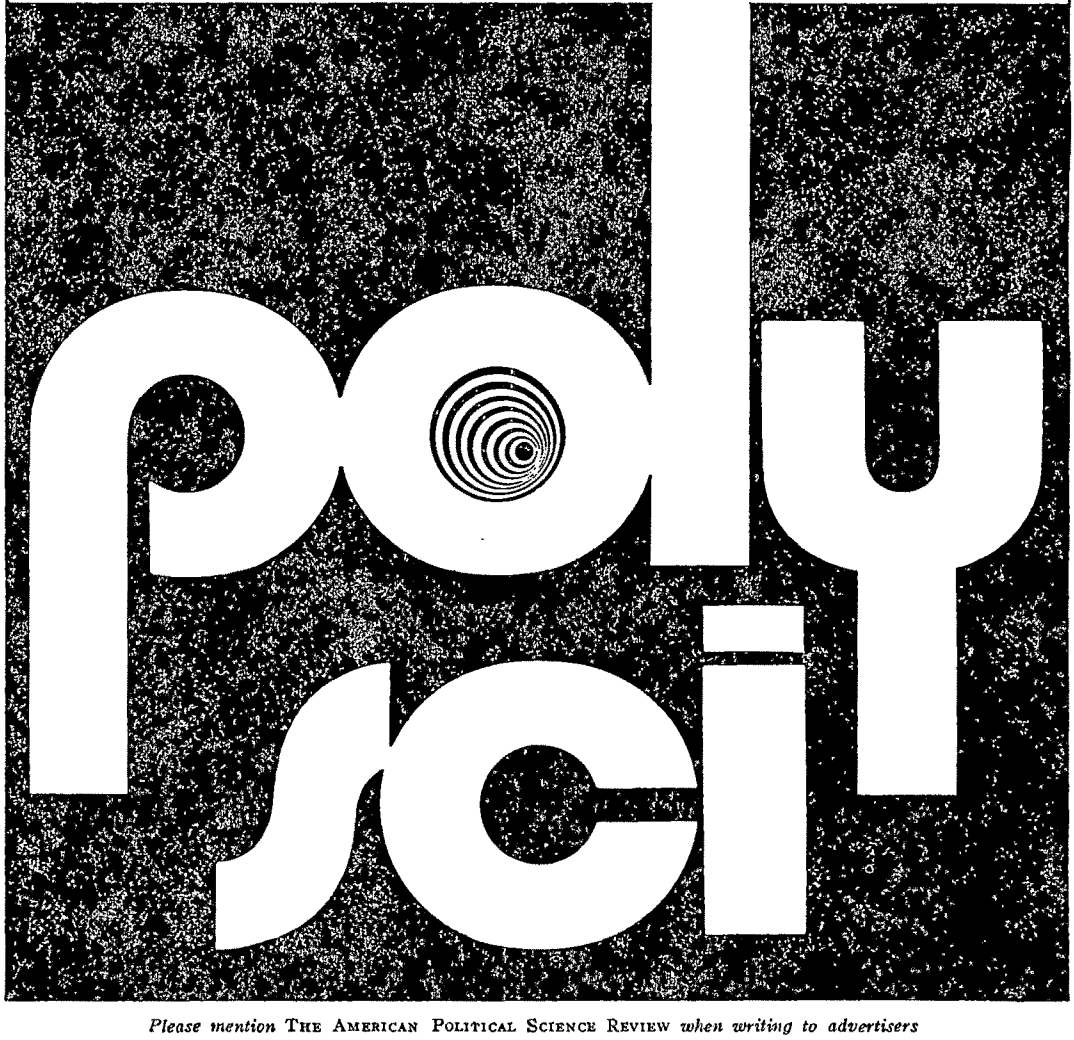
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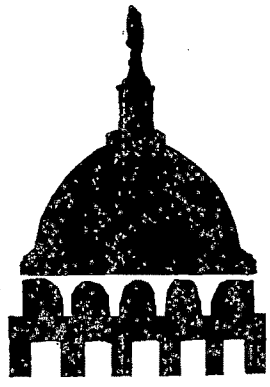
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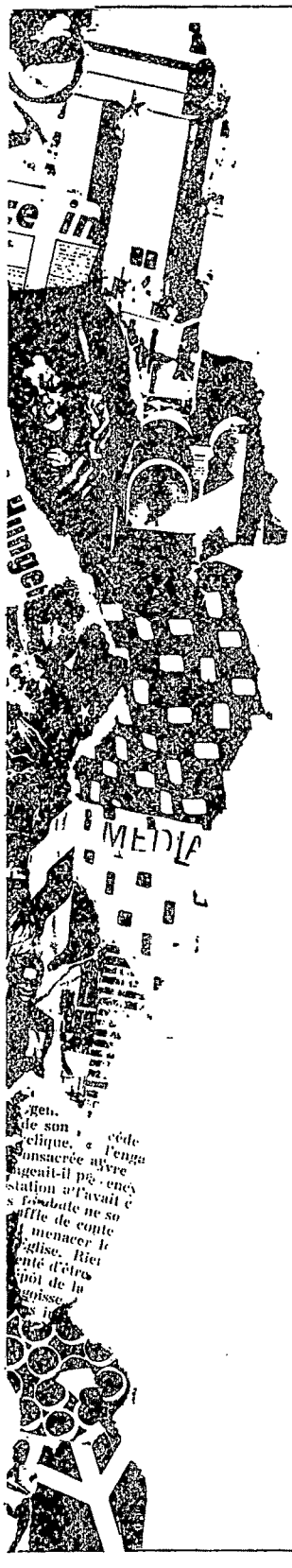
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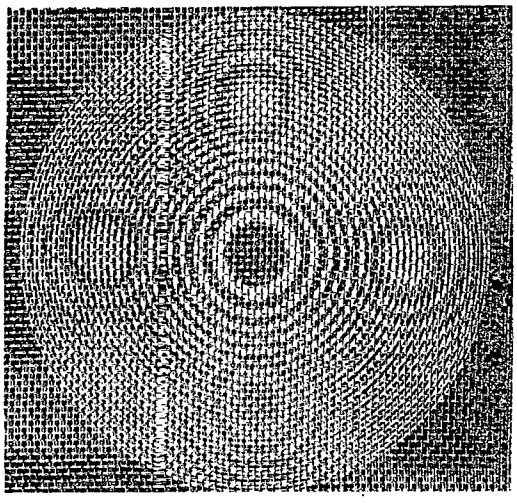
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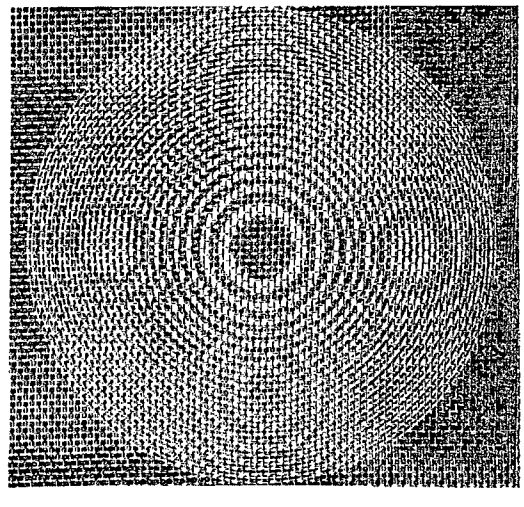
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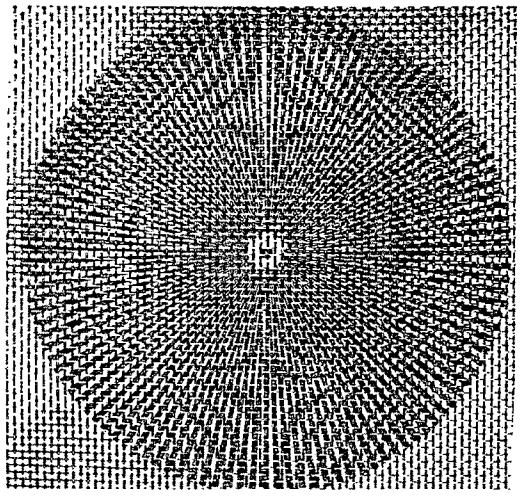
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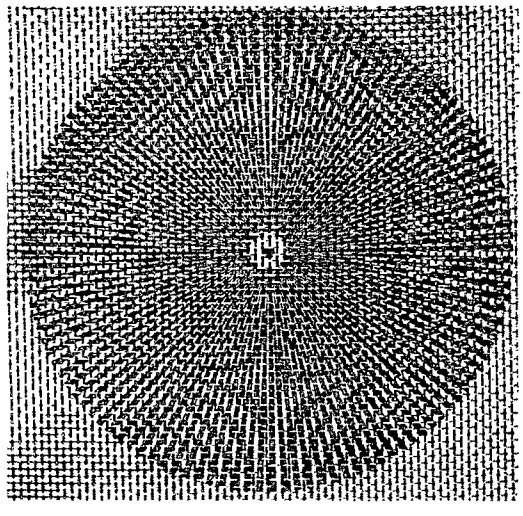
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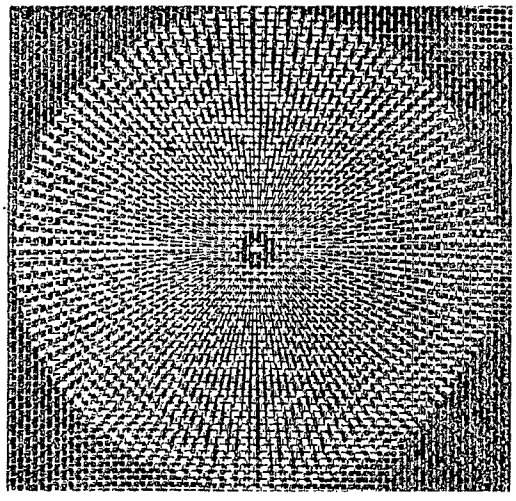
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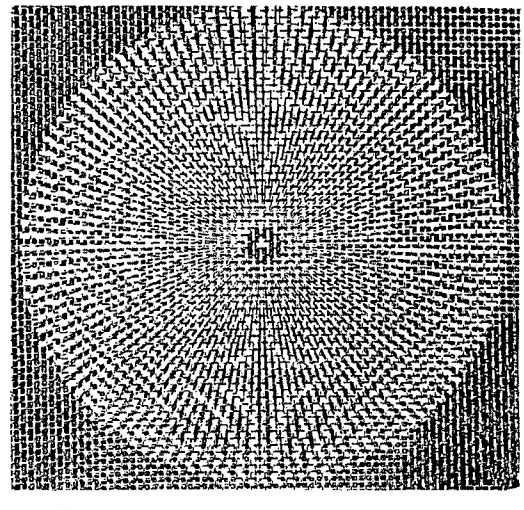
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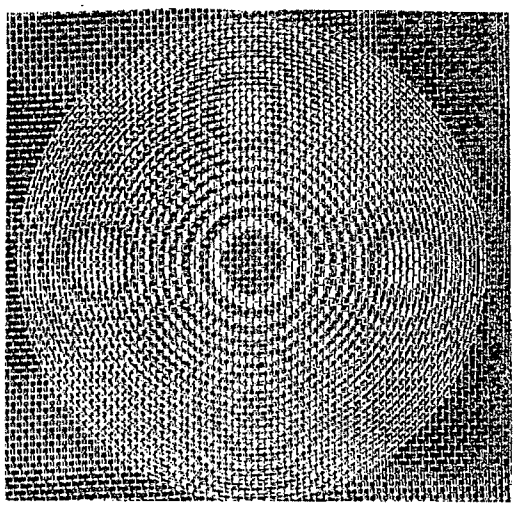
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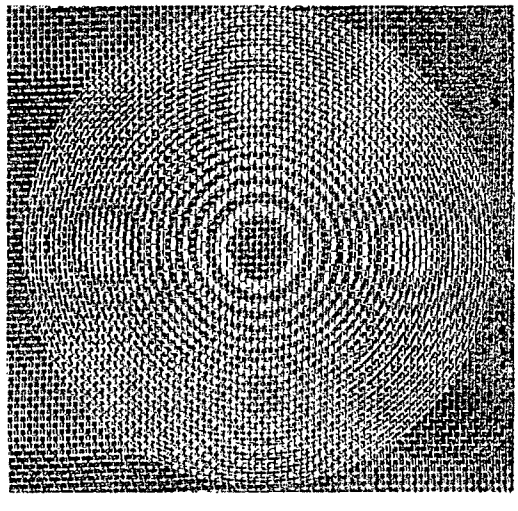
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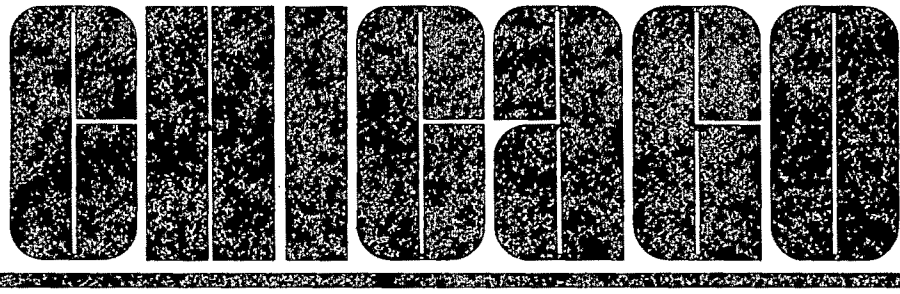
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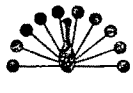
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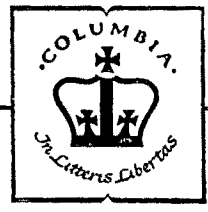
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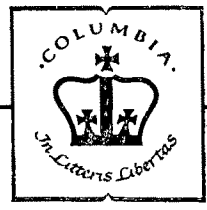
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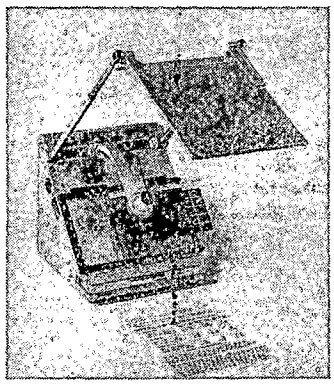
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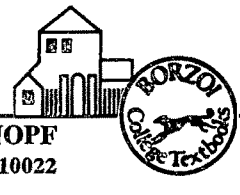
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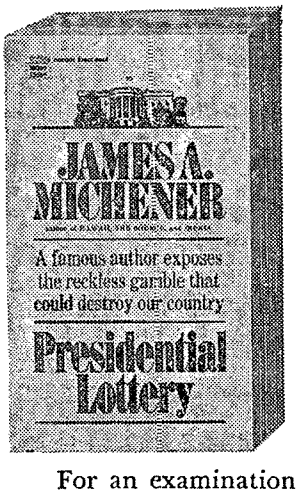
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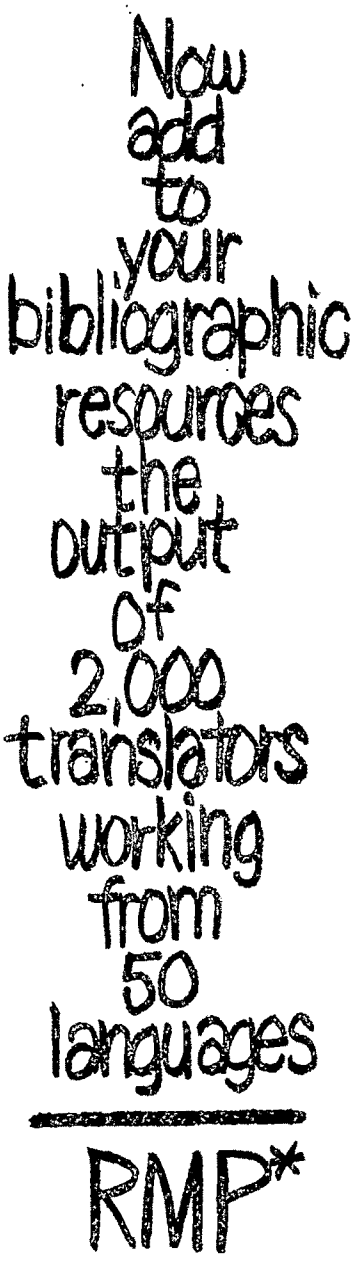
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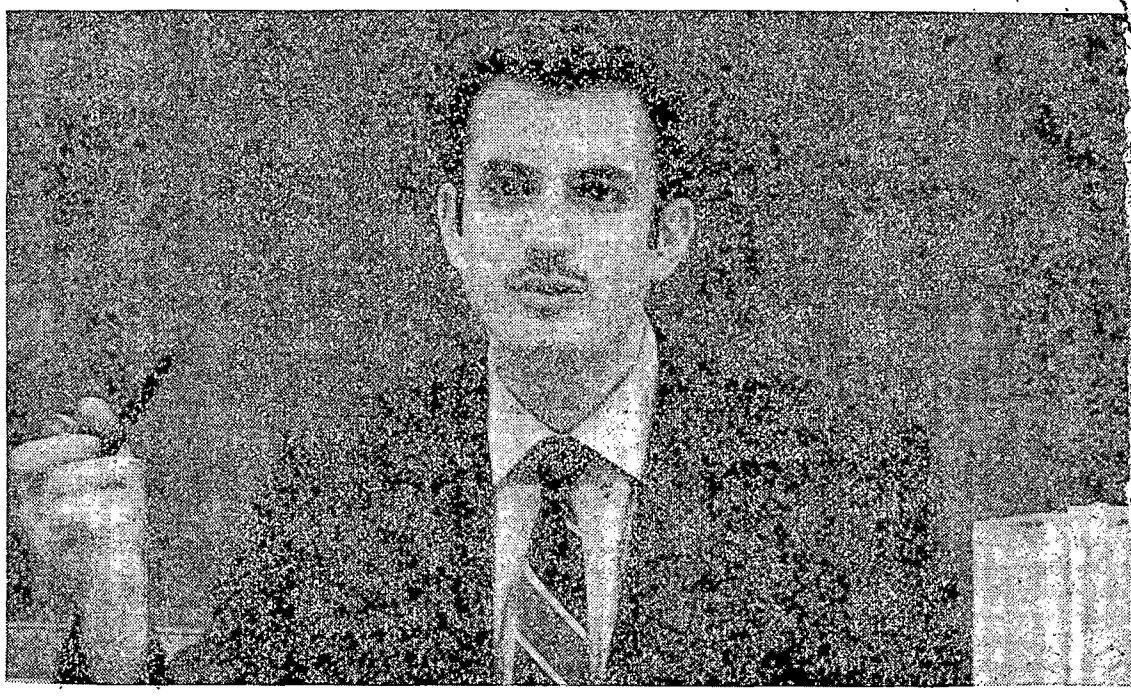


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John Wiley & Sons 1429, 1446, 1447
Winthrop Publishers 1468
Yale University Press 1458, 1459



YOU MEAN I CAN GET $50,000 OF TIAA LIFE INSURANCE FOR LESS THAN $100?

That's what an Assistant Professor asked us when he heard about TIAA's low life insurance costs.

It's true. At his age 30 the annual premium for a 20-Year Home Protection policy providing $50,000 initial amount of insurance is $159.00. The first-year dividend, based on our current dividend scale, is $61.00, making a net payment of $98.00. Dividends, of course, are not guaranteed.

The Home Protection plan is level premium Term insurance providing its largest amount of protection initially, reducing by schedule each year to recognize decreasing insurance needs. This is just one example of the many low-cost TIAA plans available. If you need more protection for your family, ask us to mail you a personal illustration with figures for a policy issued at your age. We'll also send the Life Insurance Guide describing other TIAA policies.

ELIGIBILITY

Eligibility to apply for this or other TIAA life insurance is limited to persons employed at the time of application by a college, university, private school, or other nonprofit educational or scientific institution that qualifies for TIAA eligibility.

TEACHERS INSURANCE AND ANNUITY ASSOCIATION
730 Third Avenue, New York, N. Y. 10017

TIAA

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Please mail the new Life Insurance Guide and a personal illustration.

Name_____ Your Date of Birth_____

Address_____
Street

_____
City State ZIP

Dependents' Ages_____

Nonprofit Employer_____
college, university, or other educational or scientific institution